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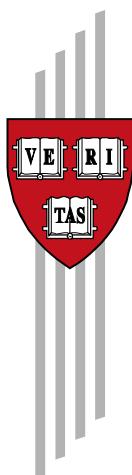
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Multiparty Competition, Founding Elections and Political Business Cycles in Africa

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Abstract

Political business cycle theory largely abstracts from institutional context, in particular assuming that elections are competitive. Yet, as empirical work on political business cycles turns increasingly to developing countries and nascent democracies for evidence, this assumption becomes untenable. We propose and test two empirical hypotheses: first, we should only see cycles when elections involve multiparty competition; second, we should see larger cycles in founding elections. Using [an](#) indicator of multiparty competition and applying recent advances in dynamic panel (generalized method of moments) econometrics to 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.

JEL: D72, N17, O11, O23

Keywords: Elections, Political Business Cycles, Africa, Democratization

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.² Recent 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), however, have applied existing theories without regard for possible differences in institutional context that differentiate developing from the developed countries for which such theories were

¹ See Widner’s 1994 volume, for example.

intended. In particular, 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. How do multiparty elections affect economic policy choices and spending decisions? 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 cycles theory in two ways: first by explicitly testing the effect of the absence of multi-party competition on the realization of political business cycles, and second by allowing the magnitude of political business cycles to vary as a function of whether a given election is the country’s *first* competitive election. Sub-Saharan Africa, with its rapid increase in the incidence of elections with multi-party competition (Bratton and van de Walle, 1997) as well as its relative lack of institutional development, provides the ideal testing ground for our proposed extensions of the empirical testing of political business cycle theory. Indeed, our results strongly confirm not only the existence of political business cycles in Africa, but the importance of considering explicitly the introduction and effects of multi-party electoral competition in empirical analysis.

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

² See Drazen (2000) and Alesina, Roubini, and Cohen (1997) for excellent reviews of both theoretical and

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 has received at best mixed support from extensive tests against data from advanced economies. This is somewhat surprising, given the uniformity with which political business cycle theory has rested upon the assumed existence of the types of institutional structures – multi-party electoral competition in particular – common to the advanced economies. These assumptions are common to both “partisan” (Hibbs, 1977; Alesina, 1987) and “opportunistic” (Nordhaus, 1975; Rogoff and Sibert, 1988; Rogoff, 1990) theories of political business cycles.

In the 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

empirical work on political business cycles

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, though opportunistic theory in particular has relied on implicit assumptions regarding the competitiveness of electoral institutions. Indeed, it has been opportunistic political business cycle theory that has guided the limited empirical testing to date that concentrates on developing countries.

In general, empirical tests in developing countries have provided stronger support for political business cycle theory. 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 is it in particular about developing countries that appears to make this true? 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 by testing explicitly the relationship between the presence of multiparty competition during elections and the existence of political business cycles.

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 as to the outcome of elections 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 motivate 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

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.

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 harassment, 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

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

First, in transition 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.⁶

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

Rogoff. Rather, we test for the types of observable behavior that are *consistent* with more institutionally accurate interpretations of opportunistic political business cycle theory.

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)

⁶ These are noted characteristics of the politics of many African countries. As we point out below, this is one of compelling reasons to use African data for empirical tests of our hypotheses.

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 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.⁷ 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.

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

⁷ Indeed, its effect cannot be estimated without some degree of variability (typically absent in a sample of

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, seigniorage, 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 2.⁸ These election dates are drawn

advanced democracies)

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

from Bratton and van de Walle (1996) and from Nohlen, Kremerich, 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 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

(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.

- 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

$$y_{i,t} = \sum_{j=1}^k \gamma_j y_{i,t-j} + \beta_1 ELE_{i,t} + \beta_2 (ELE * ECMP6)_{i,t} + \beta_3 (ELE * ECMP6 * FOUND)_{i,t} + \alpha_i + \varepsilon_{i,t} \quad (1)$$

where

$$ELE_{i,t} = \begin{cases} 1 & \text{if } t \text{ is an election year} \\ 0 & \text{otherwise,} \end{cases}$$

$$ECMP6_{i,t} = \begin{cases} 1 & \text{if } ECMP = 6 \\ 0 & \text{otherwise,} \end{cases}$$

$$FOUND_{i,t} = \begin{cases} 1 & \text{if founding election} \\ 0 & \text{otherwise} \end{cases}$$

(where founding elections are those identified as such by Bratton and van de Walle (1997) and indicated by boldface in Table 2), and α_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

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

$y_{i,t}$) 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 founding election is captured by $\hat{\beta}_3$ alone, confirming our second hypothesis concerning initial competitive elections.

The presence of lagged dependent variables with panel data complicates estimation. Dropping α_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 α_i is correlated with the included regressors. The standard LSDV (within) estimator that includes α_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 α_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

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.

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, seigniorage, and nominal exchange rate devaluation.¹¹

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.

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.

- In the case of seigniorage, 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 seigniorage during post-election years relative to election years in countries with competitively elected executives, and little if any effect in other countries.
- 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

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.

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.

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*, 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.

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.

Table 3 column (3) presents results for growth in the real money supply. In this case, neither single party nor multiparty elections *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 (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 seigniorage in multiparty electoral systems, and post-election nominal devaluations. Only in the case of seigniorage 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.¹⁴

¹⁴ Perhaps Rogoff's contention that elections are the "price" paid for more competent politicians may come closer to capturing the benefits of democratic accountability.

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 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 which voters form expectations, and how political institutions figure into this process of molding expectations.

