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# Political budget cycles in new versus established democracies☆

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## Abstract

Like other recent studies, we find a political deficit cycle in a large cross-section of countries, but show that this result is driven by the experience of "new democracies". The political budget cycle in new democracies accounts for the finding of a budget cycle in larger samples that include these countries and disappears when they are removed from the larger sample. The political deficit cycle in new democracies accounts for findings in both developed and less developed economies, for the stronger cycle in weaker democracies, and for differences in the political cycle across governmental and electoral systems. Our findings may reconcile two contradictory views of pre-electoral manipulation, one that it is a useful instrument to gain voter support and a widespread empirical phenomenon, the other that voters punish rather than reward fiscal manipulation.


*JEL classification:* D72; E62; H62

*Keywords:* Political budget cycle; New democracy; Fiscal manipulation

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## 1. Introduction

A common perception is that incumbents often try to use expansionary economic policy before elections to increase their re-election chances. Most politicians and non-politicians alike would probably subscribe to this view, and the term "election-year economics" or its equivalent is common in many countries.

In the political economy literature, this view is summarized as the "political business cycle", that is, the possibility of a macroeconomic cycle induced by the political cycle. Models of the political business cycle are motivated by the finding that good macroeconomic conditions prior to the elections help an incumbent to get re-elected, a finding that has wide support in studies (conducted mainly in developed economies).[1] The strength of this finding was an important factor generating formal modeling of how opportunistic incumbents may manipulate economic policy to induce economic expansions before elections.

However, notwithstanding both common perceptions and substantial evidence that a "strong economy" helps incumbents get re-elected, empirical studies (especially in developed economies) provide little evidence of a regular, statistically significant increase in economic activity before elections.[2] Voters care about the economy, but this does not appear to translate into econometrically verifiable cycles in aggregate economic activity.

The lack of empirical evidence for political cycles in economic outcomes induced a shift in focus to cycles in policy instruments, especially fiscal expansions in election years, termed the "political budget cycle". Many empirical studies find evidence of such a political budget cycle, consistent with conventional wisdom. Until recently the common view was that political budget cycles were more a phenomenon of less-developed countries than developed countries.[3]

Two recent studies find evidence that the political budget cycle is present in both developed and less-developed countries. Shi and Svensson (2002a, b) consider a panel data set of 91 countries, both democracies and non-democracies, over the period 1975–1995. They find that, in an election year, the government surplus falls significantly in both less-developed and developed countries, though they show that the effect is far stronger in less-developed countries, consistent with earlier studies. Both government spending rises and revenues fall, though the significance differs across the data sets and the estimation technique. The economic effect is significant for the sample as a whole, the fiscal surplus falling on average in their full sample by 1/2–1 percent in an election year, depending on the estimation method they use.

[1]See, for example, Fair (1978, 1982, 1988) for the U.S., Lewis-Beck (1988) for Britain, France, West Germany, Italy and Spain, and Madsen (1980) reported similar results for Denmark, Norway, and Sweden.

[2]See Drazen (2000), chapter 7, for a review of the empirical evidence on opportunistic political business cycles in economic activity.

[3]For less-developed countries, see, Ames (1987) for evidence of a political expenditure cycle in 17 Latin American countries, Block (2000) on a political cycle in both fiscal and monetary policy in a cross-section of 44 Sub-Saharan African countries, and Schuknecht (1996), as well as individual country studies such as Ben-Porath (1975) for Israel, Krueger and Turan (1993) for Turkey, and Gonzàlez, 2002 for Mexico, to name a few. For developed countries, Alesina et al. (1997) find a budget balance cycle in a set of 13 OECD economies over the period 1960–1993, but no significant cycle in the components of the budget. In the United States, there is evidence of a political cycle in transfers until the early or mid-1980s (Keech and Pak, 1989; Alesina et al., 1992), but none thereafter. Drazen (2001) presents further discussion.

Persson and Tabellini (2003, chapter 8) argue that there is a strong political budget cycle in developed economies as well. They restrict the sample to countries with democratic political institutions and competitive elections and consider a group of 60 democracies from 1960 to 1998. They find a political revenue cycle (government revenues as a percent of GDP decrease before elections), but no political cycle in expenditures, transfers, or the overall budget balance across countries or political systems.

In this paper, we find a political budget cycle in a large cross-section of countries, but argue that this finding is driven by the experience of ''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. It is the strong fiscal cycle in these countries that accounts for the finding of a fiscal cycle in larger samples including these countries. Once these countries are removed from the larger sample, the political fiscal cycle disappears. The political cycle in new democracies accounts: for findings in both developed and less developed economies; for the finding that the cycle is stronger in weaker democracies; and for differences in the political cycle across government or electoral systems.

Our findings also reconcile two contradictory views of pre-electoral manipulation. One, following the above-mentioned studies, argues that politicians may be expected to engage in such manipulation and that empirically it is widespread. A very different view casts doubt on the widespread existence of political cycles in macroeconomic policy, since voters in developed economies are ''fiscal conservatives'' who punish rather than reward fiscal manipulation.[4] (A fuller discussion of this view is presented in Section 5.) In new democracies it is possible to carry out such manipulation, whereas in more established democracies, voters have the ability to identify fiscal manipulation and punish such behavior, so that politicians avoid it.

The paper is organized as follows. In Section 2, we set up the basic empirical work, discuss a number of data and estimation issues, and present the basic regressions for the set of democracies as a whole. Section 3, the heart of the paper, demonstrates that the political budget cycle found in larger data sets is due to the significant political cycle in ''new democracies''. In Section 4, we argue that observable characteristics of countries that are often believed to account for the political cycle actually reflect new democracy effects. In Section 5, we discuss conceptually why the political budget cycle is a phenomenon of ''new democracies''. Section 6 concludes.

## 2. Estimating political budget cycles in democracies

### 2.1. The fiscal data

As is well known, the IFS data on which many studies are based are noisy. Therefore, as a first step in our empirical work, the data were ''cleaned''. In our underlying working paper (Brender and Drazen, 2005a), we set out what are the problems with the data on a country-by-country basis, and what were the adjustments that we made (both the working paper and the data are available at http://www.tau.ac.il/~drazen). Our basic data set consists of 106 countries for which we collect data on the central

[4]See Peltzman (1992), Besley and Case (1995), Alesina et al. (1998), and Brender (2003).

government balance, total expenditure and total revenue and grants from the IFS database. The sample period is 1960–2001, although the data for many countries cover shorter periods.

## 2.2. Democracies and non-democracies

Our initial sample includes many countries that are not democracies. In our view, if the political budget cycle reflects the manipulation of fiscal policy to improve an incumbent's re-election chances, then it only makes sense in countries in which elections are competitive. If elections are not competitive, then the basic argument underlying the existence of a political budget cycle loses much of its validity.[5] In fact, one might argue that finding a political budget cycle in non-democratic countries weakens the support for the theory, rather than strengthening it. Hence, from either an empirical or conceptual perspective, one needs to separate democratic from non-democratic countries.

We therefore separate democracies from non-democracies, analogous to Persson and Tabellini, by applying to these data a filter for the level of democracy in each country in each year. This filter is taken from the POLITY IV project, conducted at the University of Maryland, covering nations with a population exceeding half a million people. Each country is assigned in this data set a value that ranges from $-10$ (autocracy) to 10 (the highest level of democracy). We restrict our sample to democracies, by selecting only the countries that receive a score between 0 and 10 on this scale; this reduces our sample to 68 countries. These countries may be classified as those that were in the OECD for the entire sample period, the "transition" economies of Eastern Europe and the former Soviet Union (for the period 1990–2001), and all others.[6] Table A1 provides a list of all democracies, with further information in our working paper.

An important feature of the data is that the number of countries in the sample is increasing over time. This feature reflects not only the expanded coverage of the IFS, but also an increase in the number of democracies. Using the POLITY filter to identify democracies, we find that there are 31 democracies in the sample in the 1960s; 44 in the 1970s, 53 in the 1980s, and 59 in the 1990s, not counting the formerly socialist economies. If the transition economies are included the number of democracies rises to 68 in the 1990s, more than twice the number in the 1960s. More specifically, *new* democracies are being added to each of the samples over time. (The bold entries in Table A1 are the new democracies in our sample.)

