Multiparty Competition, Founding Elections and Political Business Cycles in Africa

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Abstract
Political business cycle theory and empirics typically assume that elections are competitive. Yet, as empirical work on political business cycles turns increasingly to developing countries for evidence, this assumption becomes untenable. We propose and test two empirical hypotheses regarding political business cycles: first, we should only see cycles when elections involve multiparty competition; second, we should see larger cycles in “founding” elections. Using a new indicator of multiparty competition and macroeconomic data from Africa, we find strong support for both hypotheses. These findings have implications for democratic transitions and the compatibility of economic and political reform in nascent democracies.

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1. Introduction

How do political structures affect the selection of economic policies? This is one of the central questions arising out of recent work on the political economy of development. In the 1990s, its significance was driven home by research and development experiences in Africa. Whether by scholars trying to unpack the “Africa dummy” in growth regressions or by the World Bank trying to understand the often disappointing experience of structural adjustment programs in Africa, the investigations revealed that African governments’ policy choices mattered, and furthermore, we needed to understand the political structures that produced them. Such issues are particularly critical to the extent that politically-motivated economic policies conflict with the objectives of economic reform.

In the case of Africa, many argued that democratic political institutions would provide the political incentive structures needed to induce better policy choices.\(^1\) Democracy – in particular, a multiparty electoral system – was seen as a tool for economic as well as political transformation and reform. Political business cycle theory provides a useful analytical context for exploring one aspect of the question posed at the outset – how have elections in increasingly democratic Africa affected spending policies.\(^2\) Although several scholars have carried out empirical tests of political business cycle theory in developing countries (Brender 1999; Krueger and Turan 1993; Remmer 1993; Schuknecht 1996; Ames 1987; Block 2001, among others), generally they have applied existing theories without regard for possible differences in institutional context that may

\(^1\) See Widner’s 1994 volume, for example.

\(^2\) See Drazen (2000) and Alesina, Roubini, and Cohen (1997) for excellent reviews of both theoretical and empirical work on political business cycles
differentiate newly democratizing countries from the established OECD democracies for which such theories were originally developed. Elections in nascent democracies – such as commonly found in Africa – may lack true multi-party competition or may be voters’ first experience with competitive elections. Do multiparty elections affect economic policy choices and spending decisions differently in such contexts? Are initial multiparty elections – when incumbent authoritarian leaders are less constrained and uncertainty surrounding electoral choice is higher – different from later ones? Answers to these previously ignored empirical questions may help illuminate the connections between political institutions and economic policy. Such illumination is particularly important given the emphasis of late on democratization in developing countries.

With these question in mind, we extend the empirical testing of political business cycle theory in two ways: first, by testing explicitly whether in the absence of multi-party competition political business cycles occur (in other words, whether they occur in non-competitive elections), and second by seeing whether the magnitude of political business cycles varies as a function of whether a given election is the country’s first competitive election (e.g., a founding election). Significant increases in the incidence of elections with multi-party competition (Bratton and van de Walle, 1997) and relatively undeveloped democratic institutions make Sub-Saharan Africa the ideal empirical testing ground for our proposed extensions of political business cycle theory. Indeed, our results strongly confirm not only the existence of political business cycles in Africa, but also the importance of considering explicitly the introduction and effects of multi-party electoral competition in empirical analysis. Indeed, our results confirm that political business
cycles only occur during multi-party elections, not during non-competitive elections; and that founding or first multi-party elections produce greater political business cycles.

The paper is organized as follows. Section 2 briefly reviews the existing theory and the intuition that motivates our tests, along with a brief review of previous empirical analyses. Section 3 describes our data and empirical strategy; Section 4 summarizes our results; and, Section 5 concludes.

2. Theoretical Motivation: Role of Electoral Institutions in Political Business Cycles

The literature on political business cycle theory is well established, yet testing in advanced economies has produced at best mixed evidence for the existence of political business cycles. This is somewhat surprising, given that both “partisan” (Hibbs, 1977; Alesina, 1987) and “opportunistic” (Nordhaus, 1975; Rogoff and Sibert, 1988; Rogoff, 1990) theories of political business cycles were built on the assumption of democratic institutional structures common to industrialized democracies. Paradoxically, it is in the developing world – where such institutions are uncommon – that we find stronger support for the existence of political business cycles. We will return to this puzzling reversal of results, but first let us examine the institutional assumptions of both rational and opportunistic versions of political business cycle theory..

In both sets of theories, incentives and constraints drive the cycles, but the source of those incentives and constraints differ across the theories. For rational partisan theories, the incentive to manipulate the economy derives from the partisan policy preferences of politicians running for office, and the constraint from the degree of electoral surprise. Without electoral surprise, politicians with partisan preferences would be unable to create cycles in economic activity and inflation. Rational opportunistic
cycles, on the other hand, are driven by the incentives provided by electoral uncertainty, and are constrained by the competence of incumbents. For rational opportunistic models, competence serves as a constraining factor because only high competence incumbents can attempt to signal competence through pre-electoral economic manipulation. In the latter models, incentives and constraints to manipulate the economy derive from politicians’ wanting to retain office (implying some prior positive probability that the incumbent could lose her reelection bid) and exploitable informational asymmetries. In a world with no uncertainty, the models predict no cycles.

