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Temporal Aggregation in Political Budget Cycles

Electoral cycles have been widely debated since the pioneering studies of the 1970s on monetary and fiscal policy.[1] Drazen sums up the evidence in terms of active fiscal policy and passive monetary policy: while expansionary fiscal policy is the main impulse behind electoral cycles, monetary policy has an accommodating role.[2] Recently, there has been a flurry of empirical work on political budget cycles (PBCs) using cross-country panels. The most influential works concentrate on the budget surplus because it can capture both increases in expenditures and tax cuts before elections and is thus the most sensitive indicator of aggregate PBCs.[3]

A drawback of these extant studies is their reliance on annual observations. Since elections take place between January and December, annual data do not allow one to identify the election year precisely. To get around this problem, other schemes have been proposed. For example, by the rule of the semester, the previous year is counted as the election year if elections take place before July.[4] What are the consequences of temporal aggregation for the measurement of PBCs? Instead of being concentrated in the election year, pre-electoral effects may be spread out over the two years leading up to

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1. On monetary policy, see Nordhaus (1975); on fiscal policy, see Tufte (1978) and Frey and Schneider (1978a, 1978b).
2. Drazen (2001).
3. Persson and Tabellini (2003); Brender and Drazen (2005); Shi and Svensson (2006).
4. Barberia and Avelino (2011).

TABLE 1. Simulation of the Effects of Temporal Aggregation on the Budget Surplus[a]

Quarter	*Elections 1st quarter*	*Elections 2nd quarter*	*Elections 3rd quarter*	*Elections 4th quarter*	*Total by quarter*	*Average annual effect*
Y(−1)_*Q*(1)	0	0	0	0	0	
Y(−1)_*Q*(2)	−1	0	0	0	−1	
Y(−1)_*Q*(3)	−1	−1	0	0	−2	
Y(−1)_*Q*(4)	−1	−1	−1	0	−3	−0.375 = −6/16
Y(0)_*Q*(1)	−1	−1	−1	−1	−4	
Y(0)_*Q*(2)	1	−1	−1	−1	−2	
Y(0)_*Q*(3)	1	1	−1	−1	0	
Y(0)_*Q*(4)	1	1	1	−1	2	−0.250 = −4/16
Y(1)_*Q*(1)	1	1	1	1	4	
Y(1)_*Q*(2)	0	1	1	1	3	
Y(1)_*Q*(3)	0	0	1	1	2	
Y(1)_*Q*(4)	0	0	0	1	1	0.625 = 10/16

a. Surplus = −1 in four quarters leading up to elections, surplus = 1 in the four quarters after elections, and surplus = 0 otherwise.

elections. More important, if the sign of policies is reversed after elections, lower frequency data may mask PBCs because the effects in the election year cancel out, as Akhmedov and Zhuravskaya point out in their country study of Russia.[5]

To illustrate the differences between using annual and quarterly data to identify the election year, imagine that elections take place in June. Furthermore, assume there is a pre-electoral deterioration in the budget surplus (surplus = −1) during the four quarters running up to elections, followed by an improvement (surplus = 1) the next four quarters. In the calendar year in which elections take place, the net effect on the budget surplus is zero. Elections are not always in June, of course. What happens if elections are evenly spread out over the year? Table 1 presents a simulation where *Y*(0) is the calendar year of elections, *Y*(−1) is the year before, and *Y*(1) is the year after.

For elections in the first quarter of the election year, namely, *Y*(0)_ *Q*(1), our textbook example of a pre-electoral expansion in the deficit during the four quarters prior to elections, followed by a contraction the next four quarters, implies that the surplus equals −1 from *Y*(−1)_*Q*(2) to *Y*(0)_*Q*(1), it equals 1 from *Y*(0)_*Q*(2) to *Y*(1)_*Q*(1), and it equals 0 otherwise, when the budget surplus is at its average value, normalized here at zero. The same procedure is followed for elections in the other three quarters. If we aggre-

5. Akhmedov and Zhuravskaya (2004).

gate the effects by calendar year, there is a serious underestimation of PBCs in the election year, since pre-electoral expansions are almost cancelled out by post-electoral contractions, leaving a net expansive effect that is only 25 percent of the total stimulus before elections. The least affected by temporal aggregation are post-electoral fiscal adjustments, since 62.5 percent of the total effect is reflected the year after elections. On the other hand, if there were no post-electoral fiscal adjustments, the problem of temporal aggregation would be much less severe, since 62.5 percent of the total expansive effect would be reflected the calendar year of elections and 37.5 percent the year before.

This paper tackles the issue of temporal aggregation in PBCs head-on. We build a cross-country panel with annual and quarterly data from nineteen Latin American countries and twenty member states of the Organization for Economic Cooperation and Development (OECD) over the 1980–2005 period.[6] Quarterly data allow us to identify the election year more precisely: with annual data, the election year is the calendar year in which elections take place; with quarterly data, the election year comprises the election quarter plus the three prior quarters.

We introduce two nonstandard features in our analysis. First, to address the issue of temporal aggregation, we control for post-electoral effects. In his analysis of political business cycles, Nordhaus builds a framework in which the monetary stimulus applied before elections is reversed afterward, preventing long-run consequences for inflation.[7] For political budget cycles to follow a similar pattern and avoid a permanent impact on public debt, fiscal policy should become contractionary after elections, to counteract expansionary policy before elections. However, the literature focuses almost exclusively on expansive fiscal policies during the election year.[8] Ames, however, finds that government expenditures in Latin America not only rise during the election year, but also fall the year after, and Persson and Tabellini find a significant improvement of the budget surplus the year after elections, based on a panel of sixty democracies over the 1960–98 period.[9]

6. Since quarterly GDP data are not available for this large set of countries, we used higher frequency data on imports to distribute annual GDP figures within the year.

7. Nordhaus (1975). Policy stimulus reduces unemployment as elections approach, increasing inflation in the process; after the elections, the victor raises unemployment to combat inflation (Nordhaus, 1975, p. 184).

8. This approach started with Tufte (1978) and Frey and Schneider (1978a, 1978b).

9. Ames (1987); Persson and Tabellini (2003, chap. 8).

As to our second nonstandard feature, the literature on PBCs implicitly assumes a single policymaker. Rogoff and Sibert demonstrate that even if all agents are perfectly rational, the temptation for incumbents to signal their competence, and thus to enhance their probability of reelection, leads to electoral cycles under asymmetric information on policy decisions.[10] Lohmann demonstrates that this temptation is present even if an incumbent does not have private information about his or her own competence.[11] Despite the fact that electoral cycles do not increase the chances of winning elections in equilibrium, incumbents are trapped in them because of the credibility problems caused by the discretionary power to change policy in election years.

In constitutional democracies, the legislature participates in fiscal policy, so the necessary conditions for PBCs are both asymmetric information and discretionary power for an incumbent to exploit fiscal policy.[12] Schuknecht conjectures that political budget cycles are stronger in developing countries because of weaker checks and balances.[13] Streb and Torrens formalize Schuknecht's conjecture; they show that if there is compliance with the law, a legislative veto player can lend credibility to a budget rule that prohibits the manipulation of debt.[14] To analyze this, we draw on the Henisz data set, using the political constraints index and the International Country Risk Guide (ICRG) law-and-order index to capture the presence of veto players in countries with a high degree of compliance with the law.[15] Streb, Lema, and Torrens employ these variables and find evidence that backs the Schuknecht conjecture.[16] Weaker checks and balances explain not only why PBCs are stronger in developing countries, but also why they are stronger in new democracies and presidential countries.[17] However, studies to date in this area share the shortcoming of relying on annual data.

10. Rogoff and Sibert (1988).
11. Lohmann (1998b).
12. Saporiti and Streb (2008).
13. Schuknecht (1996).
14. Streb and Torrens (2012). Presidential systems are characterized by a legislative veto player with divided government; parliamentary countries have a veto player with coalition governments. Veto players ensure not only the credibility of rules, but also of delegation, as Lohmann (1998a) and Keefer and Stasavage (2003) show for the delegation of monetary policy to a central bank.
15. Henisz (2005).
16. Streb, Lema, and Torrens (2009).
17. Compare Shi and Svensson (2006); Brender and Drazen (2005); Persson and Tabellini (2003).

The next section describes the data and econometric specification. We then compare the results of identifying the election year with annual and quarterly data. A later section looks within the election window to determine its exact timing using quarterly data, and the paper closes with a discussion of the implications.