[5]Shi and Svensson argue that the desire of dictators to eliminate signs of discontent even before "sham" elections may account for increases in spending and deficits in non-democracies that they report. Alternative explanations of pre-election fiscal expansions that might be observed under both competitive and non-competitive electoral systems would include multi-year economic plans which coincide with the term of governments or "end of term" budgeting effects.

[6]The structural changes that went along with the shift to democracy in these countries imply, among other things, that high deficits associated with the economic transition occur simultaneously with the political transition, without either one causing the other. Conversely, politicians facing the new phenomenon of contested elections who are aware of the desire for rapid economic transition may respond especially strongly with deficit spending. One therefore needs to be careful in how one treats the transition economies in the first years after transition, and interprets the results. To err on the safe side, we exclude all the elections that took place in the first two years following the transition.

### 2.3. Estimation

The basic regression is of the form:

$$f_{i,t} = \sum_k b_k f_{i,t-k} + \sum c' x_{i,t} + \mathrm{d}ELEC_t + \mu_i + \varepsilon_{i,t}, \tag{1}$$

where $f_{i,t}$ is a fiscal indicator in country $i$ in year $t$, $\boldsymbol{x}_{i,t}$ is a vector of control variables, $ELEC_t$ is an electoral dummy, and $\mu_i$ is a country fixed effect. (Year effects were generally insignificant and were dropped from the regressions.) In the tables, we present only the coefficient of the electoral variable, indicating whether or not there is a statistically significant political cycle.

In addition to fixed country effects, our control variables are those used by Persson and Tabellini, which encompass those commonly used in the literature. These include real GDP per capita taken from the 2002 version of the World Bank's World development Indicators data set (WDI), the trade share, two demographic variables representing the fraction of the population aged 15–64 and 65+ (also taken from WDI), and the log-difference between real GDP and its (country specific) trend (computed using the Hodrick-Prescott filter), as a measure of the output gap. (See our working paper for more details.)

The electoral dummy, meant to capture pre-electoral effects, is that used by these authors. It equals 1 in an election year and 0 otherwise, no matter when during the year the election occurred.[7] However, we adjust the electoral year definition to be consistent with the fiscal year, when fiscal data are reported for a fiscal year different than the calendar year. Election dates and institutional data on the election process are taken from the DPI (Database of Political Institutions) data set, provided by the World Bank (Beck et al., 2001). These data were complemented, where needed, by other political data sets, such as the IDEA (Institute for Democracy and Electoral Assistance, "Voter Turnout Since 1945 to Date") and IFES (International Foundation for Electoral Systems, election guide).

Using country fixed effects in an OLS regression with lagged dependent variables introduces a potential estimation bias that is of order $1/T$, where $T$ is the length of the panel. (see, for example, Nickell, 1981 or Wooldridge, 2002). The bias arises because the initial condition $f_{i,0}$ is correlated with the country fixed effect $\mu_i$, so that the lagged dependent variable is correlated with the error term. This problem is thought to be especially severe in micro-panel data, where the number of individuals $i$ is large, while $T$ is quite small, often less than half-a-dozen. Since the potential bias of the fixed effects estimator is of order $1/T$, the magnitude of the bias in our estimates reported below depends on which sample and fiscal indicator we use. In a panel of all democracies from 1960 to 2001, the average length of the sample is 24 years in the whole sample, 34 years in the developed country subsample, and 18 years for less-developed country subsample. (Remember that some countries do not have data for the entire period.) The average length of the time series in our panel of "old" democracies is longer—35 years, with few countries having a time series shorter than the maximum. Hence, the bias from using a fixed effects estimator in these regressions is likely to be small.

[7]See Table 6 and the associated discussion for the effect of splitting the electoral dummy into elections that occurred in the first half of the year and those that occurred in the second half of the year. We consider the endogeneity of election dates in Section 4.

The potential bias may be greater in the panel of elections in "new democracies", since by definition the sample length is much shorter (12 years including transition economies, 13 years excluding them). To address this problem we also present GMM estimates for the subsample of new democracies, using the Arellano-Bond procedure.

### 2.4. The basic results

We began by estimating equations similar to those estimated by Persson and Tabellini, using the same economic controls, variable definitions, and a somewhat extended sample. Our main conclusion is that in a broad cross-section of democracies over the period 1960–2001 there indeed exists a political cycle in the fiscal balance, though the strength of the cycle is sensitive to the set of countries included. In Section 3 we will refine this further, and show that the crucial country characteristic is whether the country is a "new" or an established democracy.

In the first column of Table 1, we present fixed-effects regressions for the fiscal balance, revenues and expenditures, all as a percentage of GDP. We present only the coefficient on the electoral variable, indicating the presence or absence of a political cycle. (The complete regressions may be found in Appendix A of the underlying working paper.) We find a highly significant political cycle in the fiscal balance, with the deficit rising in an election year by about three-tenths of one percent of GDP relative to non-election years.[8]

These results correspond to those found in Shi and Svensson (2002a, b), who considered a cross-section of both democracies and non-democracies over the period 1975–1995. When we used a sample without a democracy filter over the same 20 year period with their control variables, we found a significant coefficient of −.632 with an FE estimator, insignificantly different from their coefficient of −.49. When we ran their regression over the entire sample period using only democracies, the coefficient was −.325, identical to our estimate.[9]

## 3. The empirical importance of being a new democracy

As mentioned above, the number of democracies in the sample increased substantially as more countries, both developed and less-developed, became democracies. Whether a country is a new or established democracy may have a significant effect on the likelihood that incumbents would use pre-electoral fiscal manipulation to increase the probability of their re-election. For example, for voters to punish incumbents for deficits and wasteful spending would require voters to have the necessary information to draw such inferences, as well as the ability to process that information correctly. These would reflect experience with the electoral process by voters, the establishment of the institutions that would collect and provide the relevant data, and experience by media in disseminating and analyzing this information. In the absence of this experience, it is more likely that fiscal manipulation would be rewarded rather than punished, so that incumbents would engage in it. We will return to these arguments in more detail in Section 5.

[8]The qualitative results in these and all other regressions do not significantly change when the White Heteroskedasticity Consistent Covariance correction is used to calculate standard errors.

[9]We were unable to reproduce some of the results of Persson and Tabellini (2003), who, in contrast to our results and those of Shi and Svensson, found no deficit cycle.

Table 1
The political budget cycle across countries, fixed effects estimates, 1960–2001

| | All democracies | | | All "new democracies" | | | "New democracies" excluding "transition economies"[a] | | | "Old democracies" | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | | | (2) | | | (3) | | | (4) | | |
| Dependent variable[b] | Balance | Texp | Trg | Balance | Texp | Trg | Balance | Texp | Trg | Balance | Texp | Trg |
| Elect[c] | −0.352*** | 0.085 | −0.251 | −0.868*** | 0.747** | −0.153 | −0.684** | 0.434* | −0.237 | −0.109 | −0.131 | −0.223* |
| | (0.123) | (0.193) | (0.171) | (0.273) | (0.292) | (0.236) | (0.290) | (0.260) | (0.247) | (0.135) | (0.146) | (0.118) |
| Adjusted $R^2$ | 0.683 | 0.905 | 0.915 | 0.461 | 0.937 | 0.954 | 0.504 | 0.928 | 0.920 | 0.764 | 0.959 | 0.969 |
| $F$-Statistic | 47.96 | 211.63 | 239.87 | 9.42 | 150.57 | 203.18 | 11.62 | 140.19 | 120.61 | 94.937 | 693.30 | 928.81 |
| DW Statistic | 1.955 | 1.562 | 1.455 | 1.821 | 2.051 | 2.114 | 1.682 | 1.925 | 2.134 | 1.900 | 1.987 | 1.872 |
| No. of countries | 68 | 68 | 68 | 36 | 36 | 36 | 26 | 26 | 26 | 32 | 32 | 32 |
| No. of obs. | 1616 | 1631 | 1640 | 415 | 423 | 415 | 336 | 344 | 336 | 1105 | 1112 | 1128 |
| Avg. time series length | 23.8 | 24.0 | 24.1 | 11.5 | 11.8 | 11.5 | 13.0 | 13.3 | 13.0 | 34.5 | 34.8 | 35.3 |

The covariates include one lag of the dependent variable, the log of per-capita GDP, the ratio of international trade (sum of merchandise exports and imports) to GDP, the fraction of the population over age 65, the fraction of the population between ages 15 and 64, and the log-difference between real GDP and its (country specific) trend, estimated using a Hodrick-Prescott filter.