The institutional basis of this uncertainty, then, is a key parameter in both branches of models. Opportunistic theory in particular has relied on implicit assumptions regarding the competitiveness of electoral institutions. At the same time, the limited empirical testing to date that concentrates on developing countries has been guided by opportunistic political business cycle models. These empirical tests have provided stronger support for political business cycle theory in developing country contexts than in the developed economies for which the theory was intended. It is unsatisfactory, though, to conclude from this greater support simply that some unspecified characteristic of developing countries makes them more vulnerable to politically motivated manipulation of economic policy. What about developing countries seems to make them more vulnerable to such manipulation? Explicit efforts to model and test specific institutional factors that differentiate developing countries have only recently begun.

For example, Gonzalez (1999) adds two parameters to a Rogoff-style rational opportunistic political business cycle model: the cost of removing an incumbent from office, which she bases upon the degree of democracy; and the likelihood that publicly
available information will reveal the competence of the incumbent, which she calls the “transparency of the society.” Her model predicts the strongest cycles at what she labels “mid-levels of democracy.” Along similar lines, Svensson and Shi (2000) propose a moral hazard model of electoral competition, which includes the magnitude of the rents of remaining in office and the share of informed voters among all voters. The size of the policy cycle is increasing in the magnitude of rents and decreasing in the share of informed voters.

We add to this work about the institutional bases of cycles by testing explicitly the relationship between the presence of multiparty competition during elections and the existence of political business cycles in Sub-Saharan Africa. We examine two questions in particular: First, are political business cycles more likely to accompany multiparty versus single party elections? And second, are cycles larger in founding elections (e.g., countries’ first experience of competitive elections)?

Our theoretical justification for concentrating on these questions follows from an intuitive re-examination of rational opportunistic political business cycle theory (for which we take Rogoff (1990) as the archetype. As noted above, uncertainty about the electoral prospects is critical in motivating competent incumbents to attempt to signal their competence to voters through pre-election economic distortions. Logically, however, it follows that in the absence of multi-party electoral competition there is no incentive for incumbents to engage in pre-electoral economic policy distortions as the theory predicts.

In Rogoff (1990), for example, politicians’ utility functions differ from that of other agents only by the inclusion of the “ego rents” that accompany office. However, all
agents’ expected utility is determined by the consumption of private and public consumption goods and by public investment, and all agents including politicians suffer the same disutility from distorted fiscal policy. The possibility of ego rents alone thus motivates competent incumbents to signal their competence by “over-spending” on public consumption goods at the expense of public investment during election years.

Rogoff assumes an institutional structure in which the incumbent faces a non-zero probability of losing: in other words, a competitive electoral system (which we take as one in which multiple parties compete during the electoral process).³ If this condition does not hold, then, the model’s own logic suggests that a competent incumbent will have no reason to incur the disutility associated with fiscal policy distortions.⁴

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³ We take multiparty competition as a necessary but probably not sufficient condition for the threat of removal to feel real to incumbents. Other factors most likely matter as well: freedom of the press, the ability of the opposition to campaign without harrassment, reasonable campaign finance laws, and so on. We view testing the relationship between multiparty competition and business cycles as a first step in looking at the relationship between competition and cycles more broadly.

⁴ Although we use Rogoff’s model to develop our argument, our analysis applies to the many variations in this branch of theory. The particular empirical implications of rational opportunistic political business cycle theory, however, do not differ dramatically from those of Nordhaus (1975). The primary distinction is that the traditional models concentrate on economic outcomes, while the more recent versions emphasize policy and spending interventions. Both branches of opportunistic theory are consistent with the types of interventions examined below. Indeed, we do not directly measure or test “competence” as described in Rogoff. Rather, we test for the types of observable behavior that are consistent with more institutionally accurate interpretations of opportunistic political business cycle theory.
In a related vein, Schultz (1995) advocates a general framework for political business cycle theory that more explicitly considers a politician’s benefits and costs from electoral economic manipulation. Rather than focus on the effects of multiparty competition as we do here, he explores the impact of public opinion polls. Also, his inquiry is limited to transfer payments around British elections. Our analysis advances a similar intuition, but looks at the impact of multiparty competition on structuring incentives for opportunistic cycles. Furthermore, we move beyond the industrialized world to extend these insights to the developing world.

Our first hypothesis, then, is that we should only see evidence of political business cycles in elections with rules allowing competition. In other words, there should be a significant difference in the occurrence of political cycles between multiparty and single party elections.

Founding Elections and Political Business Cycles

Political business cycles are by their nature dynamic processes, yet empirical testing has ignored temporal effects across elections. In the developing world – Africa in particular -- with its many nascent democracies, this question takes on added significance. There are various reasons why founding elections may be associated with special circumstances around political business cycles.⁵

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⁵ We apply the definition of founding elections proposed by Bratton and van de Walle (1997), in which “…the office of head of government was openly contested following a period during which multiparty politics had been denied.” (p. 196)
First, in transition or founding elections, we would expect authoritarian leaders to have greater discretion in manipulating pre-electoral economic policies. From the standpoint of incumbent politicians, initial competitive elections offer the incentive to deter entry by future challengers. By raiding the state coffers to shower constituents with pre-electoral spending, incumbents may attempt to scare off potential challengers and solidify their bases of support before the opposition has any influence on the policy-making process. Furthermore, in founding elections, they may face fewer institutional constraints in the form of legislatures, independent central banks, and a free press, thus making available a potentially wide range of fiscal and monetary policies as tools of manipulation.  