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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

$$y_{it} = \gamma_{it-1} + \beta x_{it} + \alpha_i + u_{it} \quad (\text{A.1})$$

Where α_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 $T \geq 3$, Arellano and Bond demonstrate the existence of $m = (T - 2)(T - 1)/2$ linear moment restrictions

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

where $\Delta y_{it} = y_{it} - y_{it-1}$ and $\Delta y_{it-1} = y_{it-1} - y_{it-2}$.

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, $E(x_{it}u_{is}) \neq 0$ for $s < t$ and zero otherwise, in which case only x_{i1}, \dots, x_{is-1} are valid instruments. In the latter case, $E(x_{it}u_{is}) = 0$ for all s and t , in which case all the x_{it} '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

$$Z_i = \text{diag}(y_{i1} \dots y_{is} x_{i1} \dots x_{is+1}), (s = 1, \dots, T - 2).$$

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 $\delta = (\gamma, \beta)'$

$$\hat{\delta} = (\bar{X}' Z A_N Z' \bar{X})^{-1} \bar{X}' Z A_N Z' \bar{y}$$

where \bar{X} is a stacked $(T-2)N \times k$ matrix of observations on \bar{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)

Variable	Mean	Std. Dev.	N ^a
Govt. Cons./GDP ^b	16.5	7.2	628
Real Money Growth ^{c,f}	2.2	27.7	290
Net Claims/GDP ^d	8.9	15.9	512
Seignorage ^{e,f}	1.21	2.64	469
Exchange Rt. Growth ^{f,g}	16.5	21.3	417

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)

Country	Presidential Election Dates [Executive Scale Rating]
Angola	None
Benin	24 March 1991 [6]
Botswana	None
Burkina Faso	1 December 1991 [3]
Burundi	31 August 1984 [3]; 1 June 1993 [6]
Cameroon	4 April 1980 [3]; 14 Jan 1984 [3]; 24 April 1988 [3]; 11 Oct 1992 [6]
Cape Verde	17 February 1991 [6]
Central African Republic	15 March 1981 [2]; 25 Oct. 1992 [3]; 22 August 1993 [6]
Chad	None
Congo	2 June 1992 [6]
Côte d'Ivoire	12 October 1980 [3]; 27 Oct 1985 [3]; 28 Oct 1990 [6]
Djibouti	12 June 1981 [3]; 24 April 1987 [3]; 7 May 1993 [6]
Equatorial Guinea	15 August 1982 [3]; 25 June 1989 [3]
Ethiopia	None
Gabon	9 November 1986 [4]; 5 December 1993 [6]
Gambia	4 May 1982 [6]; 11 March 1987 [6]; April 1992 [6]
Ghana	3 November 1992 [6]
Guinea	9 May 1982 [3]; 19 December 1993 [6]
Guinea-Bissau	7 August 1994 [6]
Kenya	26 Sept 1983 [3]; 21 March 1988 [3]; 29 December 1992 [6]
Lesotho	None
Liberia	15 October 1985 [6]
Madagascar	7 November 1982 [4]; 12 March 1989 [4]; 10 February 1993 [6]
Malawi	17 May 1994 [6]
Mali	9 June 1985 [3]; 26 April 1992 [6]
Mauritania	24 January 1992 [6]
Mauritius	None
Mozambique	27 October 1994 [6]
Namibia	7 December 1994 [6]
Niger	10 December 1989 [3]; 27 March 1993 [6]
Nigeria	6 August 1983 [6]; 12 June 1993 [2]

Rwanda	19 December 1983 [3]; 19 December 1988 [3]
Senegal	27 February 1983 [6]; 28 February 1988 [6]; February 1993 [6]
Sierra Leone	1 October 1985 [3]
Somalia	23 December 1986 [3]
South Africa	None
Sudan	14 April 1983 [3]
Swaziland	None
Tanzania	26 Oct 1980 [3]; 27 Oct 1985 [3]; 28 Oct 1990 [3]; 29 Oct 1995 [6]
Togo	21 December 1986 [3]; 25 August 1993 [6]
Uganda	None
Zaire (P.R. Congo)	28 July 1984 [3]
Zambia	27 October 1983 [3]; 26 Oct 1988 [3]; 31 Oct 1991 [6]
Zimbabwe	March 1990 [6];

Source: Bratton and van de Walle (1996), Nohlen, Krennerich, and Thibaut (1999).

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

Dependent Variable	Public Expenditure (1)	Net Claims on Govt. ^e (3)	Money Growth ^{f,g} (2)	Seignorage ^{f,h} (4)	Exchange Rate Devaluation ^{e,f,h} (5)
ELE (β_1)	-.002 (.005) ^d	-.002 (.019)	-.009 (.055)	1.84*** (.113)	3.94 (8.08)
ELE x ECMP6 (β_2)	.027*** (.007)	.044 [†] (.027)	-.020 (.084)	-.602* (.350)	28.28** (14.66)
ELE x FOUND (β_3)	.063*** (.008)	.036 (.046)	.200* (.108)	1.22*** (.278)	8.08 (15.46)
n	70	200	169	110	260
Sargon ^b	[.515]	[.221]	[.312]	[.243]	[.150]
LM2 ^c	[.315]	[.656]	[.277]	[.162]	[.149]
Total Effect in Competitive System $(\beta_1 + \beta_2)$.024*** (.006)	.042** (.020)	-.029 (.061)	1.24*** (.322)	32.22** (13.15)
Total Effect of Competitive Founding Election $(\beta_1 + \beta_2 + \beta_3)$.088*** (.005)	.078 (.055)	.171** (.086)	2.46*** (.185)	40.30*** (9.11)

^a *** = significant at .01 level; ** = significant at .05 level; * = significant at .10 level[†] 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*.