*Significant at the 10 percent level; **significant at the 5 percent level; ***significant at the 1 percent level.

[a]The "new democracies": among the transition economies are listed in Table A1.

[b]Variable definitions (all in percent of GDP): balance—central government surplus; texp—total expenditure by the central government; trg—total revenue and grants of the central government.

[c]Elect—a dummy variable with the value 1 in the election year and 0 otherwise.

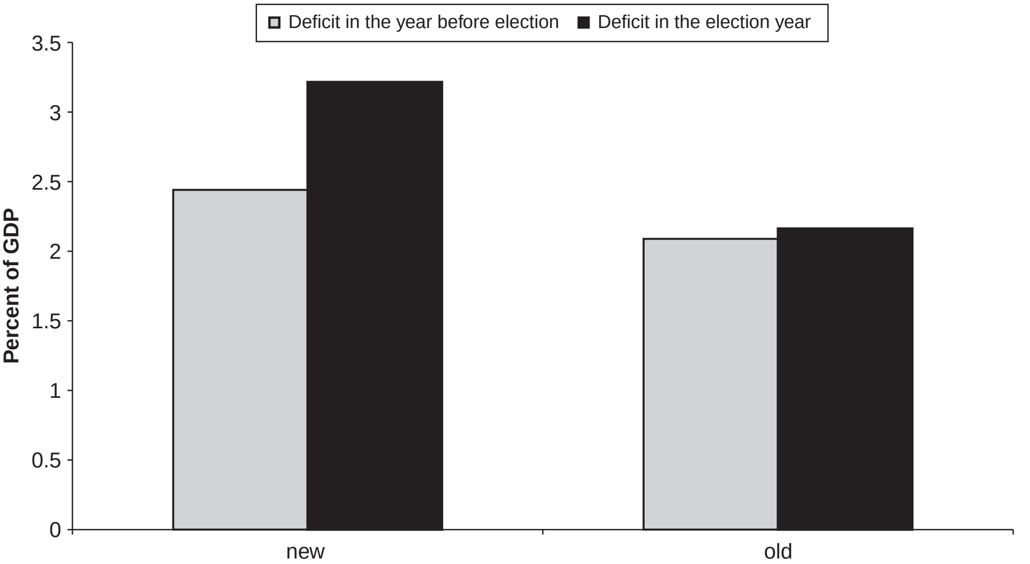


Fig. 1. Average budget deficits in the election year and in the previous year.

To test this hypothesis that political budget cycles are more prevalent in "new democracies", we separate them from established democracies in our sample. Using the POLITY filter, we separate those countries that had competitive elections during the entire sample period for which we have data from those that began having competitive elections only within the sample period. For the latter, we take observations for the first four competitive elections and define those observations as coming from a "new democracy".[10]

Fig. 1 presents the average fiscal deficit in the election year and the year prior to the election in both new versus old democracies. The figure suggests a significant difference between the two, with the deficit in an election year being appreciably higher than in the prior year in a new democracy, while the deficit does not appear to be significantly different across years in an established democracy.

Table 1 shows the results of a formal test (including the effect of covariates). In the second and third columns we present results over only new democracies in the sample both including and excluding the new democracies in Eastern Europe (columns 2 and 3, respectively).[11] The fourth column of the table presents results for only *old*, that is, established, democracies (that is, all countries which were in a sample of democracies using the POLITY filter, *excluding* the new democracies).[12]

[10]An alternative characterization of elections in a new democracy is those elections that occur within a specific time period after the country became democratic. We tried alternative definition of all elections in the first 10 years and the first 15 years after becoming democratic. The results (available on request) are very similar, not surprisingly, since generally the same elections are being captured.

[11]Removing Fiji and Guatemala from the new democracy sample does not change the basic results.

[12]There are two ways one may exclude elections in "new democracies" in testing for a political cycle in "old" democracies. One is to exclude all elections (i.e., all observations), that is, to exclude those countries that made the transition to democracy in the sample period entirely. The other is to exclude only those election observations which occurred when the democracy was in fact "new" (up to the first four elections after the transition to democracy in our definition), but to include all other observations for these countries in a sample of elections in

Because of the short sample length in the new democracy panels, there is a possible bias in using a fixed effects estimator including lagged dependent variables. In Table 2 we therefore present GMM estimates of the new democracy regressions, using the methodology of Arellano and Bond (1991). (In the table, we also present the regressions for all and only old democracies for comparison purposes, though the length of the time series in these samples implies no significant bias in the fixed effects estimates.)

A number of results stand out. First, we find a significant deficit cycle for the set of new democracies, whether or not the formerly socialist economies are included. The coefficients on the electoral variable are larger than in the sample of all democracies. We also find, in contrast to all other results presented so far, that there is a significant political expenditure cycle in the new democracies (as suggested, for example, by Schuknecht (1996)). Note, moreover, that the coefficients on the fiscal balance and on expenditures in the analogous equations are very similar (and of opposite sign), while the coefficient on revenues is smaller in absolute value and not significantly different from zero. The deficit cycle in the new democracies appears to be clearly driven by higher election-year expenditures. When the sample includes only established democracies, there is no significant deficit cycle, but a significant revenue cycle not present in the sample of all democracies. Revenues fall in an election year, similar to what was found by Persson and Tabellini.

To further test the "new democracy" effect, we run regressions for the sample as a whole, that is, both new and old democracies, including separate dummy variables for each of the first four elections, a dummy for all elections in old democracies and a dummy for all elections after the fourth in "former" new democracies. The results are presented in Table 3. Each of the four new election dummies is significant in regressions for a fiscal balance cycle, with approximately equal magnitude, while the coefficients on the dummies for elections after the fourth in new democracies and for elections in old democracies are not significant. Moreover, starting with the second election in new democracies, the significance of the coefficient drops as one moves to the third and fourth elections, suggesting that electoral fiscal effects may be becoming less strong in new democracies as there is more experience with elections. Analogous to our other results there is no significant political cycle in revenues or expenditures when separate election dummies are used.

To summarize, the political deficit cycle is a phenomenon of new democracies. The finding of a statistically political deficit cycle in a cross-section of *all* democracies is due to the first few elections in countries that are new democracies. Once these are removed from the sample and only elections in established democracies are considered, the political deficit cycle as a statistically significant phenomenon in aggregate data disappears.

We should stress that we are *not* arguing that fiscal manipulation does not occur at all in other countries, but only that it is not sufficiently prevalent and large to show up as an econometrically significant regularity in the aggregate fiscal deficit for groups of countries other than new democracies. Of course, there may be incidents of aggregate fiscal cycles in other countries, as well as fiscal manipulation other than fiscal expansion that is not

(*footnote continued*)
old democracies. As we cannot be sure a priori how long the new democratic effect persists (we take four elections as a possible minimum), we prefer the first procedure and present results using that procedure. We ran the regressions using the second definition of "old" democracies and found the same results.