Moreover, as countries introduce competitive, multi-party elections, both incumbents and voters are thrown into a new world of uncertainty. The uncertainty driving political business cycles has a temporal as well as an institutional component. There are differences in voter’s information sets between founding elections and later elections. Voters may be the least “savvy” to electoral manipulation in the first election, providing incumbents with additional incentives to induce cycles. With no prior experience to temper their assessments relating prospective performance to pre-electoral performance, voters can evaluate candidates on only the available evidence – the pre-electoral surge in spending.

This story is consistent with models of rational retrospective voting, as well as the less theoretically encumbered intuition that inexperienced voters may be more easily fooled. This reasoning suggests a second hypothesis: We should see evidence of larger

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6 These are noted characteristics of the politics of many African countries. As we point out below, this is
rational opportunistic political budget cycles in “founding elections.” The pre-electoral cycles in founding elections should be significantly different not only from those prior to multiparty elections, but also from cycles in subsequent multiparty elections.

With these hypotheses in hand, we move to a discussion of data and empirical testing.

3. Data and Empirical Strategy

Africa as a “natural experiment” for testing the institutional assumptions of PBC theory

As noted above, our empirical refinement of rational opportunistic political business cycle theory is likely to be most relevant in the developing world. The case of Africa is particularly interesting, not only because of the watershed increase in the incidence of elections during the early 1990s (Bratton and van de Walle, 1997), but also because Africa provides a “natural experiment” for our test of the institutional drivers of political budget cycles.

One way to capture empirically the incentive of electoral uncertainty in driving political business cycles is to vary the competitiveness of elections. While many African countries have held elections, some have involved competition between parties while others have not (Ferree and Singh, 2001). From 1980-95, African countries held 65 presidential elections, just under half of which were competitive in the sense of allowing opposition parties to contest the elections.

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7 Indeed, its effect cannot be estimated without some degree of variability (typically absent in a sample of advanced democracies)
At the same time, many potentially confounding institutional constraints are held constant by limiting the empirical focus to Africa. In the industrialized world, the degree of discretion allowed incumbents to manipulate macro-economic policies is severely curtailed by independent central banks and legislatures. These institutional constraints lessen the likelihood of incumbent-induced electoral cycles. The story is quite different in most newly democratized African countries. The lack of independent monetary institutions and weak legislatures results in few checks on executive discretion to engage in pre-electoral economic manipulation. (Guillaume and Stasavage 1999) The discretion afforded to incumbents in many sub-Saharan African countries makes this part of the world a particularly good place to test hypotheses about the institutional bases of political business cycles. Furthermore, politics does not play out on a partisan right-left continuum in most African elections. Accordingly, a rational opportunistic framework better describes African electoral politics than does a rational partisan framework.

It is also notable that presidential terms in all the countries in our sample are for fixed periods, according to Nohlen, Krennerich and Thibaut (1999). This addresses some concern for the potential bias that could enter into our estimates if elections are endogenous (i.e., if leaders can call elections when the economy is doing well). Yet, Africa also stands out in this period for its relatively high incidence of founding elections. There are 22 such elections in our data set. Founding elections are potentially endogenous in their timing, which is typically at the incumbent’s discretion. Controlling for Africa’s founding elections is thus both interesting in itself, and further allays concern about electoral endogeneity.
Finally, focusing our analysis on African countries reflects the crucial role that research on Africa has played in focusing policy-makers and scholars on the economic importance of the political structures through which policy decisions are made.

*Data and Methodology*

The data used to test our hypothesis includes annual observations (1980-95) for 44 Sub-Saharan African countries (listed in Table 2), creating a panel of 704 country-year observations. Macroeconomic data are drawn from the IMF’s *International Financial Statistics*. Table 1 presents descriptive statistics for the macroeconomic aggregates used in the analysis. The dependent variables with which we test for political business cycles include public expenditure, net claims on the central government, real money growth, seignorage, and nominal exchange rate devaluation. Detailed definitions and sources of these data are provided in the footnotes to Table 1.

In terms of our independent variables, the first political variable is the date of presidential elections, which are summarized in Table 2. These election dates are drawn from Bratton and van de Walle (1996) and from Nohlen, Krennerich, and Thibaut (1999). The data set includes 65 presidential elections. Of the 44 countries in the sample, only 8 held no presidential elections, 17 held only one, and the remaining 19 held multiple elections during the period. In addition to the listed presidential elections, there were 107 legislative elections in the sample. We limit the analysis to presidential elections as they pose a more direct threat to power and are more relevant in a political context.

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8 Note that for the countries included in our sample, macroeconomic data are reported only annually. Accordingly, our dummy variable for election years does not differentiate between elections held early versus late in a given year. The approach to this problem taken by other studies limited to annual data (Alesina, Roubini, Cohen (1997)) is to score the election dummy variable to equal one in the prior year when the election occurs prior to 1 June. This adjustment did not alter our results or conclusions.
characterized by Bratton and van de Walle (1997, p. 63) as featuring “the systematic concentration of political power in the hands of one individual, who resists delegating all but the most trivial decision-making tasks.”