Econometric Specification and Data

We follow the previous empirical literature on PBCs in describing the relation between a given policy variable y in country i and year t ($y_{i,t}$) and the electoral cycle as follows:

$$(1) \qquad y_{i,t} = \sum_{j=1}^{k} \beta_j y_{i,t-j} + \sum_{j=1}^{m} \gamma_j x_{j,i,t} + \delta E_{i,t} + \lambda z_{i,t} E_{i,t} + \sum_{j=1}^{n} \varphi_j t_{j,t} + \mu_i + \varepsilon_{i,t},$$

where $\mathbf{x}_{i,t}$ is a vector of m controls, $E_{i,t}$ is a dummy election variable, $z_{i,t}$ is a proxy variable for effective checks and balances conditioning the electoral policy manipulations, $t_{j,t}$ controls for time effects, μ_i is a specific country effect, and $\varepsilon_{i,t}$ is a random error term that is assumed to be independent and identically distributed.

This specification represents a dynamic panel model, where the dependent variable is a function of its own lagged levels, a set of controls, and the electoral timing conditioned by effective checks and balances. To determine the lags of the dependent variable, we pick the number that maximizes the value of the F statistic, which points to one lag for annual data and four lags for quarterly data (the Akaike information criteria point to a sharp fall at that same number of lags, though the statistic continues to decline slowly as the number of lags increases).

Like Shi and Svensson, we control for the state of the economy by using the log of real gross domestic product (GDP) per capita, as well as the growth rate of real GDP, which captures the effects of the business cycle on the budget surplus as it rises in booms and falls in recessions in response to the procyclical behavior of tax collection.[18] As to time effects, we introduce five-year

18. Shi and Svensson (2006).

dummies for the whole sample, and in the quarterly estimates we additionally control for seasonality in each country. The basic estimates are performed with STATA 10 using fixed effects.[19]

We collect data from forty-six Latin American and OECD countries in order to compare democracies from developing and developed regions. We focus on the thirty-nine countries for which data are available on a quarterly and annual basis, of which nineteen are from Latin America and twenty are from the OECD. Table A1 in the appendix reports the complete list of countries.

Table 2 provides the definitions and sources of the economic and political variables we use in our econometric estimates. The fiscal and GDP data are from the International Monetary Fund's *International Financial Statistics;* the population figures are from the World Bank's *World Development Indicators.* To construct quarterly GDP figures in nominal terms, we follow the Fernández distribution procedure, available in MATLAB, using quarterly import series.[20] This allows us to compute the ratio of the budget surplus to GDP on a quarterly basis (see the appendix). We follow a similar procedure to distribute real GDP.

The information on democratic periods is from the Polity IV Project. To define the relevant election dates, we use presidential elections in presidential countries and general legislative elections in parliamentary countries, following the classification in the Database of Political Institutions (DPI). The electoral calendar draws on the Center on Democratic Performance at Binghamton University, SUNY, for the 1994–2004 period, complemented by the *Enciclopedia electoral de América Latina y el Caribe* and various web sources.[21] When there are run-off elections, we count the second election as the moment of elections, so the second electoral round always falls within the election year.

The variables on veto players and compliance with the law are based on the POLCON data set.[22] The political constraints index POLCONIII is designed to measure the legislative constraints facing the executive branch

19. The Hausman test that compares the results of using fixed-effects and random-effects estimators leads to mixed results: in several estimates, the null hypothesis that the extra orthogonality conditions imposed by the random-effects estimator are valid is rejected; in others, it is not. If the regressors are uncorrelated with the error term, the fixed-effects estimator is consistent, albeit inefficient. To follow a uniform criterion, we always use the fixed-effects estimator.

20. Fernández (1981).

21. Nohlen (1993).

22. Henisz (2005).

TABLE 2. Definition of Variables

Variable	*Description*	*Source*[a]
Economic variable		
f	Data frequency, where *f* = *a, q* (annual, quarterly)	
expenditures_f	Total central government expenditures, *f* = *a, q*	IFS
revenues_f	Total central government revenues and grants, *f* = *a, q*	IFS
surplus_f	Budget surplus, equals *revenues_f* − *expenditures_f*, *f* = *a, q*	IFS
ngdp_a	Nominal GDP, *f* = *a*	IFS
expenditures_ngdp_f	Percentage share of *expenditures_f* in *ngdp_f*, *f* = *a, q*	AU
revenues_ngdp_f	Percentage share of *revenues_f* in *ngdp_f*, *f* = *a, q*	AU
surplus_ngdp_f	Percentage share of *surplus_f* in *ngdp_f*, *f* = *a, q*	AU
ln(1+*sur_exp*)_*q*	100 * ln (1 + (*surplus_q* / *expenditures_q*)), *f* = *q*	AU
y_f(−t)	Dependent variable *y* lagged *t* periods, *f* = *a, q*	AU
n_a	Population, *f* = *a*	WDI
gdp_a	Real GDP, *f* = *a*	IFS
ln(*gdp_per_capita*)_*f*	Natural log of real GDP per capita, *f* = *a, q*	AU
gdp_growth_f	Growth rate of real GDP, *f* = *a, q*	AU
Seasonal or temporal dummy		
*quinquennium*1	Dummy = 1 in 1980–84 period, 0 otherwise	AU
*quinquennium*2	Dummy = 1 in 1985–89 period, 0 otherwise	AU
*quinquennium*3	Dummy = 1 in 1990–94 period, 0 otherwise	AU
*quinquennium*4	Dummy = 1 in 1995–99 period, 0 otherwise	AU
quarter_country(*t*)	For each country, dummy = 1 in quarter *t,* 0 otherwise, *t* = 1,2, 3	AU
Political variable		
demo	Dummy = 1 if Polity index ≥ 0 for a country in a given year	Polity IV
pres	Dummy = 1 if presidential country, 0 if parliamentary	DPI
date_election	Date (month and year) of presidential election or, in parliamentary countries, general election	SUNY & others
ele(0)	Dummy = 1 in election year, 0 otherwise	AU
ele(1)	Dummy = 1 in post-election year, 0 otherwise	AU
pbc	Dummy = 1 in election year, −1 in post-election year, 0 otherwise	AU
ele_q(*t*)	Dummy = 1 in quarter *t* before/after election, 0 otherwise, *t* = −5, −4, −3, −2, −1, 0, 1, 2, 3, 4, 5, 6	AU
vetoplayer	Equals 1 if POLCONIII ≥ 2/3; 3/2 * POLCONIII otherwise	H(2005), AU
law	ICRG Law and Order index	ICRG, H(2005)
compliance	Dummy = 1 for country if *law* ≥ 4 always, 0 otherwise	AU
checks	Effective veto player: *vetoplayer* * *compliance*	AU
checks'	Alternative measure: *vetoplayer* * (*law* / 6)	AU
pbc_checks	Influence of *checks* on PBCs: *pbc* * *checks*	AU
*pbc_*(1−*checks*)	Discretional component of PBCs: *pbc* * (1 − *checks*)	AU
ele_checks	Equals *ele*(0) * *checks*	AU
*ele_*(1−*checks*)	Equals *ele*(0) * (1 − *checks*)	AU

a. IFS refers to the IMF *International Financial Statistics;* AU to variables constructed by the authors; WDI to the World Bank's *World Development Indicators;* Polity IV to the Polity IV Project; DPI to the Database of Political Institutions; SUNY to the Center on Democratic Performance, Binghamton University, SUNY; H(2005) to Henisz (2005); and ICRG to the International Country Risk Guide.

when implementing policy.[23] Given that the legislature typically has to authorize new debt, or government expenditure itself, the variable of interest for us is whether there is a legislative veto player.[24] Hence, the variable *vetoplayer* equals one when POLCONIII equals two-thirds or more, because the executive branch faces a full legislative veto player, while values of less than 2/3 are divided by 2/3, making *vetoplayer* vary linearly in the [0,1] interval.

There is no direct measure of compliance with the budget law. Instead, the POLCON data set reports the ICRG index on law and order, which measures the strength and impartiality of the legal system and the general observance of the law, on a scale from zero (low) to six (high). In earlier years when the law and order index is not available, we use instead the ICRG rule of law index, which behaves similarly. We create a dummy variable, *compliance,* that takes a value of one if this index is larger than four in all years that are reported for a given country and zero otherwise.[25] This treatment implies treating compliance with the law as a fixed characteristic. To construct a measure of the effective checks and balances that a legislature can impose on the executive branch through the budget process, the nominal presence of a legislative veto player is multiplied by the dummy variable that identifies countries with a high degree of compliance: *checks* = *vetoplayer* ∗ *compliance*. In the robustness tests below, we also use the original ICRG index.

