

The impact of central bank independence on political monetary cycles in advanced and developing nations*

Sami Alpanda^a, Adam Honig^{a**}

^a*Amherst College, Amherst, MA 01002*

March, 2009

Abstract

This paper examines the extent to which monetary policy is manipulated for political purposes during elections. We do not detect political monetary cycles in advanced countries or developing nations with independent central banks. We do find evidence, however, in developing countries that lack central bank independence. Furthermore, we find some evidence that these cycles are not caused by monetization of election-related fiscal expansions. This suggests that pressure by politicians on the central bank to exploit the Phillips curve may be an important factor in generating political monetary cycles.

JEL Classification: E52; E58

Keyword(s): Political monetary cycles; central bank independence

* We have greatly benefited from comments from two anonymous referees. We also wish to thank Jun Ishii, Roger Kaufman, Walter Nicholson, Geoffrey Woglom, Beth Yarbrough, and participants at the Five College Faculty Seminar for helpful comments and discussions. We are grateful to Octavia Foarta for excellent research assistance. All remaining errors are our own.

** Corresponding author: 315 Converse Hall, Amherst College, Amherst, MA 01002-5000. Phone: (413) 542-5032. Fax: (413) 542-2090. *Email:* ahonig@amherst.edu

1. Introduction

This paper investigates the extent to which monetary policy is manipulated for political purposes during election periods. In the political business cycle model of Nordhaus (1975), politicians attempt to lower the unemployment rate before elections to raise their chances of reelection. Implicit in this idea is first, that macroeconomic policy is not neutral (at least in the short-run) and therefore can alter economic outcomes; second, voters reward politicians for higher growth during election years; third, voters value growth more than other economic objectives such as low inflation; and fourth, politicians are willing and able to manipulate policy in order to exploit this short-run non-neutrality for their own benefit. Each of these issues has been explored in the literature.¹

In this paper, we concentrate on the last issue, in particular the presence of political *monetary* cycles, where there is a lack of consistent evidence. In most empirical research on this issue, a monetary policy instrument or inflation is regressed on an election cycle variable, whose coefficient is then used to test whether policy is significantly different near elections. Using this approach, Alesina and Roubini (1992), Beck (1987), Golden and Poterba (1980), and Leertouwer and Maier (2001) do not find evidence of political monetary cycles for the U.S. and OECD countries, in contrast to the findings of Boschen and Weise (2003), Grier (1987), Haynes and Stone (1989), and Abrams and Iossifov (2006).

¹ On the issue of effectiveness of policy, Lucas (1972), Sargent (1973), Sargent and Wallace (1975), and McCallum (1978) argue that rational expectations on the part of agents imply neutrality for monetary policy. Subsequent models with asymmetric information (Cukierman and Meltzer, 1986) and nominal rigidities (Fischer, 1977, Phelps and Taylor, 1977) show that monetary policy can have real effects in the short-run. Rational expectations reduce the effect of policy on economic outcomes and therefore the incentives for political manipulation prior to elections, while not completely eliminating them. On the issue of whether the economic situation right before an election has a significant impact on the election's outcome, Fair (1978, 1982) has found evidence that the voting behavior in the U.S. is in general responsive to economic conditions. In contrast, Brender and Drazen (2005b) find that voters in advanced countries do not reward politicians for higher growth. They are rewarded in developing countries but only for overall growth performance during their whole term in office; there is no additional reward for growth in the year of the election. This does not preclude the possibility, however, that politicians believe they will be rewarded and therefore exploit policy before the election.

The conflicting evidence for political monetary cycles may be a result of the literature's concentration on advanced economies. We add to the literature by using a larger sample that also includes developing nations where these cycles are more likely to exist.² Central banks in developing countries are less independent from the central government compared to their advanced economy counterparts (Cukierman, et. al, 1992) and therefore are more vulnerable to political pressure. We do not find evidence of political monetary cycles in advanced countries or developing countries with high levels of central bank independence (CBI). We do, however, detect cycles in developing countries with low levels of CBI.

We also investigate the causes of political monetary cycles. There are two primary channels through which political manipulation of monetary policy might operate before or during election years. The first is what we call the Phillips curve channel, in which politicians pressure the central bank to loosen monetary policy to stimulate the economy. The second is the fiscal-financing channel, whereby politicians force the central bank to finance election-related increases in government spending (or tax cuts). The fiscal-financing channel relies on the existence of political *budget* cycles, in which governments use expansionary fiscal policy to expand the economy and/or increase government handouts and transfers to certain constituencies.³ These fiscal expansions can in principle be financed through borrowing. In cases where the government's borrowing capacity is limited, however, central banks may be called in to monetize instead. Our results provide some evidence against the fiscal-financing

² Dreher and Vaubel (2004) test for political monetary cycles in countries with loans from the International Monetary Fund and the International Bank for Reconstruction and Development. They find a positive effect of elections on money growth using GMM but not OLS or 2SLS estimation. They do not, however, consider the impact of central bank independence on this effect.

³ Implicit in this idea is that voters reward politicians for higher overall spending (as opposed to the composition of spending (Drazen and Eslava, 2007)) during election years, and that they value the fiscal expansion more than low inflation or fiscal discipline.

channel. This suggests that pressure on the central bank to stimulate the economy by exploiting the Phillips curve may play an important role in generating political monetary cycles.⁴

The rest of the paper is organized as follows: Section 2 introduces the data and the benchmark regression equation that we use to test for political monetary cycles and the role of CBI. Section 3 presents the results and robustness checks. Section 4 concludes.

2. Testing for Political Monetary Cycles and the Role of CBI

In this section, we introduce our benchmark model and data used to test for political monetary cycles and the role of CBI. We consider a regression of a monetary policy indicator, M , on its own lag, an election-cycle variable, EC , and several control variables. Our benchmark specification is given by

$$\begin{aligned}
 M_{i,t} = & \beta_0 + \beta_1 \cdot EC_{i,t} + \beta_2 \cdot M_{i,t-1} + \beta_3 \cdot CBI_{i,t} + \beta_4 \cdot FIX_{i,t} \\
 & + \beta_5 \cdot BORCAP_{i,t} + \beta_6 \cdot YGAP_{i,t} + \beta_7 \cdot INFL_{i,t} \\
 & + \beta_8 \cdot (EC_{i,t} * CBI_{i,t}) + \beta_9 (EC_{i,t} * FIX_{i,t}) + \beta_{10} \cdot (EC_{i,t} * BORCAP_{i,t}) + \alpha_i + \varepsilon_{it} \quad (1)
 \end{aligned}$$

where i indexes country and t indexes time. The control variables used in the regression are a measure of independence, CBI , a dummy variable indicating a fixed exchange rate regime, FIX , the output gap, $YGAP$, the inflation rate, $INFL$, and a measure of the government's borrowing capacity, $BORCAP$. We also interact EC with CBI , FIX , and $BORCAP$. α_i is a country-specific component of the error term.

The sample includes 63 countries for which all necessary data were available.⁵ We use annual data for the years 1972 to 2001. The starting point, 1972, coincides with the earliest year

⁴ The absence of a fiscal-financing channel is still consistent with the presence of political budget cycles. For example, Brender and Drazen (2005a) conclude that political budget cycles exist in new democracies where voters are inexperienced with electoral politics.

for which there is data available on the Freedom House democracy indicators that were used to filter elections. The fixed exchange rate regime indicator is available until 2001, which defines the end point of the sample period.

Because of the dynamic panel bias caused by the presence of the lagged dependent variable, we estimate the model using the GMM systems estimator developed in Arellano and Bover (1995) and Blundell and Bond (1998).⁶ Results from fixed effects estimation are similar and are discussed along with the robustness tests in Section 3.

2.1 Monetary Policy Variable

We use the annual percentage growth rate of M1 as the monetary policy indicator. The data are from the IMF's *International Financial Statistics (IFS)*. We do not use inflation as our monetary indicator since it is not a policy instrument *per se* and is less directly controlled by the central bank. While the monetary base and the short term interest rate are under more direct control, data for most countries were unavailable.

For the benchmark regression, we restrict our sample to observations with annual money growth rate and inflation of less than 100%.⁷ First, high inflation periods are usually accompanied by political turmoil in the affected countries and may lead to early elections. Sachs (1987), for example, cites the hyperinflation in Bolivia as the cause of early elections in 1985. By excluding these episodes, we hope to mitigate the possibility of reverse causality. Second, during high inflationary episodes, it is unlikely that politicians can achieve political gain by

⁵ See the appendix for a list of countries and data sources. Note that data on money growth for individual countries in the European Monetary Union do not exist after the formal adoption of the Euro.

⁶ This estimator improves on the GMM difference estimator developed in Arellano and Bond (1991). The implementation of this procedure in STATA using the `xtabond2` command is outlined in Roodman (2006). We use the “collapse” option of `xtabond2` to collapse the instrument set. Otherwise, the instrument count is quadratic in T, which is problematic with a sample period of 30 years.

⁷ Results are similar when we use a 50% cutoff.

further increasing the rate of money growth; the Phillips curve channel most likely is not present.⁸ Finally, if hyperinflations happen to occur during election years, these outliers could produce a significant coefficient on the election cycle variable, even though political monetary cycles are absent in most countries. We discuss the implications of including high money growth observations in the results section.

In our benchmark specification, we include the lag of the dependent variable as a regressor to account for possible smoothing in monetary policy. Results are similar when we include two lags.

2.2 Election Cycle Variable

We constructed a large database of election dates for the national leader (the president in a presidential system and the prime-minister in a parliamentary system). Our primary source is the International Institute for Democracy and Electoral Assistance (International IDEA) whose Voter Turnout Database lists the years of parliamentary and presidential elections.