The second political variable is an indicator of electoral contestability, introduced by Ferree and Singh (2001). This scale (which we label ECMP) measures the level of competition that occurs during the executive selection process. Unlike other commonly used measures (i.e., Gastil’s political and civil liberties indices) that aggregate many considerations into a overall score, the executive scale captures a single, highly central component of electoral competitiveness – the presence or absence of competition within or between parties. While other factors also affect competition (for example, freedom of the press), they are more difficult to measure. Thus, the scale opts for specificity and clarity over trying to capture and test all aspects of competition that might matter. Ferree and Singh identify six levels as follows:

- Level 1 -- No executive exists
- Level 2 -- Executive exists but was not elected
- Level 3 -- Executive is elected, but was the sole candidate
- Level 4 -- Executive is elected, and multiple candidates competed for the office
- Level 5 -- Multiple parties were also able to contest the executive elections
- Level 6 -- Candidates from more than one party competed in executive elections

For the purposes of this analysis, the relevant distinction is between Level 6 (multiparty elections) and Levels 3 and 4 of the scale (single party elections). There are 33 multiparty elections in our data set and 32 single party. Table 2 provides a mapping between election dates and the executive scale levels. The combination of election dates and the scale of electoral competitiveness (ECMP) permits us to explore the impact of multiparty competition in shaping the incentives for opportunistic politicians to engage in
pre-electoral macroeconomic intervention. If multiparty electoral systems produce business cycles but single party systems do not, we will have uncovered an important channel through which political institutions affects economic policy-making and therefore, performance.

The econometric specification with which we test for political business cycles thus takes the form

where
(where founding elections are those identified as such by Bratton and van de Walle (1997) and indicated by boldface in Table 2), and \( a_i \) is an unobserved country-specific time-invariant effect.\(^9\)

The dependent variable in each specification is one of the four macroeconomic aggregates listed above. The appropriate number of lags (\( k \)) on the dependent variable was determined in each case by the Schwarz Information Criterion.

The specification in equation (1) is such that \( \hat{\beta}_1 \) captures the specific effect of single party non-founding elections on \( y_{i,t} \).\(^{10}\) The parameter estimate for the interaction term, \( \hat{\beta}_2 \) (and its associated t-statistic), measures the marginal difference between the effects of multiparty and single party non-founding elections. Confirmation of our hypothesis regarding electoral competition would lie in a finding that \( \hat{\beta}_1 \) is not statistically different from zero, while \( \hat{\beta}_1 + \hat{\beta}_2 \) (the total effect of multiparty elections on \( y_{i,t} \)) is significantly different from zero in the predicted direction. Similarly, the parameter estimate for \( \hat{\beta}_3 \) measures the marginal difference between the effects of multiparty non-founding elections and multiparty founding elections. Thus, if multiparty founding elections provide incentives for economic intervention, then the sum \( \hat{\beta}_1 + \hat{\beta}_2 + \hat{\beta}_3 \) will be significantly different from zero. The marginal contribution to changes in the dependent variable arising purely from a multiparty election also being a

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\(^9\) Note that one might well define the ECMP dummy variable to include observations where the executive scale is greater than or equal to 5. In this particular data set, however, there are no level 5 countries.

\(^{10}\) Note that founding elections, as defined by Bratton and van de Walle (1997), are multiparty. Thus, the specification need not address the potential effect of single party founding elections.
founding election is captured by $\hat{\theta}_3$ alone, confirming our second hypothesis concerning initial competitive elections.

The presence of lagged dependent variables with panel data complicates estimation. Dropping $a_i$ would permit consistent estimation of equation (1) by OLS (assuming no serial correlation the errors), but doing so comes at the expense of heterogeneity bias if $a_i$ is correlated with the included regressors. The standard LSDV (within) estimator that includes $a_i$ avoids heterogeneity bias, but is still inconsistent (with finite T) due to a correlation of the order $(1/T)$ between the explanatory variables and the residuals in the transformed model (Hsiao, 1986).

Arellano and Bond (1991) resolve these problems with a dynamic panel generalized methods of moments (GMM) estimator, the details of which are presented in Appendix 1. Arellano and Bond’s estimation strategy, in short, is to first-difference the equations to eliminate $a_i$, and to fix the resulting inconsistency by applying instrumental variables consisting of appropriately lagged levels of the variables. Based on derived moment conditions, the set of valid instruments grows incrementally as the year 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. Subsequent work in this vein has added to the list of moment conditions, leading to a System-GMM model (Blundell and Bond, 1998), which is also applied here as appropriate.

4. Results
The results presented in this section provide strong support for both of our hypotheses: multiparty elections are associated with political business cycles while single party ones are not; furthermore, cycles are larger in founding elections compared to subsequent ones. In keeping with the emphasis of the rational opportunistic branch of theory, we concentrate our empirical tests on policy interventions rather than real outcomes. Indeed, we find no significant results when testing for election-year effects on growth in GDP. In particular, we test for electorally timed interventions in two fiscal policy and three monetary policy variables: public expenditure, net claims on the central government, money growth, seignorage, and nominal exchange rate devaluation.  