23. Henisz (2002) derives POLCONIII in a spatial model under the assumption that the status quo policy is uniformly distributed over the policy space [0, 1]. The polar cases are as follows. The minimum is zero, when the legislature is completely aligned with the executive branch, that is, the party in the executive branch controls 100 percent of the legislative seats. The maximum is two-thirds with a single legislative chamber, when the legislature is completely independent from the executive branch, and four-fifths with two chambers, when both chambers are completely independent. The intermediate cases are as follows. If the party that heads the executive branch has a legislative majority, Henisz (2002) assumes that as this majority diminishes from holding all the legislative seats, the difficulty in satisfying the preferences of all coalition or faction members increases. Less alignment decreases the feasibility of policy change and implies more political constraints for the executive branch. Hence, this value is adjusted for the fractionalization of the legislature, which is the probability that two random draws from the legislature are from different parties. Something similar is done in case the opposition has a majority in the legislative branch, adjusting the value by one minus the fractionalization index. High fractionalization within each legislative branch increases (decreases) political constraints for an aligned (opposed) executive branch. The POLCONIII index is measured on 1 January of each year, so it is predetermined in relation to elections that year.

24. Streb and Torrens (2012).

25. Though the cut-off point of four is arbitrary, a higher cut-off would eliminate the United Kingdom as a country where there is compliance with the law and a lower one would include Argentina. In Latin America, only Chile and Costa Rica have a compliance dummy of one; in the OECD, only Greece, Italy, and Korea have a compliance dummy of zero.

Identifying Election Years with Annual and Quarterly Data

Table 3 reports descriptive statistics for the thirty-nine countries for which both annual and quarterly data are available, as well as for the Latin American and OECD subsamples. The period is divided into election years, post-election years, and normal years, that is, years that are neither election nor post-election years. There are elections roughly every four years.

The average budget surplus is −2.8 percent of GDP in the full sample, and around −2.3 percent in Latin America and −3.2 percent in the OECD. The budget deficit is largest in election years: in the full sample, the difference between election years and normal years is 0.8 percent of GDP according to annual data and 1.2 percent of GDP according to quarterly data; the same pattern is repeated for Latin American and OECD countries. Since quarterly data are not always available for the same period as annual data, the overlap between the two time series is not perfect. If, instead, we average the data in a five-year window around all elections with complete data at both annual and quarterly frequencies (there are 116 such complete episodes, out of 235 elections during democratic periods), this exercise reveals that in Latin America the budget deficit is lowest in post-election years.

Estimates for the Full Sample

We first study the behavior of the budget surplus with a fixed-effects estimator. However, when the dependent variable is a function of its own lagged levels, the error term of the fixed-effects estimator will be correlated with the lagged dependent variable. The generalized method of moments (GMM) estimator designed by Arellano and Bond for dynamic panels may be preferable for small T (number of periods) relative to N (number of countries) even though the set of observations available is smaller, because GMM makes use of the lagged values of the variables as instruments.[26] This is the case with the annual data. When T is larger than N, as is the case with the quarterly data, fixed effects work fine because the bias in the fixed-effects estimator depends on the reciprocal of T; the fixed-effects estimator of the coefficients will be consistent provided that T is sufficiently large.

Table 4 reports, for the annual data, the fixed-effects estimates in the first three columns, and the GMM estimates in the following three; for the quar-

26. Arellano and Bond (1991).

TABLE 3. Descriptive Statistics for Full Sample and Subsamples

Sample and variable	*Minimum*	*Maximum*	*Mean all years*	*Std. dev. all years*	*Mean election years*	*Mean post-election years*	*Mean normal years*
Full sample (39 countries)							
surplus_ngdp_a	−41.221	10.207	−2.755	4.136	−3.373	−2.556	−2.570
surplus_ngdp_q	−74.840	23.298	−2.758	5.598	−3.571	−2.876	−2.404
ele(0)	0	1	0.249	0.433	.	.	.
ele(1)	0	1	0.248	0.432	.	.	.
Latin America (19 countries)							
surplus_ngdp_a	−41.221	10.207	−2.455	4.474	−3.203	−1.827	−2.391
surplus_ngdp_q	−74.840	13.062	−2.269	5.338	−3.066	−2.125	−1.962
ele(0)	0	1	0.219	0.414	.	.	.
ele(1)	0	1	0.224	0.417	.	.	.
OECD countries (20 countries)							
surplus_ngdp_a	−22.055	9.054	−3.008	3.815	−3.490	−3.071	−2.753
surplus_ngdp_q	−70.604	23.298	−3.237	5.803	−3.974	−3.510	−2.925
ele(0)	0	1	0.273	0.446	.	.	.
ele(1)	0	1	0.268	0.443	.	.	.

Source: Authors' construction, based on economic variables from IFS for years in which *demo* = 1. Reported election data are based on SUNY and others and correspond to annual frequency. Normal years are neither election nor post-election years.

TABLE 4. Budget-Surplus-to-GDP Ratio, 1980–2005: Full Sample[a]

	FE estimates with f = a			*GMM estimates with f = a*			*FE estimates with f = q*		
Explanatory variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*
surplus_gdp_f(–1)	0.60***	0.60***	0.60***	0.45***	0.45***	0.44***	0.08**	0.08**	0.07*
	(0.06)	(0.06)	(0.06)	(0.11)	(0.11)	(0.11)	(0.04)	(0.04)	(0.04)
gdp_growth_f	0.11***	0.11***	0.11***	0.05	0.05	0.05*	0.16**	0.16***	0.17**
	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.06)	(0.06)	(0.07)
ele(0)	–0.44***			–0.27*			–0.62***		
	(0.16)			(0.16)			(0.18)		
ele(1)	0.63**			0.70**			0.31**		
	(0.25)			(0.33)			(0.12)		
pbc		–0.51***	–0.96***		–0.47***	–0.88**		–0.44***	–0.71***
		(0.15)	(0.33)		(0.17)	(0.36)		(0.10)	(0.13)
pbc_checks			1.07**			0.95*			0.82***
			(0.49)			(0.51)			(0.23)
Summary statistic									
No. observations	789	789	733	729	729	682	2,723	2,723	2,538
No. countries	39	39	38	39	39	38	39	39	38
R squared	0.453	0.452	0.464				0.512	0.511	0.519
Sargan test[b]				0.942	0.881	0.922			
2nd-order serial correlation test[c]				0.910	0.928	0.978			
ele(0) = –*ele*(1)?	0.522			0.242			0.173		
pbc = –*pbc_checks*?			0.600			0.733			0.511

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.
***Statistically significant at the 1 percent level.

a. Frequency *f* = *a, q*. Real GDP per capita, four quinquennial dummies, and a constant included. Conditional estimates include *checks*, and quarterly estimates include four lags of the dependent variable and country dummies for first, second, and third quarters. FE: fixed-effects estimates, with robust standard errors, clustered by country, in parentheses. GMM: Arellano-Bond one-step estimator; robust standard errors in parentheses. The instruments used in GMM regressions are two lags of the dependent variable. The covariates are treated as exogenous. Reported coefficients: lagged first difference dependent variable and first difference covariates.

b. *P* values for rejecting null in test overidentifying restrictions asymptotically distributed as χ^2 under null of instruments uncorrelated with residuals; *p* values from two-step GMM estimates.

c. *P* values for rejecting null in test second-order serial correlation in first-difference residuals asymptotically distributed as *N*(0,1) under null of no serial correlation; *p* values from two-step GMM estimates. *F* tests of hypotheses: *p* values reported.

terly data, the fixed-effects estimates are shown in the last three columns. These estimates cover the full panel of thirty-nine countries.

Column (1) decomposes PBCs into pre- and post-electoral effects. Opportunistic cycles are linked to expansions in election years, which are captured through the dummy variable *ele*(0), equal to one in election years and zero otherwise, while fiscal contractions after elections are captured by *ele*(1), the lead of *ele*(0). These dummy variables reflect the difference with normal times that are neither election nor post-election years. Pre-electoral effects are significant, but post-electoral effects are significant as well. The hypothesis that the effects cancel out, that is, that the coefficients of *ele*(0) and *ele*(1) are equal in absolute value and have opposite signs, cannot be rejected. Hence, column (2) presents the *pbc* dummy, which equals one in election years, negative one in post-election years, and zero otherwise.[27]

In column (3), we condition the variable *pbc* with *pbc_checks* = *pbc* ∗ *checks* to identify whether legislatures have a moderating influence on PBCs in countries where there is compliance with the law.[28] The effect of checks on executive discretion is significant. Since the restriction that the coefficients of *pbc* and *pbc_checks* are equal in absolute value is not rejected, this implies that if the legislature constitutes a veto player (*vetoplayer* = 1) and there is compliance with the law (*compliance* = 1), then an election year is not counted as such because PBCs are completely counteracted by legislative checks and balances.