The main criterion for including an election in their sample is that “there was a degree of competitiveness”; that is, “more than one party contested the election, or one party and independents contested the elections, or the election was only contested by independent candidates”. These elections are further categorized as free, partially free and not free based on the Political Rights and Civil Liberties indicators of Freedom House (range 1-7 with lower scores representing greater freedom) during these election years. Specifically, the IDEA database treats elections as free or partly free if the average of these two indices is less than or equal to 5. We exclude elections designated as “not free” from our sample since political monetary cycles are

⁸ Agenor, et al. (2000) suggest that inflation is countercyclical in many developing countries. The fiscal-financing channel, on the other hand, can still exist during hyperinflations; any election-related increase in government spending is most likely monetized.

not likely to be observed in countries with autocratic regimes. Autocrats have the power to control election results through other means and do not have to rely on manipulating monetary or fiscal policy.⁹

Our benchmark election cycle indicator variable, *ECI*, takes on a value between 0 and 1 for each year, depending on what fraction of that year is within an election cycle (Franzese, 2000). We consider the two years prior to an election day as the election cycle period associated with that election. The choice of two years accounts for the lag with which monetary policy affects the economy.¹⁰ For example, if an election is held on January 31st of 2003, then *ECI* is equal to 31/365 in 2003 (or 31/366 if 2003 were a leap year), equal to 1 in 2002, and equal to $1 - (31/365)$ in 2001.

If there are multiple elections in a given year (including run-off elections), then the period in-between the elections is also counted as part of the election cycle period, along with the two years prior to the *first* election of that year. Similarly, if there are overlaps between election cycles because of elections occurring within 2 years of each other, the whole period in-between is included in the election cycle.¹¹

A positive coefficient of *ECI* (in the absence of interaction terms) would indicate the presence of political monetary cycles. We consider alternative election cycle variables when we discuss robustness checks.

⁹ Even in democratic regimes, if the outcome of an election is fairly certain, there may not be an incentive to engage in expansionary fiscal or monetary policy. Since we lack data on how close the outcomes of these elections are or were expected to be beforehand, we abstract from this issue. Note, however, that by possibly including in our sample elections whose outcomes are certain, we stack the cards against finding evidence for political monetary cycles.

¹⁰ As a robustness test in Section 3, we also consider a one year election cycle period, *EC2*.

¹¹ Continuing our example, if there were an election on January 20th of 2005, then *ECI* would be equal to one in 2003, since the election in 2003 occurred within two years of the next election. Therefore all of 2003 is part of an election cycle. If on the other hand, the election in 2005 were held on February 20th, then *ECI* in 2003 would be equal to $(31/365) + (1 - (51/366))$, the first term reflecting the fraction from the election cycle associated with the election in 2003, and the second term reflecting the fraction from the election cycle associated with the election in 2005.

2.3 Central Bank Independence and Fixed Exchange Rate Regimes

For the measure of CBI, we consider the CBI indices in Cukierman et al. (1992), which are based on legal aspects of independence (*LEGAL*) and the turnover rate of central bank governors (*TURNOVER*).¹² The indices are generally available until 1989 for both advanced and developing economies and assume one value per decade. They range from 0 to 1, with higher values indicating greater CBI for the legal index and lower CBI for the turnover index.

The legal index was extended through 2002 for 24 Latin American and Caribbean countries by Jácome and Vázquez (2005), who also added a few new countries to the sample, and through 1999 for advanced countries by Siklos (2008). The turnover index was supplemented with data after 1990 from Crowe and Meade (2007) and Dreher, et al. (2008).

We also include a dummy variable, *FIX*, which takes on the value one when there is a fixed exchange rate and zero otherwise. The determination of the fixed exchange rate regime is based on the Reinhart and Rogoff (2004) *de facto* exchange rate regime indicator. A fixed exchange rate regime restricts the scope for independent monetary policy. We therefore expect fixed exchange rates to reduce overall money growth.

The level of CBI and a fixed exchange rate regime affect the likelihood of observing political monetary cycles. More independent central banks or central banks that operate fixed exchange rates are less likely to face, and if they do, more able to withstand political pressure during elections. Therefore we expect to find political monetary cycles only in countries with low levels of CBI and flexible exchange rates. To capture this idea, we include interaction terms

¹² *TURNOVER* is the average number of changes in the central bank governor per year in each decade. For example, If *TURNOVER* = 0.2, there are 2 changes per decade for an average tenure of 5 years.

of the election cycle variable with both *CBI* and *FIX* (Clark, et al., 1998, and Leertouwer and Maier, 2001).

2.4 The Role of Fiscal Policy and Borrowing Capacity

As argued in the introduction, there are two main channels through which political manipulation of monetary policy may operate during elections: the Phillips curve channel and the fiscal-financing channel. In our benchmark regression, we do not control for fiscal policy. Therefore, the coefficient on our election cycle variable captures the effects of both channels. The fiscal-financing channel should only be relevant for countries whose governments have limited ability to borrow. We therefore interact our election cycle variable, *EC*, with a measure of borrowing capacity, *BORCAP*, to capture this effect. We proxy borrowing capacity with the development of the financial sector, measured by the ratio of domestic credit to GDP. The data are from the World Bank's WDI. We also include *BORCAP* separately in the regression, apart from its interaction term with *EC*. Borrowing capacity affects the amount of pressure central banks may face from politicians during both election and non-election periods.

As a robustness check, we include a fiscal variable in our regression. To the extent to which including this variable controls for the fiscal-financing channel, comparing the effect of elections with and without this variable provides some information about the strength of the fiscal channel. The results are similar when the fiscal variable is included, suggesting that the fiscal channel is not strong, especially for the low inflation observations included in our sample. This is not surprising given the mixed evidence for political budget cycles in the literature.¹³ In a recent paper, Brender and Drazen (2005a) argue that political budget cycles are found only in

¹³ See for example Alesina et al. (1997), De Haan and Zelhorst (1990), Golden and Poterba (1980), Haynes and Stone (1988), and Shi and Svensson (2002a, b).

new democracies, where “fiscal manipulation may work because voters are inexperienced with electoral politics or may simply lack the information needed to evaluate fiscal manipulation that is produced in more established democracies”.¹⁴ They argue that the strong fiscal cycle in these countries accounts for the significant effect of the election variable in the larger sample of countries that includes the new democracies. The finding of a political budget cycle disappears when these countries are omitted from the sample.

We confirm the results of Brender and Drazen (2005a) using our sample and data and their regression equation.¹⁵ We too find political budget cycles in the whole sample but not when new democracies are excluded.¹⁶ To investigate a similar role of new democracies for political monetary cycles, we estimate equation (1) excluding new democracy observations as a robustness check. In addition, we examine the fiscal-financing channel of political monetary cycles in new democracies.

2.5 Output Gap and Inflation

We use the World Bank’s World Development Indicators (*WDI*) dataset to construct the output gap and inflation variables, *YGAP* and *INFL*. *YGAP* is the log difference between real GDP and its HP-filtered trend. Including these variables in the regression introduces a simultaneity problem as money growth affects output and inflation. In our GMM estimation,

¹⁴ Brender and Drazen (2005b) find that expansionary fiscal policy does not increase the probability of reelection in all groups of countries including new democracies. However, this does not preclude the possibility that politicians attempt to use fiscal policy to get reelected, at least in new democracies, as the results in Brender and Drazen (2005a) suggest.

¹⁵ The control variables included in their regression are the lag of budget balance, trade to GDP ratio, working age population, population age 65 and above, and GDP per capita.

¹⁶ The results are available upon request.

YGAP and *INFL*, along with the lag of the dependent variable, are instrumented using lags of differences and levels of these variables.¹⁷

These variables are primarily included to control for Taylor rule-type monetary policies that central banks may follow.¹⁸ In addition, omitting *YGAP* can bias the coefficient of *EC* since the timing of elections is not necessarily exogenous. In most parliamentary democracies, elections can be called at any time prior to the usual schedule, and it is plausible that this is more likely to occur when the economy is doing well. To test whether election dates are in fact related to economic performance, we regressed a dummy variable indicating an election year on the lagged output gap and lagged inflation. We found little evidence, however, that these variables determine the timing of elections, confirming the results of Alesina and Roubini (1997) and Botchen and Weiss (2003).

2.6 Summary Statistics

Summary statistics for all observations, advanced countries, and developing countries are provided in Table 1. The sample period is 1972-2001 and includes 63 countries; 25 are advanced economies, and 38 are developing countries. Advanced countries comprise 41% of all observations. There are 393 elections in the sample, 180 of which took place in advanced economies. These numbers include multiple elections in the same year. Both advanced and developing economies had a fixed exchange rate regime roughly 20% of the time. Developing

¹⁷ As a robustness test, we estimate a fixed effects regression in Section 3. In this case, we include the lags of *YGAP* and *INFL* in the regression to reduce simultaneity, although the fact that monetary policy affects output and inflation with a lag already mitigates this concern. However, including the lags rather than the current variables does not entirely eliminate the simultaneity problem, since past inflation might have been caused by expectations of current inflation.

¹⁸ It is likely that including *YGAP* in our regression does not shut off the Phillips curve channel since elections affect money growth which in turn affects output, although we cannot be certain given the reduced form nature of the estimation. In contrast, we exclude the budget balance variable since it is possible that including it would shut off the fiscal-financing channel (to the extent that including this variable controls for the fiscal channel). The reason is that here, elections affect money growth through their effect on fiscal policy.

countries have higher central bank governor turnover rates, although surprisingly, they have slightly higher average levels of legal CBI. This highlights the fact that the legal measure is unlikely to accurately reflect CBI in developing economies, as Cukierman, et al. (1992) point out. Developing economies have higher median money growth rates, inflation rates, and output growth rates. They have also experienced hyperinflationary episodes as the maximum values of money growth and inflation reveal. Domestic credit as a percent of GDP is much higher in advanced countries, reflecting more developed financial markets. Finally, developing countries have slightly larger government budget deficits on average.