Our specific hypotheses are as follows:

- For the first three dependent variables – public expenditure, net claims on the central government, and money growth – we expect a significant increase in election years relative to non-election years in countries with multiparty electoral systems and little if any effect in other countries.

- In the case of seignorage, rational opportunistic political business cycle theory predicts an increase in the post-election year (Alesina, Roubini, and Cohen, 1997). Our hypothesis in that case is that there is a significant increase in seignorage during post-election years relative to election years in countries with competitively elected executives, and little if any effect in other countries.

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11 Note that these monetary variables are not controlled directly by member governments in the French West African Currency Union. CFA zone countries are thus excluded from these estimations.
• We extend this post-election year hypothesis to exchange rate
devaluation, as well, though the relevant theoretical literature has
ignored this variable. 12

• We further hypothesize that transitions to democracy create added
incentives for pre-electoral economic manipulation. Hence, we expect
the occurrence of founding elections to magnify the observed political
business cycles.

These hypotheses are uniformly sustained in the results presented in Table 3.

Column (1) presents results for public expenditure. There is clearly no election
year effect in systems where the executive selection process is single party (ECMP_{i,t} < 6).
The interaction term indicates that the marginal impact of electoral competition increases
public expenditures during election years by 2.7 percentage points of GDP. The total
election-year increase in public expenditures in multiparty elections (the sum of the
estimates for single party elections and the interaction term for elections and multiparty
competition) is thus 2.4 percentage points of GDP. Column 1 further shows that the
effect of founding elections substantially (and statistically significantly) magnifies the
impact of multiparty elections. The occurrence of founding elections increases public
expenditures by 8.8 percentage points of GDP (a 6.3 percentage point marginal increase
over the effect of multi-party competition). These marginal and total effects are all
statistically significant at greater than the .01-level.

12 Among the few empirical papers to relate elections with devaluations are Frieden, Ghezzi, and Stein
(2000), and Klein and Marion (1997), both of which concentrate on Latin America.
Table 3 column (2) presents our findings for net claims on the central government, defined as claims on the central government by the central bank and deposit banks, less government deposits -- in essence, net government borrowing from the domestic banking system \((IFS, \text{line 32an})\). Examination of this variable helps to explain one means by which governments finance the election-year extravagances reflected in the results for public expenditure. In this case, as in the previous cases, there is no election-year effect in countries with ECMP < 6. (Logically, there is no need to “raid the candy store” in the absence of pre-election extravagance.) Yet, when elections are multiparty, they result in a 4.2 percentage point increase in net claims as a share of GDP. In this case, the marginal impact of founding elections is merely suggestive. The point estimate indicates an additional 3.6 percentage points (for a total increase of 7.8 percentage points) with founding elections; yet, this effect is imprecisely measured (P-value = .15).\(^{13}\)

Both fiscal policy interventions thus sustain our hypothesis that the incentives for pre-electoral intervention are contingent upon the presence of multiparty competition. The added opportunities that accompany democratic transition are also substantial.

Table 3 column (3) presents results for growth in the real money supply. In this case, neither single party nor multiparty elections \textit{per se} give rise to pre-electoral surges in money growth. Yet, founding elections are associated with a substantial increase of 17 percentage points in the rate of monetary expansion. The increase attributed to founding elections could reflect the greater control that authoritarian leaders exercise over money supply, lending support to our first explanation for the significance of founding elections.

\(^{13}\) It is reasonable to expect that founding elections may not differ in this respect, since the same means of financing would be available in founding and subsequent elections. In addition, there may be greater availability of foreign aid for founding elections.
(i.e., authoritarian regimes have more discretion to manipulate policy). In reference to our second conjecture on voter information, voters cannot observe the growth in money supply prior to the election and therefore, cannot include it in their decision calculus.

Table 3 column (4) presents our results for seignorage. This variable complements net claims on government as an explanation for how politicians finance their election-year sprees of public spending. In the case of seignorage, theory suggests that increases will occur in the post-election year. Here, too, our findings strongly confirm theory. However, even single party elections give rise to post-election increases in seignorage, which is somewhat puzzling given our logic. The evidence in column 4 indicates that single party elections are followed by increases in seignorage equivalent to 1.8 percentage points of GDP. This result is statistically significant at the .01 level. The marginal impact of multiparty competition in this case is negative (though only marginally significantly so). The total increase in seignorage, however, remains positive for multiparty elections, increasing by over 1.2 percentage points of GDP. Once again, the opportunities and incentives created by democratic transition prove substantial: the total effect of founding elections is double that of competitive non-founding elections, with a point estimate of 2.46 percentage points of GDP. This result, too, is statistically significant at the .01 level.

Exchange rate devaluations are politically unpopular. Indeed, Table 3 column (5) presents clear evidence that incumbents wait until after elections to devalue nominal exchange rates. This dependent variable, though uncommon in the political business cycle literature, may be particularly relevant in Africa where incumbents often directly set exchange rates. In keeping with our maintained hypothesis, this result does not apply
in single party electoral systems. Yet, we find that average nominal exchange rate
devaluation in post-election years in multiparty systems increases by over 32 percentage
points relative to other years. For founding elections, the point estimate is a devaluation
of over 40 percentage points, though the latter is not statistically significantly different
from multiparty elections in general.