In columns (4) through (6), we reestimate the behavior of the budget surplus around elections through GMM. We use the one-step estimator for statistical inference and the two-step estimator for the specification tests (the Sargan test and the second-order correlation test). Except for the pre-electoral coefficient *ele*(0), which is marginally significant and has a much smaller value (–0.27 versus –0.44), the other coefficients and their statistical significance are quite similar to the fixed-effects estimates. The Sargan test for the exogeneity of instruments does not reject the null hypothesis of instruments uncorrelated with the residuals.

Columns (7) through (9) present the results using quarterly data. The main advantage is that quarterly data allow us to identify the election year more precisely as the four quarters up to elections, rather than as the calendar year of elections. In column (7), both pre- and post-electoral effects are signifi-

27. Schuknecht (1996) introduces the variable *pbc* in his study of thirty-five developing countries over the 1970–92 period, where he posits that the fiscal expansion in election years is corrected after elections.

28. This procedure follows Streb, Lema, and Torrens (2009).

cant, and the restriction that these effects cancel out over the electoral cycle cannot be rejected. As temporal aggregation would lead us to expect, the pre-electoral effects and their significance are much stronger than with annual data: the coefficient of *ele*(0) is now highly significant, and its value of –0.62 more than doubles that with GMM in column (4). In column (9), checks and balances have a significant moderating influence on PBCs, and we cannot reject the hypothesis that legislative veto players prevent PBCs in countries with compliance with the law.

Estimates for the Regional Subsamples

We break the panel down into two regions, Latin America (table 5) and the OECD (table 6). As in the full sample, annual data do not allow rejecting the equality of pre- and post-electoral effects (column 1 of tables 5 and 6). However, post-electoral contractions are only significant in Latin America, while pre-electoral expansions are only significant in the OECD. Aggregate PBCs are stronger in Latin America; this pattern is consistent with the literature that points to stronger cycles in developing countries (column 2 of tables 5 and 6). When we control for effective checks and balances on executive discretion, however, the coefficients are similar in both regions (column 3 of tables 5 and 6). Stronger PBCs in Latin America can, in principle, be explained by weaker checks and balances, since the average value of *checks* is 0.07, versus 0.61 in the OECD. In the full sample (table 4, column 3), the coefficient of *pbc* is –0.96, and that of *pbc_checks* is 1.07, which implies an average impact of –0.89 = –0.96 + (0.07 ∗ 1.07) in Latin America and –0.31 = –0.96 + (0.61 ∗ 1.07) in the OECD. These average impacts approximate the unconditional coefficients of *pbc,* which are –0.84 and –0.29 (column 2 of tables 5 and 6).

When we look at the quarterly data, there are two striking differences. First, column (7) of table 5 shows that pre-electoral effects are highly significant in Latin America, with a value of –0.64 that almost doubles the GMM estimate in column (4). The fact that post-electoral contractions are also highly significant helps explain why pre-electoral effects are insignificant in column (4), because pre- and post-electoral effects partly cancel out with annual data. When we identify the electoral year more precisely with quarterly data, we detect significant pre-electoral effects. Since pre- and post-electoral effects have the same magnitude, the variable *pbc* in column (8) of table 5 fits the electoral cycle well.

Second, column (7) of table 6 shows that there is no trace of post-electoral fiscal adjustments in the OECD, and, unlike column (4), the hypothesis that

TABLE 5. Budget-Surplus-to-GDP Ratio, 1980–2005: Latin America[a]

	FE estimates with f = a			*GMM estimates with f = a*			*FE estimates with f = q*		
Explanatory variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*
surplus_gdp_f(–1)	0.45***	0.44***	0.44***	0.35***	0.33***	0.33***	0.08*	0.08*	0.07*
	(0.09)	(0.09)	(0.09)	(0.13)	(0.12)	(0.12)	(0.04)	(0.04)	(0.04)
gdp_growth_f	0.13**	0.13**	0.13**	0.09**	0.08**	0.08**	0.15**	0.15**	0.15**
	(0.05)	(0.05)	(0.05)	(0.04)	(0.03)	(0.03)	(0.06)	(0.06)	(0.07)
ele(0)	–0.47			–0.35			–0.64***		
	(0.27)			(0.31)			(0.22)		
ele(1)	1.19**			1.36**			0.59***		
	(0.51)			(0.69)			(0.15)		
pbc		–0.84**	–0.99**		–0.86**	–0.98*		–0.62***	–0.68***
		(0.31)	(0.42)		(0.41)	(0.50)		(0.13)	(0.12)
pbc_checks			1.43*			0.78			1.25***
			(0.75)			(0.78)			(0.20)
Summary statistic									
No. observations	359	359	325	338	338	308	1,372	1,372	1,229
No. countries	19	19	18	19	19	18	19	19	18
R squared	0.313	0.311	0.318				0.425	0.425	0.44
Sargan test[b]				1	1	1			
2nd-order serial correlation test[c]				0.900	0.827	0.774			
ele(0) = –*ele*(1)?	0.189			0.110			0.876		
pbc = –*pbc_checks*?			0.245			0.609			9E–7

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.
***Statistically significant at the 1 percent level.

a. Frequency *f = a, q*. Real GDP per capita, four quinquennial dummies, and a constant included. Conditional estimates include *checks*, and quarterly estimates include four lags of the dependent variable and country dummies for first, second, and third quarters. FE: fixed-effects estimates, with robust standard errors, clustered by country, in parentheses. GMM: Arellano-Bond one-step estimator; robust standard errors in parentheses. The instruments used in GMM regressions are two lags of the dependent variable. The covariates are treated as exogenous. Reported coefficients: lagged first difference dependent variable and first difference covariates.

b. *P* values for rejecting null in test overidentifying restrictions asymptotically distributed as χ^2 under null of instruments uncorrelated with residuals; *p* values from two-step GMM estimates.

c. *P* values for rejecting null in test second-order serial correlation in first-difference residuals asymptotically distributed as *N*(0,1) under null of no serial correlation; *p* values from two-step GMM estimates. *F* tests of hypotheses: *p* values reported.

TABLE 6. Budget-Surplus-to-GDP Ratio, 1980–2005: OECD Countries[a]

	FE estimates with f = a			*GMM estimates with f = a*			*FE estimates with f = q*		
Explanatory variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*
surplus_gdp_f(–1)	0.76***	0.77***	0.77***	0.58***	0.58***	0.62***	0.04	0.04	0.04
	(0.04)	(0.04)	(0.04)	(0.09)	(0.08)	(0.08)	(0.05)	(0.05)	(0.06)
gdp_growth_f	0.15**	0.15**	0.15**	0.05	0.05	0.08	0.21**	0.21**	0.22**
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.10)	(0.10)	(0.10)
ele(0)	–0.52**			–0.44**			–0.61**	–0.59**	–1.42
	(0.20)			(0.21)			(0.28)	(0.27)	(0.85)
ele(1)	0.11			0.10			–0.06		
	(0.24)			(0.19)			(0.11)		
pbc		–0.29*	–0.92***		–0.25**	–0.76***			
		(0.14)	(0.13)		(0.12)	(0.14)			
ele_checks									1.47
									(1.17)
pbc_checks			0.98***			0.78***			
			(0.27)			(0.27)			

(*continued*)

TABLE 6. Budget-Surplus-to-GDP Ratio, 1980–2005: OECD Countries[a] (*Continued*)

	FE estimates with f = a			*GMM estimates with f = a*			*FE estimates with f = q*		
Explanatory variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*	*(9)*
Summary statistic									
No. observations	430	430	408	391	391	374	1,351	1,351	1,309
No. countries	20	20	20	20	20	20	20	20	20
R squared	0.736	0.734	0.746				0.6	0.6	0.599
Sargan test[b]				1	1	1			
2nd-order serial correlation test[c]				0.773	0.163	0.244			
ele(0) = –*ele*(1)?	0.256			0.317			0.066		
ele(0) = –ele_checks?									0.911
pbc = –pbc_checks?			0.768			0.939			

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.
***Statistically significant at the 1 percent level.

a. Frequency *f = a, q*. Real GDP per capita, four quinquennial dummies, and a constant included. Conditional estimates include *checks*, and quarterly estimates include four lags of the dependent variable and country dummies for first, second, and third quarters. FE: fixed-effects estimates, with robust standard errors, clustered by country, in parentheses. GMM: Arellano-Bond one-step estimator; robust standard errors in parentheses. The instruments used in GMM regressions are two lags of the dependent variable. The covariates are treated as exogenous. Reported coefficients: lagged first difference dependent variable and first difference covariates.

b. *P* values for rejecting null in test overidentifying restrictions asymptotically distributed as χ^2 under null of instruments uncorrelated with residuals; *p* values from two-step GMM estimates.

c. *P* values for rejecting null in test second-order serial correlation in first-difference residuals asymptotically distributed as $N(0,1)$ under null of no serial correlation; *p* values from two-step GMM estimates. *F* tests of hypotheses: *p* values reported.

pre- and post-electoral effects cancel out is now rejected. Since post-electoral effects are significant only in Latin America, column (8) of table 6 employs the variable *ele*(0). The magnitude of pre-electoral effects is now similar in both regions. The reason the PBCs appear smaller in the OECD than in Latin America, in the estimates with annual data (column 5 of tables 5 and 6), is that in the OECD the coefficient of *pbc* averages out the significant effect of *ele*(0) with the insignificant one of *ele*(1).