Table 2
The political budget cycle across countries, GMM estimates, 1960–2001[a]

| | All democracies | | | All "new democracies" | | | "New democracies" excluding "transition economies"[b] | | | "Old democracies" | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | | | (2) | | | (3) | | | (4) | | |
| Dependent variable[c] | Balance | Texp | Trg | Balance | Texp | Trg | Balance | Texp | Trg | Balance | Texp | Trg |
| Elect[b] | −0.387*** | 0.222 | −0.153 | −0.805*** | 1.015*** | 0.262 | −0.719*** | 0.644** | −0.002 | −0.18 | −0.056 | −0.262** |
| | (0.120) | (0.178) | (0.148) | (0.266) | (0.338) | (0.346) | (0.253) | (0.252) | (0.327) | (0.130) | (0.152) | (0.114) |
| Sargan test[d] | 0.001 | 0.999 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 1.000 | 0.000 | 0.000 | 0.001 |
| 2nd Order test[e] | 0.177 | 0.683 | 0.164 | 0.529 | 0.632 | 0.323 | 0.675 | 0.766 | 0.567 | 0.259 | 0.393 | 0.439 |
| No. of countries | 68 | 68 | 68 | 36 | 36 | 36 | 26 | 26 | 26 | 32 | 32 | 32 |
| No. of obs. | 1444 | 1457 | 1468 | 338 | 346 | 338 | 279 | 287 | 279 | 1028 | 1033 | 1051 |
| Avg. time series length | 21.2 | 21.4 | 21.6 | 9.4 | 9.6 | 9.4 | 10.7 | 11.0 | 10.7 | 32.1 | 32.3 | 32.8 |

*Significant at the 10 percent level; **significant at the 5 percent level; ***significant at the 1 percent level.

[a]Estimated using the Arellano-Bond procedure with two lags of the dependent variable. The covariates are as in Table 1.

[b]Elect—a dummy variable with the value 1 in the election year and 0 otherwise.

[c]Variable definitions (all in percent of GDP): balance—central government surplus; texp—total expenditure by the central government; trg—total revenue and grants of the central government.

[d]*P*-values for rejecting the null hypothesis that the instruments are uncorrelated with the residuals.

[e]*P*-values for rejecting the null hypothesis that there is no second order serial correlation in the first-difference residuals.

Table 3
The evolution of the "new democracy" effect over time, 1960–2001[a]

| | All Democracies | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Dependent variable[b] | Balance | Texp | Trg |
| Elect_old[c] | −0.111 (0.146) | −0.118 (0.229) | −0.211 (0.202) |
| Elect_ND1[d] | −1.519*** (0.545) | 0.926 (0.836) | −0.555 (0.763) |
| Elect_ND2[d] | −0.855** (0.386) | 0.511 (0.592) | −0.376 (0.540) |
| Elect_ND3[d] | −0.983** (0.464) | 0.878 (0.732) | 0.115 (0.649) |
| Elect_ND4[d] | −1.190* (0.639) | 0.099 (1.007) | −1.026 (0.893) |
| Elect_ND_LATER[d] | −0.686 (0.612) | −0.234 (0.966) | −0.895 (0.856) |
| Adjusted $R^2$ | 0.684 | 0.905 | 0.915 |
| $F$-Statistic | 45.25 | 198.06 | 224.31 |
| DW Statistic | 1.958 | 1.559 | 1.458 |
| No. of countries | 68 | 68 | 68 |
| No. of obs. | 1616 | 1631 | 1640 |

*Significant at the 10 percent level; **significant at the 5 percent level; ***significant at the 1 percent level.

[a]Fixed Effects Estimates. For the list of covariates and variable definitions see Table 1.

[b]Variable definitions (all in percent of GDP): balance—central government surplus; texp—total expenditure by the central government; trg—total revenue and grants of the central government.

[c]Elect_old—a dummy variable with the value 1 in the election year—only in old democracies—and 0 otherwise.

[d]Dummy variables with the value of 1 in the election year of the first, second, third, fourth and later elections, respectively—only in new democracies—and 0 otherwise.

observable in the aggregate fiscal data. But, in terms of aggregate fiscal expansion, it is the new democracies where the political budget cycle is really occurring.

## 4. Country, government, and electoral characteristics

Many empirical studies of the political budget cycle across countries argue that the strength of cycle depends on a country's economic or political characteristics. Such arguments include: the level of economic development, whether elections dates are predetermined or not (Shi and Svensson, 2002b), constitutional rules determining electoral rules and form of government (Persson and Tabellini, 2002, 2003), the "level" of democracy (Shi and Svensson, 2002a, Gonzàlez, 2002), or other measurable factors such as "transparency" or rent-seeking (Shi and Svensson, 2002a; Alt and Lassen, 2003). In this section we consider some of these arguments. For each of the first four arguments, we show that finding a significant deficit cycle is driven by the experience of new democracies.

### *4.1. Developed versus less developed countries*

We first consider developed and less developed countries separately. As already indicated, until recently the political budget cycle was thought to be a phenomenon largely of less developed countries. Shi and Svensson found a cycle in both developed and less-developed countries, but argued that the cycle was significantly stronger in the latter.

Corresponding roughly to a set of developed countries are members of the OECD for the entire sample period. There are four "new democracies" in the sample period in this group—Greece, Portugal, Spain, and Turkey. While there are not enough data points to test for a political fiscal cycle in a sample of only new democracies, we can estimate the equations both with and without these four countries. In columns 1 and 2 of Table 4, we present results for the political fiscal cycle in OECD countries. What we find is that once the new democracies are removed from the sample, so that the sample contains only established democracies, the fiscal balance cycle found in the group of OECD countries as a whole disappears. Similar to what was found for the sample as a whole, there is a statistically significant revenue cycle in OECD established democracies. Hence, as before, the political deficit cycle in new democracies is driving the results for the sample of OECD countries as a whole.

In columns 3, 4, 5, and 6 of Table 4, we consider the political fiscal cycle in less-developed countries (strictly speaking, countries which were not in the OECD at the beginning of the sample period). The regressions correspond to all LDC democracies, LDC new democracies with both FE and GMM estimation, and LDC old democracies. As in the case of developed countries, there is a statistically significant deficit cycle in the LDC sample as a whole, but it is due to the new democracies. We also find that the deficit in the new democracies is driven by higher expenditures in election years. No statistically significant political deficit or revenue cycle is found in the subset of established LDC democracies.

### 4.2. Pre-determined election dates

The strength of the political budget cycle may also depend on whether the election date is pre-determined or not. Although one might think that fiscal manipulation in the year of an election will be stronger when the election date is exogenously fixed by law, there are two conceptual problems with such a simple presumption.

First, the distinction between electoral systems where the election date is exogenously fixed and systems where early elections may be called is not as clear cut as it may at first appear. In many countries fixed election periods are set and early elections may only be called under "exceptional circumstances", but in fact early elections are the rule rather than the exception. That is, what determines "exceptional circumstances" may in practice be quite different than what appears to be the case from a simple reading of the election laws. Since almost all countries have some provision for elections at a date earlier than the end of the legally mandated term of office for the executive or the legislature, whether the elections actually occur at the legally determined date is an empirical question. By the same token, there are countries where the government may call early elections, but rarely does.

Second, we believe that there is no clear theoretical presumption about whether fiscal manipulation will be stronger or weaker when election dates are effectively predetermined. When the election date is known well in advance, an opportunistic incumbent has ample opportunity to use fiscal policy to help his re-election, far greater, it would seem than if there are "snap" elections, with a short lag between elections being called and being held. On the other hand, since incumbents can largely control the timing of endogenous elections, there may be more scope for fiscal manipulation. As argued in the introduction, it is extremely difficult to fine tune when policy will have the desired effect; the option of early elections with a short campaign period may allow elections to be held roughly when the economy looks best.[13] Knowing this, incumbents may be more tempted to use fiscal policy in the attempt to affect voting behavior.[14] Conversely, deterioration in the fiscal situation may create a majority for replacing the government and hence lead to a call for early elections.

One way to address the endogeneity bias from reverse causation or from shocks affecting both the election date and the fiscal balance is to separate out those elections whose timing is pre-determined. We do this by looking at the constitutionally determined election interval taking as pre-determined those elections which were held either at the fixed interval or within the expected year of the constitutionally fixed term.