In sum, our results sustain our hypothesis that incumbents’ incentive to create
political business cycles in nascent democracies is strong, but contingent on multiparty
competition. Seignorage is the only variable tested for which we find evidence of
political intervention in single party as well as multiparty systems. In contrast, multiparty
systems differentially give rise to election-year interventions in public expenditures, net
claims on government, and nominal devaluations. Further, the occurrence of founding
elections magnifies the effect of multiparty competition in general in the cases of public
expenditure, money growth, and seignorage (and fall just short of statistical significance
in the case of net claims). That real money growth is evident only in the cases of
founding elections, where incentives and opportunities for intervention are magnified
may reflect greater direct executive control of money supply in countries prior to the
institution of multiparty elections.

5. Summary and Discussion

In this paper, we present a more institutionally rich test of rational opportunistic
political business cycles by considering the incentives and constraints imposed on
politicians by different electoral institutions. Specifically, we reason that in settings
where elections do not entail multiparty competition, an incumbent’s utility function does
not produce incentives to engage in electoral economic manipulation. Furthermore, we
hypothesize that initial or founding multiparty elections would present both the greatest incentives and the fewest constraints for electoral economic manipulation.

Applying recent dynamic panel econometric techniques to data from African countries, we find strong support for both hypotheses: 1) the existence of political business cycles is contingent on having multiparty elections, and 2) founding multiparty elections exhibit larger cycles than subsequent ones. Our findings demonstrate the existence of election-year increases in public expenditure, net claims on government, and post-election year surges in seignorage in multiparty electoral systems, and post-election nominal devaluations. Only in the case of seignorage is there any evidence of such effect in countries with single party elections. Moreover, our evidence strongly supports the conclusion that founding elections magnify incumbents’ incentives to create political business cycles. Thus, it appears that competition between political parties (inter-party competition) is a crucial institutional driver of rational opportunistic models of political business cycles.

Increasing competition by allowing for multi-party elections is often the first step in democratization reforms. However, our findings suggest that political reform as proxied by the introduction of multiparty competition may work at cross purposes with on-going efforts at economic reform – twin challenges in many developing regions, Africa in particular. This may offer some insights into the fragility of young democracies (Przeworski, Alvarez, Cheibub, and Limongi 2000), and highlight the importance of institutional checks on executive discretion in sustaining political and economic reforms.

By inducing macroeconomic cycles, multiparty competition seems to make a difference in politician’s responsiveness to citizens, though perhaps not in a Pareto-
improving way. Does this suggest that we need to reframe some of the debates around democracy and representation – whether elections actually induce accountability? (Przeworski, Stokes, and Manin 1999) Empirical verification of democratic accountability has too often looked for evidence of Pareto-improving outcomes in economic policy-making: studies look for relationships between survival rates of governments or vote shares and economic performance, for example. (Cheibub and Przeworski 1999; Lewis-Beck 1988; Paldam 1981; Strom and Lipset 1984; Lewis-Beck and Mitchell 1990; Powell and Whitten 1993) Perhaps scholars are looking for the wrong type of evidence? Can we instead learn something about how elections foster accountability from studying political business cycles in more institutionally rich models? Our results suggest that multiparty elections do, indeed, induce responsiveness and accountability of sorts, though not necessarily in the form of better economic policies.  

The evidence of cycles in multiparty elections is consistent with two voting models. In the first voting model, as Rogoff suggests, voters are trying to select competent leaders and use economic stimulation as a signal of competence. In the second framework, votes are bought directly through patronage spoils. Voters reward patronage spoils, so incumbents increase spending around election time. Both voting models are consistent with the evidence of cycles that we find; indeed, our results cannot distinguish between the scenarios. Doing so should be a priority for future research in this area, though it will require teasing out additional observational implications that demarcate the models. If the cycles signal competence in managing the economy and voters try to elect the more competent incumbents, we would expect those countries with  

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14 Perhaps Rogoff’s contention that elections are the “price” paid for more competent politicians may come
political business cycles to have more competent office-holders and therefore exhibit better long-run growth performance. We do not directly uncover such evidence; however, at first glance, Africa’s historically disappointing growth performance lends little support to a competence-seeking voting model. African electoral politics have by and large been a politics of patronage.

Nevertheless, our results on founding elections offer insight into the dynamics of learning in political business cycles. We discover that subsequent elections have less distortionary political business cycles than founding elections: could this mean that the economic performance-enhancing effects of competitive elections are learned over time? Do voters need time to learn how to discern competence? Or are checks on the executive put in place with subsequent elections? We offer no direct insight into such questions here, but Ferree and Singh (2001) find some evidence suggesting that the growth rates of African states with multi-party elections rise over time.