The estimates for Latin America that condition cycles on effective checks and balances on executive discretion are similar to the full sample, except that effective checks and balances seem to more than counteract PBCs (column 9 of table 5). Since we do not detect post-electoral fiscal adjustments in the OECD, we present the conditional estimates with *ele*(0) alone (column 9 of table 6). Though not very precisely measured (these effects are not statistically significant unless we isolate the discretional component of cycles), they suggest that if there is weak compliance with the law or unified government, the deficit in election years increases more in the OECD than in Latin America. The fact that there are no post-electoral contractions implies that PBCs led to the buildup of debt in the OECD over this period.[29]

Robustness Tests for Quarterly Estimates

The dependent variable in our quarterly panels—namely, the ratio of the budget surplus to GDP—relies on our estimate of quarterly GDP. The distributed quarterly GDP series introduces a potential problem of errors in variables, which may bias the results due to its use in the lags of the dependent variable. With a lagged dependent variable measured with error, as in our case, the estimates may be inconsistent even for those estimators that belong to variables without measurement error (here the relevant variables are the political dummy variables).

To get around this problem in the quarterly estimates, we first replace the dependent variable and its lags by the ratio of the budget surplus to public expenditure (columns 1–3 of tables 7, 8, and 9). The specific variable we adopt, 100 * ln(1 + *surplus/expenditures*), is not affected by measurement error in quarterly GDP.

29. In contrast, Streb, Lema, and Torrens (2009) detect significant post-electoral contractions in OECD countries using the Brender and Drazen (2005) panel of democracies. Since this panel covers the 1960–2001 period, the different results might reflect the fact that in the 1960s and 1970s, a stricter fiscal discipline was in place or access to capital markets was more limited.

TABLE 7. Robustness Tests, 1980–2005: Full Sample[a]

	Budget surplus/expenditure ratio FE estimates			*Budget surplus/GDP ratio IV estimates*			*Law as categorical variable*	
							FE	*IV*
Explanatory variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
ele(0)	−3.07***			−0.57***				
	(0.96)			(0.20)				
ele(1)	2.81***			0.52***				
	(1.03)			(0.20)				
pbc		−2.87***	−5.09***		−0.53***	−0.82***	−1.07***	−1.27***
		(0.59)	(0.88)		(0.12)	(0.17)	(0.23)	(0.29)
pbc_checks			6.12***			0.89***		
			(1.47)			(0.33)		
pbc_checks'							1.38***	1.65***
							(0.39)	(0.54)
Summary statistic								
No. observations	2,595	2,595	2,416	2,597	2,597	2,416	2,182	2,103
No. countries	39	39	38	39	39	38	36	36
R squared	0.546	0.546	0.552				0.504	0.428
ele(0) = −*ele*(1)?	0.874			0.860				
pbc = −*pbc_checks*?			0.235			0.751		
pbc = −*pbc_checks'*?							0.131	0.212

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.
***Statistically significant at the 1 percent level.

a. Quarterly frequency. Robust standard errors, clustered by country, in parentheses. Four lags of the dependent variable, real GDP per capita, real GDP growth, four quinquennial dummies, country dummies for the first, second, and third quarters, and a constant included. Conditional estimates include *checks* or *checks'*. *F* tests of hypotheses: *p* values reported.

TABLE 8. Robustness Tests, 1980–2005: Latin America[a]

	Budget surplus/expenditure ratio FE estimates			*Budget surplus/GDP ratio IV estimates*			*Law as categorical variable*	
							FE	*IV*
Explanatory variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
ele(0)	−3.30**			−0.56*				
	(1.54)			(0.30)				
ele(1)	6.41***			1.08***				
	(1.69)			(0.30)				
pbc		−4.85***	−5.60***		−0.83***	−0.88***	−0.70**	−1.00**
		(0.85)	(0.97)		(0.18)	(0.20)	(0.28)	(0.42)
pbc_checks			7.84***			1.62		
			(1.46)			(1.14)		
pbc_checks'							0.41	0.80
							(0.85)	(1.30)
Summary statistic								
No. observations	1,313	1,313	1,174	1,312	1,312	1,173	1,106	1,062
No. countries	19	19	18	19	19	18	18	18
R squared	0.515	0.514	0.519				0.450	
ele(0) = −*ele*(1)?	0.271			0.279				
pbc = −*pbc_checks*?			0.002			0.496		
pbc = −*pbc_checks'*?							0.650	0.831

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.
***Statistically significant at the 1 percent level.

a. Quarterly frequency. Robust standard errors, clustered by country, in parentheses. Four lags of the dependent variable, real GDP per capita, real GDP growth, four quinquennial dummies, country dummies for the first, second, and third quarters, and a constant included. Conditional estimates include *checks* or *checks'*. *F* tests of hypotheses: *p* values reported.

TABLE 9. Robustness Tests, 1980–2005: OECD Countries[a]

	Budget surplus/expenditure ratio FE estimates			*Budget surplus/GDP ratio IV estimates*			*Law as categorical variable*	
							FE	*IV*
Explanatory variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
ele(0)	−3.22**	−2.92**	−5.51	−0.67***	−0.65***	−1.60***	−2.84**	−3.74***
	(1.19)	(1.16)	(4.06)	(0.25)	(0.24)	(0.53)	(1.31)	(1.12)
ele(1)	−0.96			−0.07				
	(0.60)			(0.25)				
ele_checks			4.80			1.70**		
			(5.79)			(0.82)		
ele_checks'							3.78*	5.06***
							(1.91)	(1.71)
Summary statistic								
No. observations	1,282	1,282	1,242	1,285	1,285	1,243	1,076	1,041
No. countries	20	20	20	20	20	20	18	18
R squared	0.643	0.643	0.645				0.564	
ele(0) = −*ele*(1)?	0.011			0.064				
ele(0) = −*ele_checks*?			0.735			0.829		
ele(0) = −*ele_checks'*?							0.177	0.053

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.
***Statistically significant at the 1 percent level.

a. Quarterly frequency. Robust standard errors, clustered by country, in parentheses. Four lags of the dependent variable, real GDP per capita, real GDP growth, four quinquennial dummies, country dummies for the first, second, and third quarters, and a constant included. Conditional estimates include *checks* or *checks'*. *F* tests of hypotheses: *p* values reported.

The significance of the coefficients is quite similar to the quarterly fixed-effects estimates in tables 4, 5, and 6. The restriction that post-electoral effects have the same magnitude and opposite sign to pre-electoral effects is even more strongly rejected for OECD countries. Though there are PBCs in both regions, post-electoral adjustments are significant only in Latin America. Once we control for effective checks and balances, the electoral effects in the OECD are much larger, but less precisely estimated (the coefficient is insignificant, becoming marginally significant only when a variable that measures the discretional component of cycles is used). In the OECD countries, the coefficient of *ele*(0) is similar to the coefficient of *pbc* in Latin America (tables 8 and 9, column 3). Since public expenditure (the scale factor) has a larger share of GDP in the OECD, this result again suggests that if there are no effective checks and balances, pre-electoral effects in terms of GDP are stronger in OECD countries.

The ratio of the budget surplus to public expenditure has the disadvantage of not being comparable to the rest of the literature, which looks instead at the ratio of the budget surplus to GDP. Another way to address the issue of errors in variables is an instrumental variables (IV) approach. The proposed instrument for the dependent variable is the ratio of the budget surplus to government revenues, which is not correlated with the measurement error in quarterly GDP, but is correlated to the dependent variable. The IV estimates are presented in columns (4) through (6) of tables 7, 8, and 9. Overall, the results resemble the quarterly fixed-effects estimates in tables 4, 5, and 6, but they provide firmer evidence that without checks and balances, pre-electoral effects are stronger in the OECD.