The top panel of Table 2 presents the summary statistics broken up by type of country and then further by election periods vs. non-election periods, defined using *ECI*. Because the macro data are annual, while any given year can be comprised of both election and non-election periods (*ECI* is a continuous variable between 0 and 1), we define a year to be within an election period if $ECI > 0.50$. This is roughly the average value for *ECI* in the sample. In contrast to Table 1, we limit observations to those with money growth and inflation less than 100%, as in our base regression. In advanced economies, the mean growth rate of M1 does not differ considerably between election periods and non-election periods. Output growth is slightly lower during elections. For developing countries, the average rate of output growth increases by 0.53 percentage points during election periods, indicating the presence of weak political business cycles on average. In addition, money growth does not increase during elections. Thus the top panel of Table 2 suggests that there is no political monetary cycle for the average developing country.

In the bottom panel, we separate developing countries by CBI using *TURNOVER* as a proxy. In particular, we present summary statistics for *TURNOVER* below and above 0.31. As

described in the results section, $TURNOVER = 0.31$ roughly corresponds to the cutoff level for which political monetary cycles are significant at the 10% level. For observations with $TURNOVER \geq 0.31$ (slightly greater than the mean of 0.26 and the median of 0.20), money growth is roughly 2.6 percentage points higher during elections relative to non-election periods.¹⁹ We also find a 2.1 percentage point increase in mean output growth during elections for these countries. For low $TURNOVER$ observations, however, money growth and output growth barely differ during election vs. non-election periods. Thus, there is preliminary evidence of both political business cycles and political monetary cycles for developing countries but only those with low CBI.

Table 3 presents the sample correlations for the full sample, advanced countries, and developing countries. Looking at the simple correlations, there does not appear to be a relationship between election cycles and money growth for either advanced or developing countries. As we show in Section 3, however, when we control for other variables and consider the role of CBI, we find that political monetary cycles do exist in developing countries with low levels of CBI. As expected, the simple correlation between money growth and inflation is large for both advanced and developing countries. For developing countries, the sign of the correlation between the two measures of CBI, $TURNOVER$ and $LEGAL$, is unexpectedly positive. For advanced countries, the sign of the correlation between money growth and $TURNOVER$ is unexpectedly negative. As discussed in Cukierman, et al. (1992), $LEGAL$ is not a good measure of CBI for developing countries, while $TURNOVER$ is not indicative of CBI in advanced nations. We return to this issue in the results section.

¹⁹ We use the $TURNOVER$ measure of CBI rather than the $LEGAL$ measure to divide developing countries since the former is likely to be a better measure of CBI for developing nations.

Table 4 presents the summary statistics for developing countries broken up by old and new democracies (as defined by Brender and Drazen, 2005a) and by election and non-election periods. For both old and new democracies, average and median budget deficits are larger during election periods, suggesting the presence of political budget cycles in both. As mentioned earlier, however, after including the Brender and Drazen (2005a) control variables, we only find a significant effect of elections on fiscal policy in new democracies. Surprisingly, Table 4 shows a reduction in average and median money growth during elections in new democracies. In the next section, however, we show that there is no significant change in money growth after including the control variables.

3. Results on Political Monetary Cycles

Table 5 presents results from the estimation of equation (1). As mentioned above, we limit observations of money growth and inflation to less than 100%, thus dropping the outliers from Table 1. This constraint is not binding for advanced economies. We present results for the full sample, advanced countries, and developing nations using either *LEGAL* or *TURNOVER* as the measure of CBI.

To test for political monetary cycles in the *typical* economy, we use the coefficients of *ECI* and its interaction terms along with the mean values of the interaction variables. For the full sample in specification (1), the results imply that political monetary cycles do not exist on average, regardless of legal independence. In addition, although the coefficient of *ECI*LEGAL* has the predicted sign, implying that countries with more legally independent central banks have less severe political monetary cycles all else equal, this effect is not significant.

For the full sample in specification (2), however, the coefficient of $EC1*TURNOVER$ is positive and significant, implying that countries with higher levels of central bank governor turnover experience more severe political monetary cycles. In particular, during elections cycles, M1 growth increases by 10.3 percentage points more in countries with the highest levels of $TURNOVER$ relative to those with the lowest, *all else equal*. Further calculations reveal that for the typical economy, the effect of elections is significantly positive but only for countries with $TURNOVER$ greater than or equal to 0.32, slightly greater than the average. The size of the predicted impact on money growth when $TURNOVER$ equals 0.32 is approximately 1.4 percentage points (the VAR analysis in Christiano, et al. (1999) suggests that for the U.S., a one percentage point increase in M1 growth is associated with an increase in output of 1.5 percentage points and a 0.6 percentage point increase in inflation after 8 quarters, although these responses could differ in other countries). This effect increases as $TURNOVER$ increases to a maximum of 8.4 percentage points for a country with $TURNOVER$ equal to one. Thus the effect of elections on the severity of political monetary cycles in the full sample depends on the level of CBI. This effect is evident, however, only when we use $TURNOVER$ as our CBI measure. We will explain shortly why this is the case.

For advanced countries, there is no evidence of political monetary cycles for all values of $LEGAL$ (specification (3) and $TURNOVER$ (specification (4)). For developing countries (specification (5)), elections do not significantly increase money growth for any value of $LEGAL$. In addition, the coefficient of $EC*LEGAL$ is insignificant, implying that all else equal, legal independence does not affect political monetary cycles. The results in specification (6), however, imply that during elections cycles, M1 growth increases by 15.1 percentage points more in countries with the highest levels of $TURNOVER$ relative to those with the lowest, *all*

else equal. For the typical developing economy, the effect of elections is significantly positive at the 10% (5%) level but only for countries with *TURNOVER* greater than or equal to 0.30 (0.34), slightly greater than the average. This is depicted in Figure 1, which presents the marginal effect of elections on money growth in developing countries for different values of *TURNOVER* and the 90% confidence interval. Thus a significant number of developing countries have experienced political monetary cycles. The size of the predicted impact on money growth when *TURNOVER* equals 0.30 is 1.6 percentage points. This effect grows as *TURNOVER* increases to a maximum of 12.2 percentage points when *TURNOVER* equals one. The coefficients of *TURNOVER* and *ECI*TURNOVER* are jointly significant as is their sum.

As mentioned above, we find evidence of political monetary cycles in the full sample when we use *TURNOVER* as our CBI variable, but not when we use *LEGAL*. *LEGAL* has no effect for advanced countries, which for the most part do not experience these cycles, It also has no effect for developing countries, even though many do experience cycles, presumably because legal measures of independence are less accurate in developing countries (Cukierman, et al., 1992). Meanwhile, *TURNOVER* has a strong effect in developing countries but a smaller and statistically weaker (although still significant) effect in the full sample. The drop in size and significance for the full sample can be explained by the fact that some advanced countries have higher turnover rates than developing countries, but still do not experience political monetary cycles. Cukierman, et al. (1992) argue that this measure is less indicative of CBI for advanced countries than the legal measure. In the full sample, the effect of *TURNOVER*, although weaker, is still significant. The reason is that advanced countries have lower turnover rates on average

and much weaker political monetary cycles than developing countries, while developing countries with high turnover rates experience larger cycles than those with low turnover rates.²⁰

3.1 Results for the Other Variables

The coefficient of *ECI*FIX* has the predicted negative sign and is significant but only in specification (3) for advanced countries. The coefficient of *ECI*BORCAP* is insignificant in all specifications.

The coefficient of *LEGAL* by itself has the expected negative sign but is insignificant, while the coefficient of *TURNOVER* has the wrong sign for the full sample and developing countries. One explanation is that part of the effect of the CBI variables might be captured by lagged money growth or inflation. Removing both variables increases the size and significance of *LEGAL* and *TURNOVER* (and gives *TURNOVER* the correct sign). *LEGAL*, however, remains insignificant while *TURNOVER* becomes significant for developing countries.

The coefficient of *FIX* is negative as expected and close to significant at the 10% level for developing countries in specification (6). Again the effect could be partially captured by lagged money growth and inflation. If we remove these variables, the coefficient of *FIX* equals -9.6 and is significant at the 1% level for developing countries.

The coefficient of *OUTPUTGAP* has the predicted negative sign in all specifications and is significant in specifications (3) and (4), while the coefficient of *INFL* is unexpectedly positive and significant. The latter result holds even after excluding *LEGAL*, *TURNOVER*, *FIX*, or *M(-1)*. This is most likely due to the fact that both money growth and inflation are fairly persistent and

²⁰ This explanation is similar to the one provided in Cukierman, et al. (1992) for why *TURNOVER* is significant in explaining average inflation for developing countries and for the whole sample, even though it is not significant for industrial countries.

highly correlated with each other. If we exclude *INFL* from the regression, the results for political monetary cycles are virtually identical.

The coefficient of *BORCAP* has the predicted negative and significant effect for advanced countries and for the full sample but not for developing countries. This could result from greater variation in the development of financial markets among advanced economies.

3.2 Developing Countries in More Detail

In Table 6, we look more closely at the results for developing economies. We show results with and without restricting money growth observations to less than 100%. In addition, we present regressions without the interaction terms and control variables. We limit attention to *TURNOVER* as a measure of CBI for developing countries since, as previously mentioned, we find no evidence of political monetary cycles using *LEGAL*. The most likely explanation is that legal measures are unlikely to accurately reflect CBI for developing countries.