There may be a significant learning curve for both politicians and voters in learning to send and interpret accurate electoral signals. Certainly, the magnitude of political business cycles changes over time in democratic systems from the first election to subsequent ones. To adequately address this question will require us to elaborate the institutional foundations of our theoretical models. Current political business cycle theory typically models single election cycles in isolation from one another. Yet, in so far as enduring political institutions tie together these elections, they may need to be modeled as related events. Further attention must also focus on the process through

closer to capturing the benefits of democratic accountability.
which voters form expectations, and how political institutions figure into this process of molding expectations.
References

Alesina, A., “Macroeconomic Policy in a Two-Party System as a Repeated Game,”


Appendix 1

This appendix provides supporting technical details for the GMM estimators used in the paper. The generic case of a dynamic panel model takes the form

\[ y_{it} = \gamma y_{i,t-1} + \beta x_{it} + \alpha_i + u_{it} \]  

(A.1)

Where \( a_i \) represents unobserved time-invariant country-specific effects, \( x_{it} \) is an exogenous variables, and \( u_{it} \) is assumed to be i.i.d. Arellano and Bond (1991) note, given the assumption that \( E(u_{it}) = E(u_{it} u_{is}) = 0 \) for \( t \neq s \), that for the first difference of equation A.1, values of \( y \) lagged two periods or more (as well as similarly lagged differences of \( y \)) are valid instruments since they are correlated with \( (y_{it-1} - y_{it-2}) \) but uncorrelated with \( (u_{it} - u_{it-1}) \). In general, for \( T = 3 \), Arellano and Bond demonstrate the existence of \( m = (T - 2)(T - 1)/2 \) linear moment restrictions

\[ E[(\Delta y_{it} - \gamma \Delta y_{i,t-1}) y_{i,t-j}] = 0 \quad (j = 2, \ldots, (t-1); t = 3, \ldots, T) \]

where \( \Delta y_{it} = y_{it} - y_{it-1} \) and \( \Delta y_{i,t-1} = y_{i,t-1} - y_{i,t-2} \).

In models such as equation A.1, which include exogenous variables \( (x_{it}) \), a distinction is made between predetermined and strictly exogenous variables. In the former case, \( E(x_{is} u_{is}) \neq 0 \) for \( s < t \) and zero otherwise, in which case only \( x_{i1}, \ldots, x_{is-1} \) are valid instruments. In the latter case, \( E(x_{is} u_{is}) = 0 \) for all \( s \) and \( t \), in which case all the \( x_{is}'s \) are valid instruments. In the present application, \( x_{it} \) is a dummy variable indicating the timing of an election. As elections in several of the countries in the African sample
are based on parliamentary systems without fixed election schedules, we take the more conservative approach of treating the exogenous variables as merely predetermined. In this case, the optimal matrix of instrumental variables takes the form

\[ Z_i = \text{diag}(y_{it}, y_{it}, x_{i1}, \ldots, x_{it+1}), \quad (s = 1, \ldots, T - 2). \]

The estimation strategy, in short, is to first-difference the equations to eliminate the country effect, 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. It remains, however, to fix the problem of autocorrelated errors in the resulting model. Arellano and Bond (1991) propose a Generalized Method of Moments estimator for the \( k \times 1 \) coefficient vector \( \delta = (\gamma, \beta)' \)

\[ \tilde{\delta} = (\tilde{X}'Z\tilde{A}_NZ\tilde{X})^{-1} \tilde{X}'Z\tilde{A}_N \tilde{Z}' \tilde{y} \]

where \( \tilde{X} \) is a stacked \((T-2)N \times k\) matrix of observations on \( x_{it} \) and \( A_N \) is an optimally chosen weight matrix.
Table 1  Descriptive Statistics for Macroeconomic Variables, 44 Sub-Saharan African Countries (1980-95)

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>N&lt;sup&gt;a&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td>Govt. Cons./GDP&lt;sup&gt;b&lt;/sup&gt;</td>
<td>16.5</td>
<td>7.2</td>
<td>628</td>
</tr>
<tr>
<td>Real Money Growth&lt;sup&gt;f&lt;/sup&gt;</td>
<td>2.2</td>
<td>27.7</td>
<td>290</td>
</tr>
<tr>
<td>Net Claims/GDP&lt;sup&gt;d&lt;/sup&gt;</td>
<td>8.9</td>
<td>15.9</td>
<td>512</td>
</tr>
<tr>
<td>Seignorage&lt;sup&gt;e,f&lt;/sup&gt;</td>
<td>1.21</td>
<td>2.64</td>
<td>469</td>
</tr>
<tr>
<td>Exchange Rt. Growth&lt;sup&gt;f,g&lt;/sup&gt;</td>
<td>16.5</td>
<td>21.3</td>
<td>417</td>
</tr>
</tbody>
</table>

a.  N indicates the maximum number of country-year observations available.
b.  Government consumption: IFS, line 91f; GDP: IFS, line 99B.
c.  Annual growth of M1 (IFS, line 34) deflated by CPI (IFS, line 64).
d.  Net Claims on Central Govt. equals claims on central government, less central government deposits (IFS, line 32an).
e.  Seignorage equals annual inflation times the stock of high-powered money (IFS, line 14) as a share of GDP.
f.  Excludes CFA-zone countries.
g.  Annual growth of official exchange rate (IFS, line af)
Table 2. Presidential Elections in Africa (1980-1995)