Finally, instead of using the compliance dummy, which treats compliance with the law as a fixed country characteristic and imposes a strict on/off condition, we consider the original ICRG index, a categorical variable that captures the variation over time of compliance with the law. The results for the variables of interest are comparable to the fixed-effects estimates with quarterly data, though the coefficients tend to be slightly larger (column 7 of tables 7, 8, and 9 versus column 9 of tables 4, 5, and 6). The same holds for the IV estimates (column 8 versus column 6 of tables 7, 8, and 9).

Other Issues

A possible concern is the issue of endogenous elections. While election dates are typically exogenous in presidential countries, in parliamentary countries the government might be tempted to call early elections in good

times, allowing positive exogenous economic shocks to substitute for policy stimulus.[30] Though this endogeneity leads to an overestimation of the effect of election years on political business cycles, in our regressions it produces an underestimation of political budget cycles, because the government does not need to resort to expansive fiscal policy during booms. Anticipated elections could also be the consequence of bad times, if this produces a crisis in the ruling coalition, but this is controlled for by the growth rate, which captures the cyclical component of the budget.[31]

Brender and Drazen single out the first four competitive elections, when voters have little experience and there is a learning process under way, from the following elections, to distinguish between new and established democracies.[32] They find significant PBCs only in new democracies. When we divide the countries into new democracies—where at least one election happens to be one of the first four competitive elections—and established democracies, *checks* has an average value of 0.08 in new democracies and 0.57 in established democracies. This variable might thus capture part of the differences between the two groups. Indeed, in the full sample an interactive dummy for established democracies, *pbc_established,* has a significant moderating influence on PBCs, but it becomes insignificant once we control for checks and balances (although *pbc_checks* is significant).

Alternatively, if we restrict the estimates to established democracies, PBCs are still significant, and the conditional effects are similar to the quarterly estimates for the whole sample (see table 10, columns 1–3). If we further restrict the estimates to established democracies in OECD countries, there is no trace of fiscal adjustment after elections, just like in the complete group of OECD countries; we therefore report the remaining estimates with pre-electoral effects only, even though the hypothesis that pre- and post-electoral effects cancel out is not rejected (columns 4–6). PBCs are still significant in this group. As a robustness check, we estimate the same regressions using instrumental variables. The results of the IV estimation are very similar in terms of significance and marginal effects to those obtained by fixed effects.

30. Kayser (2005).
31. If we restrict the estimates to presidential countries, PBCs remain significant.
32. Brender and Drazen (2005).

TABLE 10. Budget-Surplus-to-GDP Ratio, 1980–2005: Established Democracies[a]

	Established democracies			*Established democracies in OECD*		
Explanatory variable	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
ele(0)	−0.42**			−0.35*	−0.34*	−0.85**
	(0.16)			(0.18)	(0.19)	(0.37)
ele(1)	0.21			−0.02		
	(0.15)			(0.13)		
pbc		−0.30**	−0.68***			
		(0.12)	(0.23)			
ele_checks						0.85
						(0.57)
pbc_checks			0.81**			
			(0.35)			
Summary statistic						
No. observations	1,547	1,547	1,405	1,127	1,127	1,099
No. countries	22	22	21	17	17	17
R squared	0.595	0.595	0.619	0.701	0.701	0.706
ele(0) = −*ele*(1)?	0.306			0.122		
ele(0) = −*ele_checks*?						0.980
pbc = −*pbc_checks*?			0.460			

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.
***Statistically significant at the 1 percent level.

a. Quarterly frequency; fixed-effects estimates. Robust standard errors, clustered by country, in parentheses below coefficients. Four lags of the dependent variable, four quinquennial dummies, country dummies for the first, second, and third quarters, the natural log of real GDP per capita, the growth of real GDP, and a constant are included. Conditional estimates include *checks* or *checks'*. *F* tests of hypotheses: *p* values reported.

Going within the Electoral Window

Thus far, we have followed the convention in the literature of using the election and post-election years as the units of analysis. We replicated that with the quarterly data in the last section by imposing the restriction that the dummy *ele*(0) takes a value of one in the four quarters up to elections and zero otherwise, while *ele*(1) takes a value of one in the four quarters that follow and zero otherwise. Alt and Lassen use quarterly data to study PBCs in the fifteen OECD countries (out of nineteen) for which information is available over the 1989–98 period.[33] Their quarterly and annual estimates are not comparable, however, because they do not use the quarterly data to identify the election year as the four quarters up to elections. Hence, their study does not allow addressing the issue of temporal aggregation analyzed above.

33. Alt and Lassen (2006b).

Next, we explore whether a two-year window is the correct window to use, or if the window should instead be wider or narrower. Besides identifying the duration of the electoral window, quarterly data allow determining the timing of PBCs around elections. In their estimates with quarterly data, Alt and Lassen distinguish the election window (the three quarters centered on the election) from the pre- and post-election window (the average of the next three quarters on either side).[34] Their election window is significantly negative, even without conditioning on the degree of transparency. On the other hand, the pre- and post-election window is not significant, which might be due to averaging negative effects before elections with positive effects afterwards (in their annual data, the post-election effects are positive, sometimes significantly so). We look into this in more detail now.

Instead of taking a nine-quarter window around elections and imposing a given structure to the data, we take a twelve-quarter window that details the behavior of each of the six quarters up to elections and the six quarters that follow. Since there are elections every 3.9 years, on average (4.4 years in Latin America and 3.5 in the OECD), the window becomes less informative as it widens because it becomes intertwined with other election episodes.[35]

The coefficients in table 11 for the whole sample (column 1) are insignificant in quarters –5, –4, 5, and 6, so they can be excluded from the analysis. The same happens in the OECD (column 9). In Latin America (column 5), quarter –4 is significant, but its positive sign indicates that it does not belong to the period of pre-electoral stimulus. On the other hand, the pre-electoral coefficients in quarter –3 are significant in the full sample and in the OECD, and the post-electoral coefficients in quarter 4 are significant in Latin America. These results point to an eight-quarter window around elections.

When we reduce the window in table 11 to the four quarters before and after elections, quarter –3 remains significantly negative in the whole sample (column 2) and the OECD (column 9), so this indicates a pre-electoral window of exactly one year. The behavior in the whole sample and both regions is pretty similar, and the equality of the coefficients of the pre-electoral dummies is never rejected. The election year can thus be represented by the dummy *ele*(0) used before, which takes a value of one in the four quarters up to elections and zero otherwise. However, since the effects in the election

34. Alt and Lassen (2006b).

35. In years –2 or 2 around presidential elections, there are mid-term legislative elections in Argentina, Chile (for two legislative elections), the Dominican Republic, and the United States. Parliamentary countries have more variability than presidential ones, and early elections have been called within a year.

quarter are about twice as large as the rest, an electoral dummy that instead takes a value of two in the election quarter tracks the actual behavior better.

As to the post-electoral dummies, the equality of the four post-electoral dummies is rejected. This is due, in part, to the fact that the adjustment starts in the second quarter after elections. Furthermore, though the first quarter does not have a significant coefficient, its negative value resembles the pre-electoral period, so it could alternatively be included there, a hypothesis that is not rejected for our data set. Alt and Lassen combine the first post-electoral quarter with the election quarter.[36] This could reflect delays in the new administrations taking control of the situation, as well as the interlude in some countries between election day and the inauguration of the new administration.

Even if we restrict ourselves to post-electoral quarters 2, 3, and 4, the data still reject the hypothesis that the dummies are equal in the whole sample. This is because the adjustment is not evenly spread out in Latin America (the adjustment in the second quarter is much larger than the following two quarters), while nothing seems to happen in the OECD. The data do not reject the hypotheses that the contractive effect of the second quarter is either three times as large as that of quarters 3 and 4 or four times as large (columns 2, 6, and 10).

Since the effects within each phase are not evenly spread out, we define *pre_ele_A* as taking values 1, 1, 1, 2 in quarters −3 through 0 (and 0 otherwise) and *pos_ele_A* as taking values 0, 3, 1, 1 in quarters 1 through 4 (and 0 otherwise). This reflects a strong separation between the election and post-election years. Alternatively, we can group the first post-electoral quarter with the pre-electoral phase, defining *pre_ele_B* as taking values 1, 1, 1, 2, 1 in quarters −3 through 1 (and 0 otherwise) and *pos_ele_B* as taking values 4, 1, 1 in quarters 2, 3, and 4 (and 0 otherwise). With both specifications, post-electoral effects are significant in Latin America, but not in the OECD. These specifications allow testing if pre- and post-electoral effects cancel out. While this hypothesis is rejected for both regions, in Latin America the net effect is small, and in the OECD the expansive pre-electoral effects are not counterbalanced by contractive post-electoral effects.