With the election cycle variable as the sole regressor (apart from lagged money growth), we find evidence of election cycles only when we do not restrict money growth to less than 100% (specification (2)). The large coefficient of *ECI* is mostly likely due to reverse causality from high inflation episodes to elections. In fact, if we restrict money growth to less than 2000%, the coefficient falls to 9.0 with a p-value of 0.20. If we further restrict money growth to less than 1000%, the coefficient falls to 0.30 with a p-value of 0.88, results that are very similar to column (1).

When we add the control variables to the regression that includes the outliers (specification (4)), this effect disappears. This is not because of the control variables themselves, but rather because of the reduction in sample size. Specifically, including *TURNOVER*

eliminates some of the highest money growth episodes in the sample since *TURNOVER* data are not available for these observations. In fact, if we estimate specification (2) but limit the sample to data on *TURNOVER*, the coefficient of *ECI* becomes insignificant. Therefore, the sub-sample of countries and observations with data on *TURNOVER* do not experience these cycles on average, regardless of whether money growth is restricted or not.

However, as shown in column (5) (which reproduces column (6) from Table 5), the impact of elections for these countries does depend on the level of *TURNOVER*. The restriction on money growth and inflation has reduced the sample size by 78 observations out of 791. In column (6), however, the coefficient of *ECI*TURNOVER* has the right sign but is insignificant. The fall in significance when we drop the restriction on money growth can be explained by the presence of just two outliers. The only observations in this regression of money growth greater than 4000% are Nicaragua in 1988 and Argentina in 1989. Restricting money growth to less than 4000% yields similar results to column (5), as shown in column (7).

3.3 Robustness Tests

We conduct several tests to check the robustness of our results. First we include year dummies, two lags of money growth instead of one lag, and both measures of CBI at the same time. We restrict money growth and inflation to less than 50% as opposed to 100%. We filter the data using the measure for competitive elections developed in Przeworski et al. (2000) as opposed to the Freedom House variable.²¹ None of these modifications change the results significantly. Second, we include only one year before the election day as the election cycle

²¹ The authors define democracies as polities in which there are contested elections, i.e. there is some chance that the incumbent executive or party in the legislature is replaced. We therefore limit the regression to democracies. Although the sample size is smaller using this indicator as opposed to the Freedom House variable, the results are similar and are available upon request.

indicator (*EC2*), as opposed to two years. The results are presented in Table 7. In this case, we do not find evidence of political monetary cycles in both advanced and developing countries, although the coefficient of *EC2*TURNOVER* has the predicted positive sign. This is not entirely surprising given that we may not be capturing the full lag in monetary policy. The VAR analysis in Christiano, et al.(1999) indicates that the peak in the response of output to a monetary policy shock is at least a year and often six quarters or more. If monetary policy becomes more expansionary two years prior to an election, including only one of the years as part of the election cycle is likely to result in monetary policy that looks similar between election and non-election periods.

Third, we estimate the model using fixed effects estimation as opposed to GMM, although in the presence of a lagged dependent variable the fixed effects estimator does not eliminate dynamic panel bias. While this bias is insignificant for large T, Judson and Owen (1999) in simulations find a bias of 20% in the coefficient estimate even when T=30, the maximum number of observations per country in our sample. Moreover, a number of countries have fewer observations, especially new democracies that have had competitive elections for a shorter period of time. Results are similar and are available upon request.²²

As a fourth robustness test, we include *BUDGET*, the budget surplus as a percent of GDP, in our regression. To the extent to which including *BUDGET* controls for the fiscal-financing channel, comparing the effect of elections with and without this variable provides some information about the strength of the fiscal channel. Again, we do not find evidence of political monetary cycles in advanced countries, and so we only present results for developing countries. Table 8 presents the results. In columns (1) and (2), we look at the full sample of

²² We use lags of the output gap and inflation to mitigate simultaneity problems when we do fixed effects estimation.

developing countries. We continue to limit observations to money growth and inflation of less than 100%. In column (1) we show the default specification that does not include fiscal policy. Therefore column (1) presents identical results to column (6) in Table 5. In column (2), we remove *BORCAP* and *ECI*BORCAP* since we include *BUDGET*.²³

A comparison of the columns suggests that controlling for fiscal policy weakens the effect of elections only slightly. However, this is due solely to the drop in sample size and not the inclusion of the fiscal variable itself. Specifically, performing the regression in column (1) but limiting to the observations in column (2) yields similar results to those presented in column (2). This suggests that the fiscal channel is not strong. In addition, the effect of elections is significantly positive at the 10% level for countries with *TURNOVER* greater than 0.40. The fact that elections still have an impact after controlling for fiscal policy provides some evidence that elections influence monetary policy through the Phillips curve channel.

We also look at the effect of fiscal policy on political monetary cycles in old and new democracy observations. Brender and Drazen (2005a) define new democracy observations as the period that includes the first four competitive elections after a country becomes democratic. If, as Brender and Drazen (2005a) find and we confirm, political budget cycles occur only in new democracies, then controlling for fiscal policy should reduce the strength of political monetary cycles for these countries, but only if the expansionary fiscal policy is monetized. In contrast, the results for old democracies should be unchanged. In columns (3) and (4), we again look at developing countries but limit attention to those that are also in the Brender and Drazen (2005) sample. This reduces the sample size falls from 38 countries to 27. The new and old democracy

²³ The coefficient of *BUDGET* is theoretically ambiguous. Larger budget surpluses require less debt monetization, resulting in a negative coefficient. On the other hand, a central bank might respond to tighter fiscal policy with looser monetary policy, yielding a positive coefficient. Table 8 shows a coefficient that is not significantly different from zero. In addition, if we include the interaction *BUDGET*TURNOVER*, we do not find that more independent central banks react to fiscal expansions with more contractionary monetary policy than dependent central banks.

observations are drawn from this group. Again, the weaker political monetary cycle in column (4) vs. column (3) is a result of the smaller sample size, not the inclusion of *BUDGET*.

For new democracy observations, we find no evidence of political monetary cycles even before controlling for fiscal policy. This suggests that their election-related fiscal expansions are not monetized and/or that expansionary monetary policy is not needed when fiscal policy is used instead. For old democracies, the political monetary cycle is a little weaker when we control for fiscal policy in column (8), although again this is a result of the drop in sample size and not the inclusion of the fiscal variable itself.²⁴ Therefore, controlling for fiscal policy does not impact political monetary cycles in old democracies, a result that is consistent with the absence of political budget cycles in old democracies.

Finally, we consider an additional channel through which elections might affect monetary policy. If elections cause a rise in political risk in some countries, the central bank might respond by raising interest rates to prevent capital flight. We therefore interact *ECI* with a measure of political risk. We use the average of Government Stability, Internal Conflict, External Conflict, and Ethnic Tensions from the International Country Risk Guide. Results were similar. In addition, eliminating the sudden stop episodes listed in Honig (2008) does not affect the results.

²⁴ For the mean old democracy observation in column (7), the effect of elections is close to, but never significant at the 10% level. For example, for countries with *TURNOVER* greater than 0.45, the p-value for the effect of elections is 0.15. Note that even though we do not find a significant impact of elections at the 10% level for both new and old democracies, the impact in the combined sample can still be significant if there are differences between the samples.

4. Conclusion

Our first goal in this paper is to expand the analysis of election-induced monetary cycles. Existing research has not come to a definitive conclusion on the existence of political monetary cycles in advanced economies. Our results are consistent with studies that have not found a significant effect. Moreover, we add to the literature by analyzing the role of CBI in developing economies, where these cycles are more likely to occur. We find evidence of political monetary cycles in developing countries with low levels of CBI. The findings in this paper, therefore, underscore the importance of CBI.

Second, we attempt to identify the channels through which elections impact monetary policy. We find some evidence that political monetary cycles are not caused by monetization of election-related fiscal expansions, even in new democracies where political budget cycles exist. This suggests that the Phillips curve channel, i.e. pressure on the central bank to stimulate the economy in order to increase the probability of re-election, may be an important factor in generating political monetary cycles.

References

- Abrams, B. A., and P. Iossifov, 2006, "Does the Fed Contribute to a Political Business Cycle?" *Public Choice*, 129:3, 249-262.
- Agenor, Pierre-Richard, McDermott, C. John, and Prasad, Eswar S. 2000. "Macroeconomic Fluctuations in Developing Countries: Some Stylized Facts." *World Bank Economic Review*, 14(2) p. 251-85.
- Alesina, A., and N. Roubini, 1992, "Political Cycles in OECD Economies," *Review of Economic Studies*, 59:4, 663-88.
- Alesina, A., Roubini, N., Cohen, G., 1997. Political cycles and the macroeconomy. MIT Press, Cambridge, MA.
- Arellano, M., Bond, S., 1991. Some tests of specification for panel data: Monte Carlo evidence and an application to employment equations. *Review of Economic Studies* 58, 277–297.
- Arellano, M. and O. Bover. 1995. Another look at the instrumental variable estimation of error-components models. *Journal of Econometrics* 68: 29-51.