The results are presented in the first two rows of Table 5. In column 1 of the table, we report the results for the sample of all democracies using an OLS fixed-effects estimator. We find that the coefficient on the electoral variable is similar in size and statistically significant for both pre-determined and endogenous election dates. In columns 2 and 3, we restrict the sample to only new democracies, using a fixed-effects and a GMM estimator, respectively. The coefficient is significant for both pre-determined and endogenous election dates using either method. There is no significant political cycle in established democracies either for pre-determined or for endogenous election dates.

An alternative is to instrument for actual election dates. Explaining early elections in a large panel is beyond the scope of this paper, but as a first pass, we considered the probability of an election in a given year as a function of the legally scheduled election date, which is exogenous to fiscal and other economic variables.[15] (This is obviously a minimally specified model of determinants of actual election dates. However, since the scheduled date is a valid instrument, there is no problem of consistency.) More specifically, in the first stage regression we ran a Probit of the actual on the scheduled date over the whole sample using country slope dummies for the countries in which elections occurred prior to the scheduled year. We then used the results to construct the conditional probability of an election being held in a given year. In the second stage we replaced the 0–1 dummy used in other regressions with the estimated conditional probability of election in a given year. (For countries like the U.S. where elections are held on scheduled dates, the probability index is identical to the 0–1 dummy.) The results for the fiscal balance are presented in the third row of Table 5. As we see, the results basically reproduce the earlier ones.

To summarize, we find a deficit political cycle in ‘‘new’’ democracies, but not in established democracies, regardless of whether elections were pre-determined or took place before their scheduled date. Using the probability of an election rather than the 0–1 dummy also does not change the results. We also find a significant expenditure cycle in ‘‘new’’ democracies for the case of endogenous election dates and a revenue cycle in pre-determined elections in old democracies. Taken as a whole, the results suggest that our finding that the political budget cycle found in large samples is due to new democracies is not caused by the endogeneity of election dates.

[13] Heckelman and Berument (1998) find, for example, that election dates in Japan and the UK are endogenous.

[14] The view that there is no clear theoretical presumption of the effect in one direction or the other is consistent with the results of Shi and Svensson. They find that the coefficient on the fiscal balance was similar across countries with pre-determined versus endogenous election dates.

[15] We are indebted to Yona Rubinstein for this suggestion.

Table 4
The political budget cycle in developed and less developed economies, 1960–2001

| | Developed[a] economies, FE estimation | | | Developed economies excluding "new democracies"[b], FE estimation | | |
|---|---|---|---|---|---|---|
| | (1) | | | (2) | | |
| Dependent variable[c] | Balance | Texp | Trg | Balance | Texp | Trg |
| Elect[d] | −0.267** (0.124) | −0.074 (0.303) | −0.341 (0.275) | −0.108 (0.125) | −0.125 (0.144) | −0.230** (0.116) |
| Adjusted $R^2$ | 0.822 | 0.864 | 0.872 | 0.830 | 0.970 | 0.977 |
| $F$-Statistic | 126.91 | 175.55 | 189.71 | 136.80 | 916.89 | 1218.65 |
| DW Statistic | 1.827 | 1.289 | 1.234 | 1.757 | 1.830 | 1.849 |
| No. of countries | 24 | 24 | 24 | 20 | 20 | 20 |
| No. of obs. | 819 | 823 | 832 | 722 | 726 | 734 |
| Avg. time series length | 34.1 | 34.3 | 34.7 | 36.1 | 36.3 | 36.7 |

| | All less developed economies, FE estimation | | | Less developed "new democracies", FE estimation | | | Less developed "new democracies", GMM estimation | | | Less developed "old democracies", FE estimation | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (3) | | | (4) | | | (5) | | | (6) | | |
| Dependent variable[c] | Balance | Texp | Trg | Balance | Texp | Trg | Balance | Texp | Trg | Balance | Texp | Trg |
| Elect[d] | −0.480** | 0.27 | −0.158 | −0.848*** | 0.661** | −0.187 | −0.706* | 0.822** | 0.114 | −0.13 | −0.151 | −0.211 |
| | (0.215) | (0.228) | (0.187) | (0.296) | (0.322) | (0.266) | (0.366) | (0.365) | (0.311) | (0.314) | (0.332) | (0.269) |
| Adjusted $R^2$ | 0.574 | 0.935 | 0.947 | 0.365 | 0.936 | 0.952 | — | — | — | 0.693 | 0.937 | 0.948 |
| *F*-Statistic | 22.46 | 234.63 | 289.07 | 6.49 | 143.22 | 192.06 | — | — | — | 48.85 | 319.64 | 383.47 |
| DW Statistic | 1.987 | 2.160 | 2.072 | 1.856 | 2.038 | 2.128 | — | — | — | 1.983 | 2.107 | 1.902 |
| Sargan test[e] | — | — | — | — | — | — | 1.000 | 1.000 | 1.000 | — | — | — |
| 2nd Order test[e] | — | — | — | — | — | — | 0.879 | 0.387 | 0.674 | — | — | — |
| No. of countries | 44 | 44 | 44 | 32 | 32 | 32 | 32 | 32 | 32 | 12 | 12 | 12 |
| No. of obs. | 797 | 808 | 808 | 364 | 375 | 364 | 295 | 303 | 295 | 383 | 386 | 394 |
| Avg. time series length | 18.1 | 18.4 | 18.4 | 11.4 | 11.7 | 11.4 | 9.2 | 9.5 | 9.2 | 31.9 | 32.2 | 32.8 |

The covariates are as in Table 1.

*Significant at the 10 percent level; **significant at the 5 percent level; ***significant at the 1 percent level.

[a]OECD Economies that were members of the organization during the entire sample period.

[b]The "new democracies" among the developed economies are Spain, Portugal, Greece and Turkey.

[c]Variable definitions (all in percent of GDP): balance—central government surplus; texp—total expenditure by the central government; trg—total revenue and grants of the central government.

[d]Elect—a dummy variable with the value 1 in the election year and 0 otherwise.

[e]For the definitions of these tests see Table 2.

Table 5
The political budget cycle in different electoral and political systems: new vs. old democracies, 1960–2001