In summary, according to the pattern in the quarterly data in table 11, as in the previous literature that relies on annual data, it is correct to use an eight-quarter electoral window. The main potential difference with the literature is that the effects in the first quarter after elections are negative. Since these effects are statistically insignificant, they could be ignored. In this characterization, an expansionary election year is followed, in some cases, by a

36. Alt and Lassen (2006b).

TABLE 11. Behavior of Budget-Surplus-to-GDP Ratio around the Quarter of Elections (Quarter 0)

	Full sample			
Explanatory variable	*(1)*	*(2)*	*(3)*	*(4)*
ele_q(−5)	−0.10			
	(0.27)			
ele_q(−4)	0.35			
	(0.22)			
ele_q(−3)	−0.68**	−0.69**		
	(0.33)	(0.34)		
ele_q(−2)	−0.34	−0.39		
	(0.33)	(0.31)		
ele_q(−1)	−0.41	−0.40		
	(0.34)	(0.33)		
ele_q(0)	−1.04***	−1.12***		
	(0.38)	(0.36)		
ele_q(1)	−0.55	−0.57		
	(0.51)	(0.51)		
ele_q(2)	1.02***	0.97***		
	(0.29)	(0.28)		
ele_q(3)	0.44	0.45		
	(0.31)	(0.27)		
ele_q(4)	0.35	0.26		
	(0.36)	(0.34)		
ele_q(5)	−0.02			
	(0.23)			
ele_q(6)	0.17			
	(0.39)			
ele_pre_A			−0.49***	
			(0.15)	
ele_pos_A			0.35***	
			(0.09)	
ele_pre_B				−0.51***
				(0.13)
ele_pos_B				0.26***
				(0.07)
Summary statistic				
No. observations	2,723	2,723	2,723	2,723
No. countries	39	39	39	39
R squared	0.515	0.515	0.514	0.515
$q(-5)=q(-4)=q(-3)=q(-2)=q(-1)=q(0)$?	0.001			
$q(1)=q(2)=q(3)=q(4)=q(5)=q(6)$?	0.110			
$q(-3)=q(-2)=q(-1)=q(0)$?		0.185		
$q(1)=q(2)=q(3)=q(4)$?		0.077		
$q(-3)=q(-2)=q(-1)=.5q(0)$?		0.894		
$0.33q(2)=q(3)=q(4)$?		0.855		
ele_pre_A = −ele_pos_A?			4E−6	
$q(-3)=q(-2)=q(-1)=.5q(0)=q(1)$?		0.912		
$0.25q(2)=q(3)=q(4)$?		0.725		
ele_pre_B = −ele_pos_B?				5E−6

*Statistically significant at the 10 percent level.
**Statistically significant at the 5 percent level.
***Statistically significant at the 1 percent level.

a. Fixed-effects estimates. Robust standard errors, clustered by country, in parentheses below coefficients. Four lags of the dependent variable, four quinquennial dummies, country dummies for the first, second, and third quarters, the natural log of real GDP per capita, the growth of real GDP, and a constant are included. *F* tests of hypotheses: $q(t)$ stands for *ele_q*(*t*); *p* values reported.

	Latin America				OECD			
	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	−0.10				−0.09			
	(0.33)				(0.44)			
	0.82**				−0.02			
	(0.31)				(0.30)			
	−0.68	−0.76			−0.67*	−0.62*		
	(0.59)	(0.62)			(0.35)	(0.34)		
	−0.27	−0.34			−0.39	−0.44		
	(0.65)	(0.56)			(0.34)	(0.38)		
	−0.21	−0.22			−0.54	−0.53		
	(0.45)	(0.47)			(0.47)	(0.42)		
	−1.01**	−1.23**			−1.06*	−1.05*		
	(0.47)	(0.44)			(0.54)	(0.54)		
	−0.84	−0.93			−0.45	−0.41		
	(0.98)	(1.02)			(0.42)	(0.40)		
	2.04***	1.96***			0.16	0.11		
	(0.47)	(0.39)			(0.33)	(0.34)		
	0.80*	0.80*			0.04	0.06		
	(0.41)	(0.39)			(0.44)	(0.35)		
	0.78**	0.57			−0.10	−0.09		
	(0.36)	(0.35)			(0.58)	(0.56)		
	0.18				−0.19			
	(0.39)				(0.27)			
	0.16				0.29			
	(0.62)				(0.50)			
			−0.50**				−0.47*	
			(0.17)				(0.23)	
			0.66***				0.06	
			(0.14)				(0.10)	
				−0.59***				−0.45**
				(0.17)				(0.18)
				0.49***				0.04
				(0.09)				(0.07)
	1,372	1,372	1,372	1,372	1,351	1,351	1,351	1,351
	19	19	19	19	20	20	20	20
	0.436	0.435	0.434	0.434	0.601	0.601	0.600	0.599
	0.002				0.315			
	0.002				0.617			
		0.369				0.563		
		0.004				0.769		
		0.839				0.971		
		0.899				0.967		
			1E−5				0.027	
		0.916				0.989		
		0.636				0.967		
				0.0001				0.016

contractionary post-election year. Alternatively, we can group the effects in the first post-electoral quarter together with the expansive pre-electoral phase, which gives us a depiction of the election window as five quarters of fiscal stimulus, sometimes followed by three quarters of adjustment.

Implications and Final Remarks

This paper focuses on overcoming the problems of temporal aggregation by using quarterly data instead of annual data. First, it employs quarterly data to identify the election year not as the calendar year of elections, but rather as the four quarters leading up to elections. This avoids the problem of temporal aggregation, where expansive effects before elections may be cancelled out by contractive effects afterward, but at the same time maintains a temporal unit of analysis that makes it comparable with the previous literature based on cross-country panels and annual data. Second, it employs a twelve-quarter window to determine the exact duration of the election window, confirming that the relevant window is an eight-quarter window around elections. However, the significant expansive effects in the election year in Latin America and the OECD seem to linger in the first quarter after elections. In the second through fourth quarters after elections, there is a significant fiscal adjustment in Latin America, but no trace of it in the OECD.

Our results contradict a widespread consensus that PBCs are only a developing country phenomenon or a phase experienced by young democracies.[37] The rationale for this hypothesis is that voters in developed countries are fiscal conservatives who punish deficit spending.[38] These conclusions might have been affected by temporal aggregation. In our 1980–2005 panel with annual data, we uncover significant electoral stimulus in the OECD, but not in Latin America, a developing region with new democracies. Part of the explanation for this anomaly in terms of the conventional wisdom is that most cross-country panels define "normal" times as all years that are not election years, which differs from our definition of normal times as all but election and post-election years.[39] Post-electoral effects are significantly positive and large

37. See, for instance, Akhmedov and Zhuravskaya (2004).

38. Peltzman (1992). However, Alt and Lassen (2006b) find that if fiscal transparency is low, incumbents are tempted to use debt for electoral purposes even in OECD countries, due to the problems of asymmetric information about fiscal policy that voters face.

39. For example, Brender and Drazen (2005); Shi and Svensson (2006).

in Latin America. Consequently, if post-election years were grouped with all other nonelection years, the fall of the budget surplus in election years would become more significant, since the change would be measured against a larger average budget surplus in normal times.

This is only part of the explanation, however. Since the fiscal stimulus in Latin America is reversed after elections, the effects cancel out around elections if annual data are used. When we identify the election year as the four quarters leading up to elections, rather than as the calendar year, we instead detect significant pre-electoral effects in Latin America, despite the fact that normal times do not include the post-election year. This same problem of counteracting effects may have been at work in the earlier literature on PBCs that controlled for post-electoral effects using annual data. For instance, Persson and Tabellini detect significant post-electoral increases in the budget surplus in their full sample of democracies over the 1960–98 period, as well as in the subset with the best democratic institutions.[40] Likewise, Streb, Lema, and Torrens, who use the Brender-Drazen panel of democracies over the 1960–2001 period, find significant post-electoral increases in the budget surplus both in their full sample and in the subset of OECD countries.[41] In both studies, pre-electoral reductions in the budget surplus are barely significant, if not outright insignificant, which is precisely what one would expect from temporal aggregation.