- Arnone, Marco, Bernard J. Laurens, Jean-François Segalotto, and Martin Sommer. (2007) "Central Bank Autonomy: Lessons from Global Trends." IMF Working Paper 07/88.
- Beck, Nathaniel, 1987, "Elections and the Fed: Is there a Political Business Cycle," *American Journal of Political Science*, 31(1), pp. 194–216.
- Blundell, R., and S. Bond. 1998. Initial conditions and moment restrictions in dynamic panel data models. *Journal of Econometrics* 87: 115-43.
- Boschen, J. and C. Weise. 2003. "What Starts Inflation: Evidence from the OECD Countries." *Journal of Money, Credit and Banking*. 35(3): 323-349.
- Brender, A. and A. Drazen. 2005a. "Political budget cycles in new versus established democracies," *Journal of Monetary Economics* 52: 1271–1295.
- _____. 2005b. "How Do Budget Deficits and Economic Growth Affect Reelection Prospects? Evidence from a Large Cross-Section of Countries." *NBER Working Papers*, 11862.
- Christiano, L., Eichenbaum, M. and C.L. Evans (1999), "Monetary Policy Shocks: What Have we Learned and to What End?" in J.B. Taylor and M. Woodford (eds), *Handbook of Macroeconomics*, Volume 1A. pp. 65-148. Amsterdam: Elsevier Science B.V.
- Clark, W.R., U.N. Reichert, S.L. Lomas and K.L. Parker. 1998. "International and domestic constraints on political business cycles in OECD economies," *International Organization* 51, pp. 87–120.
- Crowe, C., and E. Meade. 2007. "The Evolution of Central Bank Governance around the World," *Journal of Economic Perspectives*, 21:4, 69-90.
- Cukierman, A. and A. H. Meltzer (1986). "A Theory of Ambiguity, Credibility, and Inflation under Discretion and Asymmetric Information," *Econometrica*, 54(5), pp. 1099-1128.
- Cukierman, A., S. Webb, and B. Neyapti. 1992. "Measuring The Independence of Central Banks and Its Effect on Policy Outcomes." *World Bank Economic Review*, 6(3): 353–98.
- Drazen, A. and M. Eslava (2007), "Electoral Manipulation via Voter-Friendly Spending: Theory and Evidence," working paper. (Earlier version circulated as NBER Working Paper 11085).
- de Haan, J., and D. Zelhorst. 1990. "The impact of government deficits on money growth in developing countries," *Journal of International Money and Finance*, 9(4): 455-469.
- Dreher, A., Sturm, J.E., and J. de Haan. 2008. Does high inflation cause central bankers to lose their job? Evidence based on a new data set. *European Journal of Political Economy* 24: 778–787.
- Dreher, A., and R. Vaubel. 2004. "Do IMF and IBRD Cause Moral Hazard and Political Business Cycles? Evidence from Panel Data." *Open economies review* 15: 5–22.
- Fair, Ray, 1978, "The Effect of Economic Events on Votes for President," *The Review of Economics and Statistics*, 2:159-173.
- _____. 1982. "The Effect of Economic Events on Votes for President: 1980 Results," *The Review of Economics and Statistics*, May 1982, 322-325.
- Fischer, S. 1977. "Long-Term Contracts, Rational Expectations, and the Optimal Money Supply Rule." *Journal of Political Economy*, 85 (1): 191-205.
- Franzese, R., 2000. Electoral and Partisan Manipulation of Public Debt in Developed Democracies, 1956-1990. In R. Strauch and J. von Hagen (eds), *Institutions and Fiscal Policy*, pp. 61-83. Dordrecht: Kluwer academic press.
- Golden, D. G. and J. M. Poterba, 1980, "The Price of Popularity: The Political Business Cycle Re-examined," *American Journal of Political Science*, 24: 696-714.

- Grier, K. B., 1987, "Presidential Elections and Federal Reserve Policy: An Empirical Test," *Southern Economic Journal*, 54: 475-96.
- Haynes, S. E., and J. A. Stone, 1988, "Does the political business cycle dominate United States unemployment and inflation? In T. Willett, ed., *Political business cycles: The economics and politics of stagflation*. San Francisco: Pacific Institute.
- _____. 1989. "An Integrated Test for Electoral Cycles in the U.S. Economy," *Review of Economics and Statistics*, 71:3, 426-34.
- Honig, A. 2008. "Do Improvements in Government Quality Necessarily Reduce the Incidence of Costly Sudden Stops?" *Journal of Banking & Finance*, 32:3, 360-373.
- Jácome, L. I., and F. Vázquez. 2005. "Any Link between Legal Central Bank Independence and Inflation? Evidence from Latin America and the Caribbean." IMF Working paper 75.
- Judson, R.A., and A.L. Owen. 1999. Estimating dynamic panel models: A practical guide for macroeconomists *Economics Letters* 65: 9-15.
- Leertouwer, Erik, and Maier, Philipp, 2001, "Who Creates Political Business Cycles: Should Central Banks Be Blamed?" *European Journal of Political Economy*, 17(3), pp. 445-463.
- Lucas, Robert E., Jr. 1972. "Expectations and the Neutrality of Money." *Journal of Economic Theory*, 4, 103-124.
- McCallum, B. T., 1978, "The Political Business Cycle: An Empirical Test," *Southern Economic Journal*, 44, 504-515.
- Nordhaus, W.D., 1975, The political business cycle. *Review of Economic Studies* 42, 169–190.
- Persson, T., Tabellini, G., 2003. *The Economic Effect of Constitutions: What do the Data Say?* MIT Press, Cambridge, MA.
- Phelps, E. S. and J. B. Taylor, 1977. "Stabilizing Powers of Monetary Policy under Rational Expectations," *Journal of Political Economy*, 85 (1), 163-190.
- Przeworski, A., Alvarez, M., Cheibub, J. and Limongi, F. (2000). *Democracy and Development: Political Regimes and Economic Well-being in the World, 1950-1990*, New York: Cambridge University Press. *Democracy and Development Extended Data Set*.
- Reinhart, C. and K. Rogoff, 2004, The Modern History of Exchange Rate Arrangements: A Reinterpretation?, *Quarterly Journal of Economics* 119, 1-48.
- Roodman, D. 2006. How to Do xtabond2: An Introduction to "Difference" and "System" GMM in Stata. Working Paper 103. Center for Global Development, Washington.
- Sachs, Jeffrey. (1987) "The Bolivian Hyperinflation and Stabilization." *American Economic Review*, 77:2, 279-83.
- Sargent T. (1973). "Rational Expectations, the Real Rate of Interest, and the Natural Rate of Unemployment," *Brookings Papers on Economic Activity*, 1973 (2), 429–72. Correction in BPEA, 1973 (3).
- Sargent T. and N. Wallace (1975). "Rational Expectations, the Optimal Monetary Instrument, and the Optimal Money Supply Rule," *Journal of Political Economy*, 83 (2), 241–254.
- Shi, M., Svensson, J., 2002a. Conditional political budget cycles, CEPR Discussion Paper #3352.
- _____. (2002b). Political business cycles in developed and developing countries, working paper, IIES, Stockholm University.
- Siklos, P., 2008. No Single Definition of Central Bank Independence is Right for All Countries. Paolo Baffi Centre Research Paper No. 2008-02

Table 1: Summary Statistics

All Observations	obs.	mean	median	s.d.	min.	max.
<i>M</i>	1,431	53.73	14.01	431.47	-29.62	11,673.40
# elections	1,431	0.27	0	0.51	0	2
<i>ECI</i>	1,431	0.48	0	0.42	0	1
<i>LEGAL</i>	1,144	0.41	0.40	0.17	0.09	0.86
<i>TURNOVER</i>	1,146	0.22	0.20	0.18	0.00	1.00
<i>FIX</i>	1,431	0.19	0	0.39	0	1
<i>YGAP</i>	1,431	0.08	-0.04	3.54	-18.59	17.81
<i>GROWTH</i>	1,431	3.55	3.56	3.82	-26.48	19.56
<i>INFL</i>	1,430	55.72	8.66	453.78	-7.63	10,205.03
<i>BORCAP</i>	1,431	70.46	58.70	48.96	-72.99	311.42
<i>BUDGET</i>	1,330	-3.19	-2.83	4.76	-26.74	22.63
Advanced Countries						
<i>M</i>	593	11.83	9.86	9.74	-15.93	78.17
# elections	593	0.30	0	0.50	0	2
<i>ECI</i>	593	0.55	1	0.40	0	1
<i>LEGAL</i>	593	0.39	0.34	0.16	0.09	0.73
<i>TURNOVER</i>	488	0.16	0.13	0.12	0.00	0.60
<i>FIX</i>	593	0.21	0	0.41	0	1
<i>YGAP</i>	593	0.00	-0.13	2.55	-8.11	8.66
<i>GROWTH</i>	593	3.12	3.03	2.69	-7.28	13.44
<i>INFL</i>	592	7.85	5.63	8.22	-1.84	84.22
<i>BORCAP</i>	593	90.87	86.60	47.13	14.91	311.42
<i>BUDGET</i>	585	-3.35	-2.61	3.87	-15.75	9.12
Developing Countries						
<i>M</i>	788	87.96	18.95	579.29	-29.62	11,673.40
# elections	788	0.25	0	0.52	0	2
<i>ECI</i>	788	0.42	0	0.43	0	1
<i>LEGAL</i>	563	0.43	0.43	0.17	0.14	0.86
<i>TURNOVER</i>	619	0.26	0.20	0.21	0.00	1.00
<i>FIX</i>	788	0.17	0	0.38	0	1
<i>YGAP</i>	788	0.11	-0.03	4.17	-18.59	17.81
<i>GROWTH</i>	788	3.87	4.21	4.45	-26.48	19.56
<i>INFL</i>	788	94.87	11.75	608.62	-7.63	10,205.03
<i>BORCAP</i>	788	52.91	41.47	42.40	-72.99	264.17
<i>BUDGET</i>	706	-3.29	-3.00	4.98	-26.74	22.63

NOTES:

1. Unless otherwise noted, all data are annual. Growth rates and ratios are expressed in percentage terms. The sample period is 1972 to 2001. There are 25 advanced countries and 38 developing countries (63 total).

2. Variable definitions: *M* = % growth in M1, # elections = the number of election days in a given year (1 for one election day and 2 for 2 election days, i.e. multiple elections), *ECI* = the default two-year election cycle indicator. *LEGAL* = legal measure of CBI (one number per decade with higher numbers indicating greater independence), *TURNOVER* = average number of changes in the central bank governor per year in each decade (one number per decade with higher numbers indicating less CBI). *FIX* = 1 for fixed exchange rate regime and 0 otherwise (*de facto* measure). *YGAP* = log difference between real GDP and its HP-filtered trend. *GROWTH* = % growth in real GDP. *INFL* = CPI inflation rate, *BORCAP* = borrowing capacity of the government measured by domestic credit to the private sector as a % of GDP, *BUDGET* = government budget surplus as a % of GDP.