| | All democracies, FE estimation | | | "New democracies", FE estimation | | | "New democracies", GMM[a] estimation | | | Old democracies, FE estimation | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | | | (2) | | | (3) | | | (4) | | |
| Dependent variable[b] | Balance | Texp | Trg | Balance | Texp | Trg | Balance | Texp | Trg | Balance | Texp | Trg |
| 1. Elect-pred[c] | −0.293* (0.151) | −0.002 (0.236) | −0.299 (0.209) | −0.839** (0.325) | 0.417 (0.345) | −0.414 (0.281) | −0.700** (0.309) | 0.548 (0.360) | −0.025 (0.397) | −0.059 (0.165) | −0.217 (0.179) | −0.281* (0.144) |
| 2. Elect-endog[d] | −0.447** (0.186) | 0.227 (0.294) | −0.172 (0.259) | −0.931** (0.468) | 1.476*** (0.502) | 0.403 (0.402) | −0.996** (0.456) | 1.819*** (0.566) | 0.736 (0.502) | −0.186 (0.200) | 0.001 (0.217) | −0.133 (0.176) |
| 3. Elect-prob[e,f] | −0.310** (0.155) | 0.142 (0.245) | −0.175 (0.218) | −0.852** (0.340) | 0.613* (0.362) | −0.191 (0.288) | −0.949*** (0.355) | 0.919** (0.401) | 0.108 (0.421) | −0.073 (0.168) | −0.26 (0.182) | −0.341** (0.149) |
| 4. Elect-pres[g] | −0.400* (0.219) | 0.406 (0.344) | 0.071 (0.302) | −0.848** (0.328) | 0.747** (0.355) | −0.108 (0.284) | −0.732** (0.325) | 0.938** (0.375) | 0.321 (0.401) | 0.152 (0.318) | −0.158 (0.344) | 0.111 (0.273) |
| 5. Elect-parl[h] | −0.330** (0.148) | −0.059 (0.232) | −0.398* (0.205) | −0.912* (0.482) | 0.747 (0.501) | −0.253 (0.416) | −0.965** (0.484) | 1.229* (0.627) | 0.136 (0.537) | −0.165 (0.148) | −0.126 (0.160) | −0.298** (0.130) |
| 6. Elect-prop[i] | −0.346** (0.141) | 0.106 (0.221) | −0.205 (0.195) | −0.993*** (0.305) | 0.901*** (0.326) | −0.043 (0.264) | −0.900*** (0.282) | 1.235*** (0.382) | 0.462 (0.401) | −0.079 (0.155) | −0.133 (0.167) | −0.181 (0.136) |
| 7. Elect-maj[j] | −0.378 (0.255) | 0.033 (0.401) | −0.389 (0.350) | −0.364 (0.613) | 0.11 (0.660) | −0.593 (0.529) | −0.402 (0.592) | 0.243 (0.676) | −0.66 (0.632) | −0.202 (0.267) | −0.127 (0.289) | −0.347 (0.231) |
| 8. Elect-low[k] | −0.659*** (0.194) | 0.394 (0.304) | −0.205 (0.267) | −0.782*** (0.300) | 0.611* (0.320) | −0.257 (0.259) | −0.804*** (0.306) | 0.900** (0.354) | 0.153 (0.374) | −0.312 (0.275) | −0.143 (0.299) | −0.309 (0.234) |
| 9. Elect-high[l] | −0.151 (0.158) | −0.118 (0.248) | −0.282 (0.220) | −1.267** (0.637) | 1.396** (0.688) | 0.331 (0.549) | −0.79 (0.558) | 1.515** (0.750) | 0.704 (0.678) | −0.047 (0.153) | −0.128 (0.166) | −0.195 (0.136) |
| No. of countries | 68 | 68 | 68 | 36 | 36 | 36 | 36 | 36 | 36 | 32 | 32 | 32 |
| No. of obs. | 1617 | 1632 | 1641 | 416 | 424 | 416 | 338 | 346 | 338 | 1105 | 1112 | 1128 |
| Avg. time series length | 23.8 | 24.0 | 24.1 | 11.6 | 11.8 | 11.6 | 9.4 | 9.6 | 9.4 | 34.5 | 34.8 | 35.3 |

*Significant at the 10 percent level; **significant at the 5 percent level; ***significant at the 1 percent level.

[a]Estimated using the Arellano-Bond procedure as described in Table 1.

[b]Variable definitions (all in percent of GDP): balance—central government surplus; texp—total expenditure by the central government; trg—total revenue and grants of the central government.

[c]Elect-pred—a dummy variable with the value 1 in an election year if the elections are in their predetermined dates, as defined in the text, and 0 otherwise.

[d]Elect-endog—a dummy variable with the value 1 in an election year if the elections are not in their predetermined dates, as defined in the text, and 0 otherwise.

[e]Elect-prob—Probability of election in a given year. See text for derivation.

[f]The number of observations for All Democracies, New Democracies, New Democracies(GMM) and Old Democracies are 1579 ,412 ,337 ,1086, respectively. The number of observations for the texp in the GMM regression is 338.

[g]Elect-pres—a dummy variable with the value 1 in an election year if system is presidential, and 0 otherwise.

[h]Elect-parl—a dummy variable with the value 1 in an election year if system is parliamentary, and 0 otherwise.

[i]Elect-prop—a dummy variable with the value 1 in an election year if the electoral system is proportional, and 0 otherwise.

[j]Elect-maj—a dummy variable with the value 1 in an election year if the electoral system is majoritarian, and 0 otherwise.

[k]Elect-low—a dummy variable with the value 1 in an election year if the level of democracy is between 0 and 9, and 0 otherwise.

[l]Elect-high—a dummy variable with the value 1 if the level of democracy is 10, and 0 otherwise.

### 4.3. Constitutional rules

Persson and Tabellini (2003) argue that fiscal policy outcomes depend significantly on the nature of the government system—whether a country has a parliamentary or presidential system of government and whether voting for the legislature was primarily via proportional or majoritarian rules. In a number of papers they consider the importance of constitutional rules on fiscal policy in general and find significant empirical differences in fiscal policy outcomes across systems. Persson and Tabellini (2003) find differences in political budget cycles across government systems.

Following their differentiation of systems, we considered the difference in the political budget cycle across these four categories. There are in fact differences in the deficit cycle across systems when one considers the sample of all democracies. However, similar to our earlier results, we find that these differences reflect the experience of new democracies.

The results on differences between presidential and parliamentary systems are presented in the fourth and fifth rows of Table 5, where the classification follows Persson and Tabellini. We split our electoral dummy into two: one for elections in parliamentary systems, the other for elections in presidential systems. In column 1 of the table we show that in the sample of all democracies, there is a significant deficit cycle in both presidential and parliamentary systems. As before, when we separate new from old democracies, we find that the deficit cycle exists only in the former. Interestingly, the statistically significant revenue cycle which we found in some earlier specifications for established democracies is a phenomenon of parliamentary established democracies, as we see comparing the columns.

In rows 6 and 7 we compare the effect of proportional versus majoritarian voting rules on the political budget cycle. In column 1 we show that in the sample of all democracies, the deficit cycle is significant only in those countries that use proportional voting rules. The coefficient for majoritarian systems is similar, but it is not statistically significant. As before, when we separate the sample into new and old democracies, we find a strong and significant cycle in new democracies with proportional systems, but no significant cycle in the analogous old democracies. Moreover, we also find that the cycle reflects increased expenditures during election years in the new democracies. Hence, we find that the electoral rule matters, consistent with Persson and Tabellini's arguments, but only in the group of countries where the fiscal cycle exists to begin with, namely, the new democracies.[16]

### 4.4. Level of democracy

Another hypothesis is that it is not the length of time a country has been a democracy, but the level of democracy that matters for the existence of a political fiscal cycle. That is, the political fiscal cycle may be a phenomenon of countries where democracy is relatively weaker (see, for example, Shi and Svensson, 2002a; Gonzàlez, 2002) To examine this, we compare the political budget cycle in countries with a lower level ("quality") of democracy to those with a higher level. Specifically, we once again split the electoral dummy into two: Elect-low takes a value 1 in an election year if the POLITY value is between 0 and 9 and a value of 0 otherwise; and Elect-high, which takes a value 1 in an election year if the POLITY value is 10 and a value of 0 otherwise.[17]

[16] We note, however, that our sample includes only 19 majoritarian elections in new democracies.

[17] In some countries the POLITY index changed over time, in which case we split the observations for the country between the groups according to the index in each year.

The results for the sample as a whole and for new and old democracies separately are given in rows 8 and 9 of Table 5. In the first column, we indeed find that the political budget cycle is stronger in countries with a lower level of democracy. The deficit cycle is significant in those countries where the POLITY index of democracy is between 0 and 9, whereas it is insignificant in countries with a POLITY index of 10.

However, once we separate old democracies from new democracies we find that the apparent effect of the level of democracy is entirely due to the new democracies. In the second and third columns we show that for new democracies, the deficit cycle is significant, *regardless* of the level of democracy. In contrast, in the last column, where we consider only established democracies, we find that there is no political budget cycle, once again regardless of their level of democracy.

The reason we find stronger evidence for a political budget cycle in the sample of all countries when we condition on the level of democracy is probably a composition effect. The proportion of new democracies in the group of lower "quality" democracies is significantly higher: 50 percent of the data points in that group, compared to 7 percent among the countries with a high level of democracy. The findings also rule out the explanation that the results for new democracies actually reflect their lower level of democracy, rather than their being "new".