<table>
<thead>
<tr>
<th>Country</th>
<th>Presidential Election Dates [Executive Scale Rating]</th>
</tr>
</thead>
<tbody>
<tr>
<td>Angola</td>
<td>None</td>
</tr>
<tr>
<td>Botswana</td>
<td>None</td>
</tr>
<tr>
<td>Burkina Faso</td>
<td>1 December 1991 [3]</td>
</tr>
<tr>
<td>Burundi</td>
<td>31 August 1984 [3]; 1 June 1993 [6]</td>
</tr>
<tr>
<td>Cape Verde</td>
<td>17 February 1991 [6]</td>
</tr>
<tr>
<td>Chad</td>
<td>None</td>
</tr>
<tr>
<td>Ethiopia</td>
<td>None</td>
</tr>
<tr>
<td>Gabon</td>
<td>9 November 1986 [4]; 5 December 1993 [6]</td>
</tr>
<tr>
<td>Lesotho</td>
<td>None</td>
</tr>
<tr>
<td>Liberia</td>
<td>15 October 1985 [6]</td>
</tr>
<tr>
<td>Malawi</td>
<td>17 May 1994 [6]</td>
</tr>
<tr>
<td>Mauritius</td>
<td>None</td>
</tr>
<tr>
<td>Namibia</td>
<td>7 December 1994 [6]</td>
</tr>
<tr>
<td>Country</td>
<td>Date(s)</td>
</tr>
<tr>
<td>------------------</td>
<td>------------------------------</td>
</tr>
<tr>
<td>Sierra Leone</td>
<td>1 October 1985 [3]</td>
</tr>
<tr>
<td>Somalia</td>
<td>23 December 1986 [3]</td>
</tr>
<tr>
<td>South Africa</td>
<td>None</td>
</tr>
<tr>
<td>Sudan</td>
<td>14 April 1983 [3]</td>
</tr>
<tr>
<td>Swaziland</td>
<td>None</td>
</tr>
<tr>
<td>Uganda</td>
<td>None</td>
</tr>
<tr>
<td>Zaire (P.R. Congo)</td>
<td>28 July 1984 [3]</td>
</tr>
<tr>
<td>Zimbabwe</td>
<td>March 1990 [6];</td>
</tr>
</tbody>
</table>

Table 3  GMM Estimates of Political Business Cycles in Africa, 1980-95\(^a\)

<table>
<thead>
<tr>
<th>Dependent Variable</th>
<th>Public Expenditure (1)</th>
<th>Net Claims on Govt.(^e) (3)</th>
<th>Money Growth(^f,g) (2)</th>
<th>Seignorage(^h) (4)</th>
<th>Exchange Rate Devaluation(^e,h) (5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>ELE ((\beta_1))</td>
<td>-.002</td>
<td>-.002</td>
<td>-.009</td>
<td>1.84(***)</td>
<td>3.94</td>
</tr>
<tr>
<td></td>
<td>(.005)(^d)</td>
<td>(.019)</td>
<td>(.055)</td>
<td>(.113)</td>
<td>(8.08)</td>
</tr>
<tr>
<td>ELE x ECMP6 ((\beta_2))</td>
<td>.027(***)</td>
<td>.044(^\dagger)</td>
<td>-.020</td>
<td>-.602(*)</td>
<td>28.28(**)</td>
</tr>
<tr>
<td></td>
<td>(.007)</td>
<td>(.027)</td>
<td>(.084)</td>
<td>(.350)</td>
<td>(14.66)</td>
</tr>
<tr>
<td>ELE x FOUND ((\beta_3))</td>
<td>.063(***)</td>
<td>.036</td>
<td>.200(*)</td>
<td>1.22(***)</td>
<td>8.08</td>
</tr>
<tr>
<td></td>
<td>(.008)</td>
<td>(.046)</td>
<td>(.108)</td>
<td>(.278)</td>
<td>(15.46)</td>
</tr>
<tr>
<td>n</td>
<td>70</td>
<td>200</td>
<td>169</td>
<td>110</td>
<td>260</td>
</tr>
<tr>
<td>Sargon(^b)</td>
<td>[.515]</td>
<td>[.221]</td>
<td>[.312]</td>
<td>[.243]</td>
<td>[.150]</td>
</tr>
<tr>
<td>LM2(^c)</td>
<td>[.315]</td>
<td>[.656]</td>
<td>[.277]</td>
<td>[.162]</td>
<td>[.149]</td>
</tr>
</tbody>
</table>

Total Effect in
Competitive System
(\(\beta_1 + \beta_2\))

| .024\(***\) | .042\(**\) | -.029 | 1.24\(***\) | 32.22\(**\) |
| (.006)       | (.020)     | (.061) | (.322)      | (13.15)      |

Total Effect of
Competitive Founding Election
(\(\beta_1 + \beta_2 + \beta_3\))

| .088\(***\) | .078 | .171\(**\) | 2.46\(***\) | 40.30\(***\) |
| (.005)       | (.055) | (.086) | (.185)      | (9.11)       |

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

\(^1\) P-value = .107

\(^a\) Specification includes lags of dependent variable. Results suppressed (available from authors).

\(^b\) P-value of Sargon test of over-identifying restrictions (null hypothesis of acceptable instruments).

\(^c\) P-value of Lagrange multiplier test of second-order serial correlation in the first-differenced residuals.

\(^d\) Heteroskedasticity-consistent standard error.

\(^e\) Estimator is System-GMM (Blundell and Bond, 1998).

\(^f\) Excludes observations from CFA zone countries.

\(^g\) Arellano and Bond 1-step estimator result.

\(^h\) The election dummy in the seignorage and exchange rate specifications indicates the post-election year.