Table 2: Summary Statistics by country type (Advanced vs. Developing), election period, and CBI

Advanced - 25 Countries		obs.	mean	median	s.d.	min.	max.
Non-Election Period	<i>M</i>	259	11.68	9.81	10.10	-15.93	78.17
	<i>YGAP</i>	259	0.15	-0.01	2.58	-8.02	6.96
	<i>GROWTH</i>	259	3.43	3.20	2.71	-2.92	12.03
	<i>INFL</i>	259	7.56	4.91	8.83	-1.39	84.22
	<i>BUDGET</i>	256	-2.93	-2.43	3.55	-13.70	9.12
Election Period	<i>M</i>	333	11.96	9.94	9.47	-2.48	65.65
	<i>YGAP</i>	333	-0.12	-0.18	2.53	-8.11	8.66
	<i>GROWTH</i>	333	2.88	2.84	2.65	-7.28	13.44
	<i>INFL</i>	333	8.07	5.88	7.73	-1.84	51.03
	<i>BUDGET</i>	328	-3.69	-2.83	4.09	-15.75	6.19
Developing - 38 Countries		obs.	mean	median	s.d.	min.	max.
Non-Election Period	<i>M</i>	404	20.70	17.68	17.11	-24.82	96.23
	<i>YGAP</i>	404	0.14	-0.20	4.31	-18.59	17.81
	<i>GROWTH</i>	404	3.93	4.33	4.41	-26.48	17.08
	<i>INFL</i>	404	17.16	11.14	17.51	-0.94	97.00
	<i>BUDGET</i>	364	-2.84	-2.72	4.80	-23.76	22.63
Election Period	<i>M</i>	309	21.30	17.38	17.96	-29.62	94.67
	<i>YGAP</i>	309	0.37	0.30	3.66	-11.19	11.57
	<i>GROWTH</i>	309	4.46	4.46	3.98	-10.27	19.56
	<i>INFL</i>	309	16.97	10.61	17.33	-7.63	98.93
	<i>BUDGET</i>	279	-3.35	-3.10	4.67	-19.97	21.97
Developing, <i>TURNOVER</i> < 0.31 (high CBI) - 34 countries ⁴		obs.	mean	median	s.d.	min.	max.
Non-Election Period	<i>M</i>	311	20.25	17.40	15.69	-8.94	94.90
	<i>YGAP</i>	311	0.33	-0.11	3.81	-9.90	12.75
	<i>GROWTH</i>	311	4.07	4.40	4.01	-13.13	17.08
	<i>INFL</i>	311	15.34	10.63	14.85	-0.88	86.23
	<i>BUDGET</i>	288	-3.43	-3.27	4.53	-23.76	18.27
Election Period	<i>M</i>	234	20.17	17.20	16.60	-29.62	94.67
	<i>YGAP</i>	234	0.14	0.29	3.54	-11.19	10.83
	<i>GROWTH</i>	234	4.10	4.35	3.63	-10.27	13.66
	<i>INFL</i>	234	16.28	10.06	17.44	-0.24	98.93
	<i>BUDGET</i>	219	-4.19	-3.75	4.30	-19.97	9.63
Developing, <i>TURNOVER</i> >= 0.31 (low CBI) - 17 countries		obs.	mean	median	s.d.	min.	max.
Non-Election Period	<i>M</i>	93	22.19	18.57	21.20	-24.82	96.23
	<i>YGAP</i>	93	-0.52	-0.28	5.62	-18.59	17.81
	<i>GROWTH</i>	93	3.46	3.91	5.56	-26.48	12.18
	<i>INFL</i>	93	23.25	13.43	23.47	-0.94	97.00
	<i>BUDGET</i>	76	-0.60	-1.57	5.15	-7.48	22.63
Election Period	<i>M</i>	75	24.81	22.13	21.41	-20.13	83.53
	<i>YGAP</i>	75	1.07	0.44	3.94	-6.46	11.57
	<i>GROWTH</i>	75	5.58	4.97	4.78	-4.41	19.56
	<i>INFL</i>	75	19.11	13.32	16.92	-7.63	66.69
	<i>BUDGET</i>	60	-0.27	-0.87	4.72	-8.10	21.97

NOTES:

1. See the notes in Table 1.
2. *M* and *INFL* restricted to <100%.
3. A year is defined to be within an election period if *ECI* > 0.50.
4. There are 34 countries that at one point during the sample had *TURNOVER* < 0.31. Similarly, there are 17 countries that at one point had *TURNOVER* >= 0.31. The union of these two groups is 38 countries.

Table 3 - Correlation Matrix

All Observations									
	<i>M</i>	<i>ECI</i>	<i>LEGAL</i>	<i>TURNOVER</i>	<i>FIX</i>	<i>YGAP</i>	<i>GROWTH</i>	<i>INFL</i>	<i>BORCAP</i>
<i>ECI</i>	-0.03								
<i>LEGAL</i>	-0.03	0.01							
<i>TURNOVER</i>	0.10	0.01	0.11						
<i>FIX</i>	-0.12	-0.03	-0.12	0.02					
<i>YGAP</i>	-0.01	0.00	0.01	0.01	0.03				
<i>GROWTH</i>	0.11	-0.01	-0.09	0.07	0.06	0.29			
<i>INFL</i>	0.62	-0.04	-0.03	0.21	-0.16	-0.01	-0.15		
<i>BORCAP</i>	-0.30	0.04	0.01	-0.16	-0.02	-0.06	-0.16	-0.33	
<i>BUDGET</i>	-0.06	-0.07	0.21	0.13	-0.07	0.07	0.20	-0.11	-0.11
Advanced									
	<i>M</i>	<i>ECI</i>	<i>LEGAL</i>	<i>TURNOVER</i>	<i>FIX</i>	<i>YGAP</i>	<i>GROWTH</i>	<i>INFL</i>	<i>BORCAP</i>
<i>ECI</i>	-0.01								
<i>LEGAL</i>	-0.15	-0.01							
<i>TURNOVER</i>	-0.09	0.00	-0.08						
<i>FIX</i>	-0.11	-0.06	-0.05	-0.04					
<i>YGAP</i>	0.07	-0.06	-0.03	-0.02	-0.07				
<i>GROWTH</i>	0.21	-0.09	-0.15	0.15	0.06	0.29			
<i>INFL</i>	0.55	0.02	-0.17	-0.13	-0.19	0.15	-0.09		
<i>BORCAP</i>	-0.30	-0.03	0.16	0.09	-0.05	-0.07	-0.16	-0.37	
<i>BUDGET</i>	-0.07	-0.10	0.18	-0.04	-0.05	0.16	0.22	-0.23	0.04
Developing									
	<i>M</i>	<i>ECI</i>	<i>LEGAL</i>	<i>TURNOVER</i>	<i>FIX</i>	<i>YGAP</i>	<i>GROWTH</i>	<i>INFL</i>	<i>BORCAP</i>
<i>ECI</i>	0.02								
<i>LEGAL</i>	-0.03	0.09							
<i>TURNOVER</i>	0.04	0.09	0.16						
<i>FIX</i>	-0.15	-0.03	-0.19	0.08					
<i>YGAP</i>	-0.05	0.03	0.03	0.01	0.09				
<i>GROWTH</i>	0.04	0.07	-0.09	-0.02	0.10	0.28			
<i>INFL</i>	0.58	-0.01	-0.03	0.20	-0.18	-0.06	-0.23		
<i>BORCAP</i>	-0.16	0.01	-0.05	-0.12	0.00	-0.06	-0.09	-0.19	
<i>BUDGET</i>	-0.04	-0.06	0.22	0.23	-0.01	0.01	0.16	-0.07	-0.26

NOTES:

1. See the notes in Table 1.
2. *M* and *INFL* restricted to <100%.
3. A year is defined to be within an election period if *ECI* > 0.50.

Table 4: Summary Statistics by country type (new vs. old democracy) and election period

Old Developing Democracy Observations - 24 countries ^a		obs.	mean	median	s.d.	min.	max.
All Periods	<i>M</i>	316	21.16	18.00	16.71	-29.62	96.23
	<i>YGAP</i>	316	0.28	0.01	4.19	-18.59	17.81
	<i>GROWTH</i>	316	3.94	4.40	4.33	-26.48	14.19
	<i>INFL</i>	316	17.57	11.65	17.55	-7.63	97.00
	<i>BUDGET</i>	283	-4.06	-3.31	4.18	-23.76	5.26
Non-Election Period	<i>M</i>	184	20.81	17.74	17.43	-24.82	96.23
	<i>YGAP</i>	184	0.28	-0.05	4.56	-18.59	17.81
	<i>GROWTH</i>	184	3.79	4.41	4.52	-26.48	11.56
	<i>INFL</i>	184	18.72	11.61	19.56	-0.07	97.00
	<i>BUDGET</i>	164	-3.59	-2.77	4.00	-23.76	5.26
Election Period	<i>M</i>	132	21.64	18.54	15.71	-29.62	83.53
	<i>YGAP</i>	132	0.29	0.16	3.62	-11.19	10.83
	<i>GROWTH</i>	132	4.14	4.40	4.06	-10.27	14.19
	<i>INFL</i>	132	15.97	11.77	14.20	-7.63	78.31
	<i>BUDGET</i>	119	-4.69	-4.06	4.35	-19.97	2.26
New Developing Democracy Observations - 19 countries							
All Periods	<i>M</i>	203	22.95	19.78	18.68	-20.13	93.58
	<i>YGAP</i>	203	0.13	0.11	3.16	-9.44	9.70
	<i>GROWTH</i>	203	3.57	4.19	3.33	-11.89	12.82
	<i>INFL</i>	203	20.11	11.44	19.44	-1.17	88.11
	<i>BUDGET</i>	189	-2.62	-2.31	2.98	-13.02	3.34
Non-Election Period	<i>M</i>	103	24.28	21.22	18.14	-9.15	93.58
	<i>YGAP</i>	103	-0.33	-0.29	3.06	-7.97	7.92
	<i>GROWTH</i>	103	3.14	3.97	3.63	-11.89	10.63
	<i>INFL</i>	103	20.47	11.73	19.34	-0.94	75.43
	<i>BUDGET</i>	96	-2.50	-2.23	2.98	-13.02	2.93
Election Period	<i>M</i>	100	21.59	17.60	19.21	-20.13	79.20
	<i>YGAP</i>	100	0.61	0.72	3.20	-9.44	9.70
	<i>GROWTH</i>	100	4.01	4.30	2.94	-4.41	12.82
	<i>INFL</i>	100	19.74	10.90	19.64	-1.17	88.11
	<i>BUDGET</i>	93	-2.74	-2.31	2.99	-10.22	3.34