### 4.5. Election dates

Following much of the empirical literature, our election dummy was equal to one in the year of an election no matter when in the year the election took place. If the election took place late in the year, then the dummy indeed captures mostly the period before the election. However, if the election took place early in the year, then the dummy may be capturing primarily post-electoral effects.[18]

One way to address this problem with annual data is to define the dummy as equal to one in the year *before* the election if the election took place in the first half of the year, and equal one in the year of the election otherwise. However, this covers a time period so far before the election in the first case (and may still miss the few months nearest to the election) that the dummy may also be a poor indicator of pre-electoral effects, especially if fiscal manipulation to gain votes is strongest in the months right before an election as suggested, for example, by Akhmedov and Zhuravskaya (2004). Optimally, one would like to have high-frequency data if electoral manipulation is short-lived. However, since this is not possible in a large cross-section study, these considerations suggest that any electoral dummy used with annual data (as must be) might be quite "noisy" for cases where elections are held in the first part of the year. Hence, rather than re-defining the electoral dummy as discussed above, we split the dummy into two, one for elections held in the first half of the fiscal year, the other for elections held in the second half.

The results of our estimation with the dummies are presented in Table 6. We find that for elections held in the second half of the year, where we believe the variable is less noisy, there is a deficit cycle in the sample as a whole that is due to the new democracies. The

[18]We also tested directly the existence of a post-electoral effect by adding a dummy variable for the year after elections. The coefficient was not statistically significant in almost all the equations, did not affect the significance of any of our new democracy results, and eliminated the significance of the revenue coefficient in the old democracies, except for those with parliamentary elections.

Table 6
Alternative timing of elections and the budget cycle, 1960–2001

| | Balance[a], FE estimation | | |
|---|---|---|---|
| | (2) | | |
| Dependent variable | All democracies | New democracies | Old democracies |
| Elect-half1[b] | −0.162 (0.165) | −0.444 (0.387) | 0.031 (0.178) |
| Elect-half2[c] | −0.546*** (0.167) | −1.235*** (0.362) | −0.258 (0.183) |
| Adjusted $R^2$ | 0.683 | 0.463 | 0.764 |
| $F$-Statistic | 47.425 | 5.015 | 92.576 |
| DW Statistic | 1.955 | 1.813 | 1.903 |
| Elections in Parliamentary systems | 68 | 36 | 32 |
| No. of obs. | 1616 | 415 | 1105 |
| Avg. time series length | 23.8 | 11.5 | 34.5 |

*Significant at the 10 percent level; **significant at the 5 percent level; ***significant at the 1 percent level.
[a]Balance—central government surplus (in percent of GDP).
[b]Elect-half1—a dummy variable with the value 1 in an election year if the elections are in the first half of the year, and 0 otherwise.
[c]Elect-half2—a dummy variable with the value 1 in an election year if the elections are in the second half of the year, and 0 otherwise.

coefficient estimates are larger than those reported in Table 1. There is no cycle in old democracies. In contrast, for elections held in the first half of the year, there is no significant cycle in any sample, which we attribute to the noisiness of the dummy in this case.

## 5. The new democracy effect

Why are new democracies more susceptible than established democracies to election-year economics at the aggregate level? It is beyond the scope of this paper to investigate this question in any depth, though our results, as well as those of others, may suggest some avenues of research.

Whether election-year deficits are rewarded or punished at the polls may depend on the availability of information—including the existence of media that would deliver such information to voters (see, for example, Brender, 2003). An incumbent might be rewarded at the polls only if he can hide the manipulation and make the public believe that the good economic conditions reflect the success of his policy or his high ability. This assumption seems unreasonable in many countries because voters—*especially experienced ones* (who understand the incentives and the tools of electoral manipulation)—know that election years are particularly "suspect" for manipulation and therefore would interpret "surprises" in these years with special caution. Therefore, in economies in which the electorate has a lot of experience with elections, and where the collection and reporting of the relevant data to evaluate economic policy are common, voters would be unlikely to "fall" for the trick of making the economy look good right before elections.

In contrast, fiscal manipulation may work when voters lack the necessary information to draw such inferences, as well as the ability to process that information correctly. This would reflect a lack of experience with an electoral system, of the availability of data, and of media experienced in finding, disseminating and analyzing the relevant data.[19] This is more likely to characterize a new democracy.[20]

We want to stress that the ability to draw inferences about incumbent performance from pre-electoral economic variables is not meant simply to represent the experience of voters, but of experience and interactions of all actors with the electoral system. Put another way, it is not that new democracies are characterized by unsophisticated or naïve voting population, but that in countries with less of an electoral history, and hence less exposure to pre-electoral fiscal manipulations, a political cycle is more likely to occur. In many new democracies, even basics like the collection of data and reporting it to the public are not well established, so that fiscal manipulation is easier to engage in. (The demand for data may in fact be driven in part by the possibility of holding office-holders accountable through elections.)

Some recent papers found evidence consistent with this view in specific countries. Brender (2003) shows how the electoral response to deficit spending in local Israeli elections changed dramatically over the period 1989–1998. He found that when direct elections for mayors were introduced in Israel, voters were initially indifferent to deficits and local fiscal management. By the 1998 elections, however, when accounting and reporting standards were enforced on the local authorities, and when the local media expanded, deficit spending was "punished" at the polls.

A recent paper by Akhmedov and Zhuravskaya (2004) found similar evidence in regional elections in Russia after its transition to democracy. Using monthly data between 1996 and 2003, they found sizable, but short-lived political budget cycles in which large expansions and contractions in local fiscal spending occur in the two months before and after elections. They find that the magnitude of the cycle becomes smaller over time and that an additional election in a region reduces the magnitude of the cycles by over 30 percent, so that each new round of regional elections had substantially smaller cycles, with the cycle disappearing for most (but not all) fiscal instruments after two rounds of elections.

A number of papers have considered the role of transparency more formally and may thus shed further light on one important characteristic of new democracies. Gonzàlez (2002) and Shi and Svensson (2002a) extend Rogoff's (1990) model to study the effect of the degree of democracy and the level of institutions on the magnitude of fiscal cycles. Both models stress the importance of "transparency," which ultimately means the probability that voters learn the incumbent's characteristics costlessly, that is, independent of signaling. The higher the degree of transparency, the smaller is the political budget cycle.

Shi and Svensson further argue that while the proportion of uninformed voters—who may be influenced by fiscal manipulation—is initially large, it is likely to decrease over

[19]Another reason why the interpretation of economic data by voters may be more complicated in new democracies is the shift in economic structure that often goes along with the shift to democracy, as, for example, in the transition economies.

[20]If "pivotal" voters are harder to identify in a new democracy, then transfers meant to woo voters may be spread more widely, implying higher aggregate government deficits. We are indebted to Alessandra Casella for this suggestion.

time, thus decreasing the magnitude of budget cycles. They create a measure of the availability of information and show that as voters become more informed the magnitude of the cycle decreases.[21]

Other papers also find that greater transparency is associated with smaller political cycles. Akhmedov and Zhuravskaya (2004) find similarly that measures of the freedom of the regional media and the transparency of the regional governments were important predictors of the magnitude of the cycle. Alt and Lassen (2003) find that in OECD countries, higher fiscal transparency also lowers the magnitude of the electoral cycle. All these results are consistent with ours if lack of transparency or information, as measured by these papers, is an important characteristic of new democracies.

One should also note an essential difference between some of these arguments and ours. Whereas Shi-Svensson and Gonzàlez, for example, view transparency primarily as a characteristic of political systems (that may evolve over time, with institutional change or development), our new democracy results suggest a somewhat different view. "Transparency" reflects experience with the elections themselves, with the crucial variable being the number of competitive elections a country has held (or, the length of time a country has been a democracy), rather than the level of democracy. Our findings in Table 5, namely that the importance of the level of democracy in explaining the cycle may actually reflect the new democracy effect, suggest the importance of distinguishing the two. A key implication of our view is that the signal content of fiscal actions necessarily *changed* over time as voters became more experienced with electoral fiscal manipulation and were provided with more economic and fiscal information in order to draw inferences. This is certainly consistent with the findings of Akhmedov and Zhuravskaya (2004) discussed at the beginning of this section. Hence, any positive effect of deficit spending on an incumbent's electoral prospects would not only diminish over time, but would probably change sign as a country has more experience with a competitive electoral process.