NOTES:

1. See the notes in Table 1.
2. *M* and *INFL* restricted to <100%.
3. A year is defined to be within an election period if *ECI* > 0.50.
4. There are 24 developing countries that at one point during the sample were classified as old democracies. Similarly, there are 19 countries that at one point were classified as new democracies. The union of these two groups is 27 countries.

Table 5: Equation (1) - GMM Estimation (using $ECI = T-2$ years,T)

Dependent variable: $M = M1$ Growth (%)						
	(1) Full Sample	(2) Full Sample	(3) Advanced	(4) Advanced	(5) Developing	(6) Developing
<i>ECI</i>	2.465 (2.444)	-2.502 (2.175)	0.880 (2.049)	0.524 (2.173)	3.964 (5.189)	-3.408 (3.306)
<i>M(-1)</i>	0.088 (0.047)*	0.041 (0.044)	0.098 (0.046)**	0.131 (0.046)***	0.065 (0.061)	0.030 (0.057)
<i>LEGAL</i>	-0.385 (3.637)		-2.031 (3.141)		-1.449 (6.099)	
<i>TURNOVER</i>		-6.155 (5.075)		2.003 (3.259)		-13.294 (7.039)*
<i>FIX</i>	-1.078 (1.484)	-2.652 (1.517)*	0.335 (1.363)	0.139 (1.300)	-2.360 (2.297)	-3.601 (2.311)
<i>BORCAP</i>	-0.035 (0.016)**	-0.042 (0.016)***	-0.043 (0.015)***	-0.027 (0.010)***	-0.025 (0.023)	-0.021 (0.021)
<i>YGAP</i>	-0.157 (0.150)	-0.232 (0.178)	-0.296 (0.135)**	-0.376 (0.198)*	-0.025 (0.226)	-0.171 (0.237)
<i>INFL</i>	0.541 (0.089)***	0.484 (0.103)***	0.293 (0.151)*	0.462 (0.127)***	0.642 (0.091)***	0.584 (0.103)***
<i>ECI*LEGAL</i>	-4.139 (4.309)		-4.546 (3.272)		-5.112 (8.318)	
<i>ECI*TURNOVER</i>		10.334 (4.899)**		-5.281 (5.296)		15.118 (7.208)**
<i>ECI*FIX</i>	-0.690 (1.721)	2.547 (2.034)	-2.287 (1.254)*	-0.759 (1.137)	0.403 (3.483)	5.389 (4.147)
<i>ECI*BORCAP</i>	0.000 (0.014)	0.001 (0.018)	0.011 (0.012)	0.004 (0.012)	0.000 (0.048)	-0.009 (0.040)
Observations	1,105	1,355	592	642	513	713
Countries	60	63	25	25	35	38
AR(2) test p-value	0.08	0.36	0.59	0.24	0.18	0.50
Hansen J-test p-value	1.00	1.00	1.00	1.00	1.00	1.00

NOTES:

1. See the notes in Table 1.
2. * significant at 10%; ** significant at 5%; *** significant at 1%.
3. Panel regression, 1972-2001, estimated by GMM. Robust standard errors in parentheses.
4. M and $INFL$ restricted to <100%.

Table 6: Equation (1) - GMM Estimation (using $ECI = T-2$ years, T) - Developing countries with and without $M < 100\%$

Dependent variable: $M = M1$ Growth (%)							
	(1) $M < 100\%$	(2)	(3) $M < 100\%$	(4)	(5) $M < 100\%$	(6)	(7) $M < 4000\%$
<i>ECI</i>	-0.046 (1.034)	42.198 (23.811)*	1.025 (1.014)	1.205 (12.203)	-3.408 (3.306)	-27.223 (25.130)	-41.798 (19.740)**
$M(-1)$	0.156 (0.064)**	0.223 (0.032)***	0.035 (0.056)	-0.154 (0.048)***	0.030 (0.057)	-0.155 (0.048)***	0.087 (0.054)
<i>TURNOVER</i>			-6.001 (7.175)	67.033 (41.002)	-13.294 (7.039)*	54.410 (57.285)	1.061 (23.303)
<i>FIX</i>			-1.519 (1.394)	-5.712 (4.285)	-3.601 (2.311)	-6.852 (9.532)	-5.407 (5.331)
<i>BORCAP</i>			-0.026 (0.011)**	0.325 (0.160)**	-0.021 (0.021)	0.159 (0.067)**	-0.027 (0.041)
<i>YGAP</i>			-0.166 (0.239)	-0.713 (0.634)	-0.171 (0.237)	-0.870 (0.802)	0.861 (0.866)
<i>INFL</i>			0.562 (0.103)***	0.930 (0.041)***	0.584 (0.103)***	0.931 (0.041)***	0.715 (0.156)***
<i>ECI*TURNOVER</i>					15.118 (7.208)**	29.968 (54.031)	76.493 (39.811)*
<i>ECI*FIX</i>					5.389 (4.147)	4.233 (17.361)	6.111 (8.027)
<i>ECI*BORCAP</i>					-0.009 (0.040)	0.364 (0.290)	0.379 (0.223)*
Observations	822	905	713	791	713	791	783
Countries	38	38	38	38	38	38	38
AR(2) test p-value	0.90	0.38	0.48	0.55	0.50	0.54	0.18
Hansen J-test p-value	0.70	0.59	1.00	1.00	1.00	1.00	1.00

NOTES:

1. See the notes in Table 1.
2. * significant at 10%; ** significant at 5%; *** significant at 1%.
3. Panel regression, 1972-2001, estimated by GMM. Robust standard errors in parentheses.

Table 7: Equation (1) - GMM Estimation (using $EC2 = T-1$ year,T)

Dependent variable: $M = M1$ Growth (%)						
	(1) Full Sample	(2) Full Sample	(3) Advanced	(4) Advanced	(5) Developing	(6) Developing
<i>ECI</i>	1.879 (3.259)	-0.363 (2.039)	1.180 (3.445)	3.651 (2.165)*	0.938 (6.677)	-3.232 (3.655)
<i>M(-1)</i>	0.086 (0.047)*	0.046 (0.044)	0.099 (0.046)**	0.134 (0.047)***	0.070 (0.060)	0.042 (0.057)
<i>LEGAL</i>	-1.108 (2.799)		-4.814 (2.727)*		-1.064 (4.499)	
<i>TURNOVER</i>		-0.295 (4.525)		2.933 (3.800)		-6.125 (6.093)
<i>FIX</i>	-1.496 (1.153)	-2.339 (1.107)**	-0.734 (1.252)	-0.589 (1.221)	-2.512 (1.660)	-2.671 (1.530)*
<i>BORCAP</i>	-0.037 (0.015)**	-0.044 (0.013)***	-0.032 (0.013)**	-0.020 (0.011)*	-0.041 (0.020)**	-0.032 (0.014)**
<i>YGAP</i>	-0.157 (0.149)	-0.218 (0.180)	-0.297 (0.132)**	-0.368 (0.192)*	-0.014 (0.222)	-0.169 (0.240)
<i>INFL</i>	0.542 (0.086)***	0.469 (0.103)***	0.299 (0.150)**	0.461 (0.124)***	0.624 (0.087)***	0.555 (0.104)***
<i>ECI*LEGAL</i>	-4.978 (5.404)		0.745 (5.452)		-9.812 (9.457)	
<i>ECI*TURNOVER</i>		-2.309 (7.123)		-12.855 (6.395)**		2.439 (10.676)
<i>ECI*FIX</i>	0.345 (2.359)	3.265 (2.253)	-0.443 (2.145)	1.082 (2.178)	1.064 (4.990)	5.402 (4.454)
<i>ECI*BORCAP</i>	0.007 (0.017)	0.005 (0.016)	-0.012 (0.016)	-0.013 (0.018)	0.077 (0.071)	0.027 (0.048)
Observations	1,105	1,355	592	642	513	713
Countries	60	63	25	25	35	38
AR(2) test p-value	0.07	0.38	0.22	0.50	0.17	0.52
Hansen J-test p-value	1.00	1.00	1.00	1.00	1.00	1.00

NOTES:

1. See the notes in Table 1.
2. * significant at 10%; ** significant at 5%; *** significant at 1%.
3. Panel regression, 1972-2001, estimated by GMM. Robust standard errors in parentheses.
4. *M* and *INFL* restricted to <100%.

Table 8: Equation (1) - GMM Estimation (using $ECI = T-2$ years, T) - Controlling for fiscal policy

Dependent variable: $M = M1$ Growth (%)								
	(1) Developing	(2) Developing	(3) DEV-BD ⁵	(4) DEV-BD	(5) New dem. obs.	(6) New dem. obs.	(7) Old dem. obs.	(8) Old dem. obs.
<i>ECI</i>	-3.408 (3.306)	-3.289 (1.678)*	-3.159 (3.441)	-4.027 (1.866)**	-3.058 (6.085)	-0.969 (5.694)	-6.306 (5.184)	-6.904 (3.295)**
<i>M(-1)</i>	0.030 (0.057)	0.039 (0.065)	0.060 (0.064)	0.111 (0.076)	0.092 (0.089)	0.113 (0.087)	0.016 (0.073)	0.040 (0.094)
<i>TURNOVER</i>	-13.294 (7.039)*	-12.363 (6.665)*	-11.477 (7.824)	-10.499 (6.681)	-12.389 (7.234)*	-12.430 (8.469)	-7.727 (13.413)	-4.479 (12.241)
<i>FIX</i>	-3.601 (2.311)	-2.395 (2.076)	-4.372 (2.640)*	-3.140 (2.130)	-2.222 (3.449)	-0.559 (3.499)	-6.538 (2.552)**	-5.471 (2.081)**
<i>BORCAP</i>	-0.021 (0.021)		-0.019 (0.018)		-0.060 (0.029)**		-0.028 (0.020)	
<i>BUDGET</i>		-0.017 (0.097)		0.024 (0.170)		-0.117 (0.381)		-0.022 (0.279)
<i>YGAP</i>	-0.171 (0.237)	-0.192 (0.258)	-0.272 (0.152)*	-0.376 (0.207)*	-0.189 (0.340)	-0.078 (0.327)	-0.311 (0.185)*	-0.267 (0.197)
<i>INFL</i>	0.584 (0.103)***	0.504 (0.125)***	0.485 (0.099)***	0.424 (0.106)***	0.541 (0.109)***	0.564 (0.103)***	0.292 (0.135)**	0.177 (0.165)
<i>ECI*TURNOVER</i>	15.118 (7.208)**	11.793 (6.138)*	13.182 (7.636)*	10.438 (6.502)	-1.538 (22.702)	-3.225 (22.785)	22.845 (14.148)	17.502 (11.918)
<i>ECI*FIX</i>	5.389 (4.147)	3.591 (3.995)	6.112 (4.684)	5.463 (4.791)	1.511 (4.764)	0.665 (4.513)	8.026 (5.251)	7.018 (5.811)
<i>ECI*BORCAP</i>	-0.009 (0.040)		-0.016 (0.039)		0.051 (0.041)		-0.014 (0.042)	
Observations	713	643	519	472	203	189	316	283
Countries	38	36	27	27	19	19	24	22
AR(2) test p-value	0.50	0.67	0.15	0.31	0.31	0.58	0.18	0.25
Hansen J-test p-value	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00

NOTES:

1. See the notes in Table 1.
2. * significant at 10%; ** significant at 5%; *** significant at 1%.
3. Panel regression, 1972-2001, estimated by GMM. Robust standard errors in parentheses.
4. *M* and *INFL* restricted to <100%.
5. Developing country in the Brender and Drazen (2005) sample.

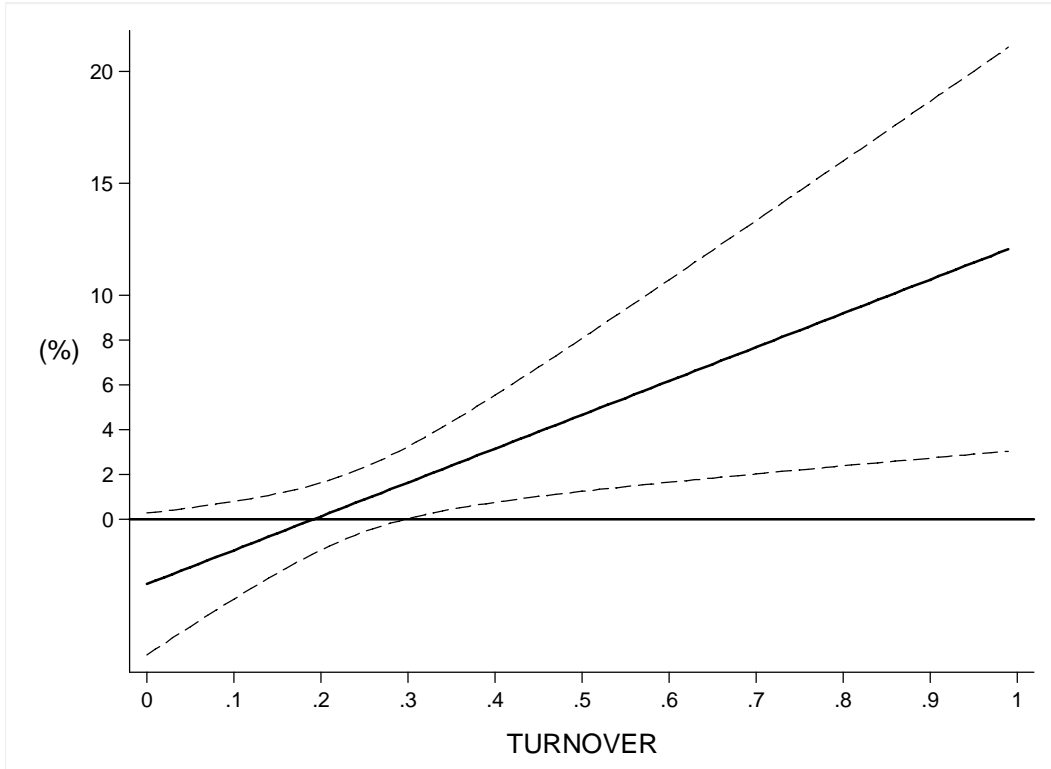


FIG. 1. Marginal Effect of Elections (*ECI*) on Money Growth (*M*) in Developing Countries for each level of *TURNOVER* using specification (6) in Table 5. The dotted lines represent the 90% confidence interval. See the notes in Table 1 for a description of the variables.

Table A1: Variables and Data Sources

Variable	Description and Source
<i>M</i>	Annual % change in M1. Source: IFS variable 34x.
<i>ECl</i>	The default two-year election cycle indicator. Main source - IDEA database. Additional data sources were used to obtain exact election dates and whether the country has a presidential or parliamentary system. These sources are available upon request.
<i>OUTPUTGAP</i>	The log difference between real GDP and its (country specific) trend, estimated using a Hodrick-Prescott filter. GDP is in constant local currency units (WDI).
<i>INFL</i>	Annual percent change in consumer price index. Source: WDI.
<i>FIX</i>	1 for fixed exchange rate regime and 0 otherwise based on Reinhart and Rogoff (2004) course classification.
<i>BUDGET</i>	Central government budget surplus (% of GDP). Source: IFS v80.
<i>GROWTH</i>	GDP growth (annual %). Source: WDI. NY.GDP.MKTP.KD.ZG
<i>BORCAP</i>	Domestic credit to private sector (% of GDP): refers to financial resources provided to the private sector, such as through loans, purchases of nonequity securities, and trade credits and other accounts receivable, that establish a claim for repayment. For some countries these claims include credit to public enterprises. Source: WDI.
<i>TURNOVER</i>	<i>TURNOVER</i> is the average number of changes in the central bank governor per year in each decade. For example, if <i>TURNOVER</i> = 0.2, there are 2 changes per decade for an average tenure of 5 years. One observation per decade (1950's to 1980's). Source: Cukierman, et al., (1992). Data after 1990 obtained from Crowe and Meade (2007) and Dreher, et al. (2008).
<i>LEGAL</i>	One observation per decade (1950's to 1980's). Source: Cukierman, et al., (1992). Data updated by Jácome and Vázquez (2005) and Siklos (2008).
<i>DEV</i>	0 for advanced, 1 for developing or emerging economy based on the classification in Arnone, et al. (2007).
Political Rights	1 low 7 high. 1972-2005. Source: Freedom House.
Civil Liberties	1 low 7 high. 1972-2005. Source: Freedom House.
<i>DEM</i>	Average of Political Rights and Civil Liberties. Source: Freedom House.
<i>reg</i>	Przeworski et al. (2000) <i>Democracy and Development Extended Data Set</i> . 1 for dictatorships, 0 for democracy.
<i>Political Risk Variables</i>	
Government Stability	Source: ICRG published by the PRS group.
Ethnic Tensions	Source: ICRG published by the PRS group.
Internal Conflict	Source: ICRG published by the PRS group.
External Conflict	Source: ICRG published by the PRS group.
<i>Brender and Drazen (2005a) regressors</i>	
<i>TRADE</i>	Total trade as a % of GDP. Source: WDI.
<i>POP1564</i>	Population ages 15-64, % of total. Source: WDI.
<i>POP65</i>	Population ages 65 and above, % of total. Source: WDI.
<i>GDPPC</i>	GDP per capita (constant 2000 \$). Source: WDI.

Table A2: Countries

Argentina	Hungary	Pakistan
Australia	Iceland	Panama
Austria	India	Paraguay
Belgium	Ireland	Peru
Bolivia	Israel	Philippines
Botswana	Italy	Poland
Brazil	Jamaica	Portugal
Canada	Japan	Singapore
Chile	Kenya	South Africa
Colombia	Korea	Spain
Costa Rica	Luxembourg	Suriname
Denmark	Malaysia	Sweden
Dominican Republic	Malta	Switzerland
Ecuador	Mexico	Tanzania
Finland	Morocco	Thailand
France	Nepal	Turkey
Germany	Netherlands	United Kingdom
Ghana	New Zealand	United States
Greece	Nicaragua	Uruguay
Guatemala	Nigeria	Venezuela
Honduras	Norway	Zimbabwe