This last point brings us back to the relation between the theory of the opportunistic political business cycle, predicated on the view voters may reward deficit spending at the polls, and the view that voters may punish deficit spending at the polls. According to the second view, it is agreed that a "strong economy" helps incumbents' re-election prospects, but it is argued that politicians have very limited ability to successfully manipulate the economy to help their re-election chances. There are at least two reasons for this. The first is the lack of technical ability to time the expansion accurately enough to happen just before the elections: it is impossible to fine-tune the aggregate economic effects of economic policy so that they can be turned on and off with sufficient precision.[22]

Second, even if it were technically possible to time precisely the aggregate effects of policy, manipulating economic activity is considered harmful to the economy over time in terms of "unsmoothing" consumption, inducing investment cycles, etc. If voters are rational and well informed they would not support such policies, so that pre-electoral manipulation would be punished rather than rewarded at the polls. A number of papers (see footnote 4) present evidence that voters in developed economies are "fiscal conservatives" and often tend to remove deficit-producing incumbents from office.

[21]The index is a product of the number of radios per capita and a binary variable of whether the country had freedom of broadcasting.

[22]See, for example, the discussion in Lewis-Beck (1988).

Our results for new democracies are consistent with the view that voters may "reward" election-year deficit spending, while the findings for established democracies are consistent with the view that they punish it. Proponents of the latter view, such as Peltzman (1992) or Alesina et al. (1998) looked at established democracies, and it is not surprising that they do not find support for an electoral benefit of deficit spending. In Brender and Drazen (2005b) we present results that in a similar cross-section of countries rising deficits during an incumbent's term (and especially before the elections) reduce the probability of reelection in old democracies but not in new democracies. Similarly, though in a much smaller sample, we find indications that an incumbent who engaged in fiscal manipulation in the previous elections is punished in the current elections in old but not new democracies. In short, our new democracy result—and the view that there is a learning process which leads to the empirical disappearance of an aggregate political budget cycle—can reconcile and make consistent these two approaches.

An implication of the argument that voters in established democracies may punish deficit spending is that opportunistic politicians will use fiscal policy to influence voters in ways that don't increase the overall budget deficit. This may be by changing the composition of expenditures in an election year in a way designed to get more votes, or, more specifically, by targeting particular groups of voters. Drazen and Eslava (2004) models rational voters in the first case and presents evidence on the importance of composition of spending effects for the political budget cycle in regional and municipal elections in Colombia. In Drazen and Eslava (2005), it is shown that when rational but imperfectly informed voters must infer whether or not they are targeted for electoral purposes, effective fiscal manipulation can take place without increasing the government budget.

## 6. Conclusions

In this paper we considered the empirical evidence for the existence of a political budget cycle. The question of *whether* such a cycle exists on the macroeconomic level across countries turns out to be a question of *where* it exists, that is, in which types of countries. The answer to that question is not only empirically relevant, but theoretically important as well, since it sheds light on what factors may account for the existence of a cycle.

Our empirical results indicate that the political deficit cycle is a phenomenon of new democracies. The strong political cycle in those countries, which is characterized by increased expenditures in election years, accounts for the finding of a political deficit cycle in larger samples including these countries. Once these countries are removed from the larger sample, the political deficit cycle in larger samples disappears. Furthermore, our results indicate that empirical findings of stronger cycles in less-developed than in developed countries, in countries with lower levels of democracy, or across government systems and electoral rules is driven by the experience of new democracies.

This finding suggests that fiscal manipulation is used more broadly in "new" democracies, where it may "work" because of lack of experience with electoral politics or lack of information that is available in established democracies and used by experienced voters. As models that view rational voters as "fiscal conservatives" suggest, once a country becomes experienced in electoral politics, the scope for a political fiscal cycle at the aggregate level should be diminished, perhaps significantly so.

## Appendix A

See Table A1 for details.

Table A1
Country classification

| No. | Country[a] | Year of becoming a democracy[b] | Number of elections in the sample[c] | OECD economy |
|---|---|---|---|---|
| 1 | **Argentina** | 1983 | 3 | |
| 2 | Australia | | 16 | × |
| 3 | Austria | | 13 | × |
| 4 | Belgium | | 12 | × |
| 5 | **Bolivia** | 1982 | 3 | |
| 6 | **Brazil** | 1985 | 3 | |
| 7 | ***Bulgaria**** | 1990 | 2 | |
| 8 | Canada | | 12 | × |
| 9 | Chile | 1989 | 2 | |
| 10 | Colombia | | 7 | |
| 11 | Costa-Rica | | 7 | |
| 12 | Cyprus | | 5 | |
| 13 | ***Czech Republic**** | 1990 | 2 | |
| 14 | Denmark | | 15 | × |
| 15 | **Dominican Republic** | 1978 | 4+2 | |
| 16 | **Ecuador** | 1979 | 4+2 | |
| 17 | **El Salvador** | 1984 | 3 | |
| 18 | ***Estonia**** | 1991 | 3 | |
| 19 | **Fiji** | 1970, 1990 | 5 | |
| 20 | Finland | | 10 | × |
| 21 | France | | 7 | × |
| 22 | Germany | | 11 | × |
| 23 | **Greece** | 1975 | 4+4 | × |
| 24 | **Guatemala** | 1966, 1986 | 4 | |
| 25 | **Honduras** | 1982 | 2 | |
| 26 | ***Hungary**** | | 2 | |
| 27 | Iceland | | 8 | × |
| 28 | India | | 8 | |
| 29 | Ireland | | 11 | × |
| 30 | Israel | | 10 | |
| 31 | Italy | | 10 | × |
| 32 | Japan | | 8 | × |
| 33 | **Korea** | 1988 | 2+1 | |
| 34 | ***Lithuania**** | 1991 | 1 | |
| 35 | Luxembourg | | 5 | × |
| 36 | **Madagascar** | 1992 | 2 | |
| 37 | Malaysia | | 9 | |
| 38 | **Mali** | 1992 | 1 | |
| 39 | Mauritius | | 6 | |
| 40 | **Mexico** | 1988 | 2 | |
| 41 | **Nepal** | 1990 | 4 | |
| 42 | Netherlands | | 12 | × |
| 43 | New Zealand | | 12 | × |

Table A1 (*continued*)

| No. | Country[a] | Year of becoming a democracy[b] | Number of elections in the sample[c] | OECD economy |
|---|---|---|---|---|
| 44 | **Nicaragua** | 1990 | 1 | |
| 45 | Norway | | 10 | × |
| 46 | **Pakistan** | 1988 | 3 | |
| 47 | **Panama** | 1989 | 2+1 | |
| 48 | Papua New Guinea | | 6 | |
| 49 | **Paraguay** | 1989 | 2 | |
| 50 | **Peru** | 1980 | 2 | |
| 51 | Philipines | 1987 | 3+3 | |
| 52 | *Poland** | 1989 | 2 | |
| 53 | **Portugal** | 1976 | 4+2 | × |
| 54 | ***Romania**** | 1990 | 2 | |
| 55 | ***Russia**** | 1992 | 2 | |
| 56 | ***Slovak Republic**** | 1993 | 1 | |
| 57 | ***Slovenia**** | 1991 | 2 | |
| 58 | South Africa | | 9 | |
| 59 | **Spain** | 1978 | 4+2 | × |
| 60 | Sri Lanka | | 7 | |
| 61 | Sweden | | 13 | × |
| 62 | Switzerland | | 10 | × |
| 63 | Trinidad & Tobago | | 4 | |
| 64 | **Turkey** | 1983 | 3+2 | × |
| 65 | United KinGdom | | 9 | × |
| 66 | United States | | 10 | × |
| 67 | **Uruguay** | 1985 | 3 | |
| 68 | Venezuela | | 8 | |

[a]New democracies in bold letters. New democracies which are also transition economies are in italics and marked with a (*).

[b]The first year in which the country receives a positive value in the POLITY scale, following a substantial period of negative values. The actual transition (e.g., first democratic elections) can take place during the previous year.

[c]The number after a + sign indicates the number of elections that took place during years in which the was not a "new democracy". For some countries, these refer to elections *before* a transition to not being a democracy.

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