We have controlled for the institutional differences between Latin America and the OECD, since countries in Latin America are more likely to face the credibility problems of an executive branch that faces no effective legislative veto players in election years. An appropriate institutional framework may prevent self-interest from directing actions to areas where private and social returns do not match.[42] In line with this emphasis on institutional constraints, Rose finds significant PBCs in U.S. states unless they prohibit the issue of public debt or require a popular referendum to authorize debt.[43] Our own evidence adds to this, confirming earlier results with annual data on the moderating role of effective checks and balances in PBCs.[44]

40. Persson and Tabellini (2003).

41. Streb, Lema, and Torrens (2009); Brender and Drazen (2005).

42. See North and Thomas (1973, chap. 1).

43. Rose (2006) finds there are PBCs in states without balanced budget rules or with weak balanced budget rules that allow borrowing, but she does not control for the effect of divided government in these cases.

44. In Streb, Lema, and Torrens (2009).

However, the main difference between the two regions in our data set is that we do not detect any significant post electoral adjustments in the OECD over the 1980–2005 period, with either annual or quarterly data. Furthermore, quarterly data squarely reject any compensating adjustment in OECD countries after elections, and we find pre-electoral expansions that are similar, on average, in the two regions. This reveals a new feature, namely, that unchecked executives in the OECD may behave more extremely than those in Latin America, perhaps because of greater ease of access to capital markets.

The evidence for Latin America, where pre-electoral fiscal expansions are cancelled out by the post electoral contractions, is consistent with the theoretical models of PBCs under asymmetric information, in which the executive branch can exercise full discretion over fiscal policy, but there is a post-electoral adjustment because debt is distortionary, so the net effect on public debt is nil.[45] These rational stop-go policies stand in stark contrast to myopic populist go-go policies of increasing the budget deficit without concern for future consequences.

Though democratic governments in Latin America over the 1980–2005 period fit a pattern of rational opportunistic manipulation, in which the economy is stimulated before elections and adjustment is implemented afterwards to avoid adverse long-term consequences, one has to be careful in evaluating the evidence.[46] This trend could instead stem from necessity, not virtue—that is, governments in Latin America might have had less leeway to pile up debt than those in the OECD over this period. Based on evidence mainly from the 1980s, Remmer stresses that reforms and adjustments were enacted after elections in Latin America to correct the policies of previous administrations that stimulated the economy until they ran out of resources and access to finance.[47] Additionally, Stein and Streb find that, over the 1960–94 period, the depreciation rate in Latin America rose after elections, a measure that allows reducing government expenditure in real terms.[48] Similar measures, like postponing hikes in regulated utility prices until after elections, have also been used.

The Shi-Svensson framework suggests an explanation for these differences: Latin America faced much larger borrowing costs and more limited

45. Shi and Svensson (2006); Alt and Lassen (2006a). This was first hypothesized by Schuknecht (1996). In Rogoff (1990), PBCs have no long-run impact on debt either, but this is by construction.

46. Nordhaus (1975).

47. Remmer (1993).

48. Stein and Streb (2004).

access to credit than OECD countries, so public debt was much more distortive over this period.[49] This points to two different sets of limits on economic policy: institutional limits put in place by the political system and economic limits put in place by capital markets. Though limited creditworthiness does not prevent the manipulation of fiscal policy, it does discourage the accumulation of public debt.

In brief, our simulations and econometric estimates show that temporal aggregation can lead to a serious underestimation of election year effects with annual data. Our results imply that studies of electoral cycles should be based on quarterly, not annual, data.

49. Shi and Svensson (2006).

Appendix: Data Description and Methodological Note

Table A1 lists the countries included in the study, disaggregated by regional subsample, together with the election observations and fiscal data available.

Distribution of Annual GDP at Quarterly Frequency

Quarterly GDP data are available for a few countries during short periods in the International Financial Statistics (IFS) of the International Monetary Fund (IMF), so we disaggregate annual GDP data at quarterly frequency using import data.

Real GDP and imports in constant dollars are $I(1)$ series, while their first differences are $I(0)$. In general, the residuals of the unrestricted regression in levels of real GDP against real imports follow a random walk, but when the first differences of these variables are used, the null of a random walk can be rejected according to augmented Dickey-Fuller (ADF) tests.[1]

Hence, we follow the approach proposed by Fernández when the residuals of the regressions in levels are nonstationary, but the first differences are stationary.[2] The methodology is to apply Denton's distribution technique to construct a high-frequency series from a low-frequency series, which is solved by minimizing a quadratic loss function, using the sum of the squares of the differences between the first differences of the series to be estimated and the first differences of the high-frequency series, subject to the constraint that the sum of the variations of the estimated high-frequency series must add up to the actual annual variation.[3] To distribute yearly real GDP on a quarterly basis, we used the coefficients of the restricted regressions of real GDP against imports in dollars, deflated by the U.S. consumer price index (CPI).[4]

As to nominal GDP, it is first deflated by the CPI and then distributed using imports in dollars, deflated by the U.S. CPI. The use of the CPI to deflate the nominal series is dictated by its availability on a quarterly basis. With our quarterly estimates of real GDP, the CPI is used to construct the

1. See table A2 of the working paper version at www.ucema.edu.ar/publicaciones/download/documentos/403.pdf.
2. Fernández (1981).
3. Denton (1971).
4. These coefficients were estimated using the MATLAB package (www.mathworks.com/matlabcentral/fileexchange/loadFile.do?objectId=15597) developed by Enrique Quilis.

TABLE A1. List of Countries, Elections, and Fiscal Data in the 1980–2005 Period[a]

Latin America[b]					*OECD*[c]				
Country	*ele(0) >0*	*demo ≥ 0*	*Budget surplus*	*Frequency*	*Country*	*ele(0) >0*	*demo ≥ 0*	*Budget surplus*	*Frequency*
Argentina	5	1983–	1980–2004	*a,q*	Australia	10	Old	1980–2002	*a,q*
Barbados	6	Old	1980–2004	*a,q*	Austria	7	Old	1980–96	*a,q*
Bolivia	7	1982–	1980–88, 1993–2005	*a,q*	Belgium	7	Old	All	*a,q*
Brazil	4	1985–	1980–94	*a,q*	Canada	7	Old	1980–2001	*a,q*
Chile	4	1989–	1980–2000	*a,q*	Denmark	9	Old	1980–2000	*a*
Colombia	6	Old	All	*a,q*	Finland	6	Old	All	*a,q*
Costa Rica	6	Old	1980–2002	*a,q*	France	6	Old	1980–97	*a,q*
Dominican Republic	7	1978–	1980–2000, 2002, 2004–05	*a,q*	Germany	8	Old	1980–98	*a,q*
Ecuador	6	1979–	1980–2004	*a,q*	Greece	9	1975–	1980–99	*a,q*
El Salvador	5	1984–	–	–	Iceland	6	Old	All	*a,q*
Guatemala	6	1986–	1980–83, 1985–2005	*a,q*	Ireland	8	Old	1980–2002	*a,q*
Guyana	5	Old	1980–97	*a*	Italy	6	Old	All	*a,q*
Honduras	7	1982–	All	*a,q*	Japan	9	Old	1980–93	*a,q*

(*continued*)

TABLE A1. List of Countries, Elections, and Fiscal Data in the 1980–2005 Period[a] (*Continued*)

Latin America[b]					*OECD*[c]				
Country	*ele(0) >0*	*demo ≥ 0*	*Budget surplus*	*Frequency*	*Country*	*ele(0) >0*	*demo ≥ 0*	*Budget surplus*	*Frequency*
Jamaica	6	Old	1980–85	*a,q*	Korea	5	1988–	1980–97	*a,q*
Mexico	4	1988–	All	*a,q*	Luxembourg	5	Old	1980–97	*a*
Nicaragua	4	1990–	1991–2005	*a,q*	Netherlands	8	Old	1986–2005	*a,q*
Panama	5	1989–	1980–2000	*a,q*	New Zealand	9	Old	1980–88, 1990–2000	*a,q*
Paraguay	6	1989–	1980–2001	*a,q*	Norway	7	Old	1980–2003	*a*
Peru	6	1980–99, 2002–	All	*a,q*	Portugal	9	1976–	1980–98	*a*
Trinidad and Tobago	7	Old	1980–89, 1993–95	*a*	Spain	7	1978–	All	*a,q*
Uruguay	5	1985–	All	*a,q*	Sweden	7	Old	1980–2000, 2002–05	*a,q*
Venezuela	5	Old	1980–2001	*a,q*	Switzerland	6	Old	All	*a,q*
					United Kingdom	6	Old	1980–99	*a,q*
					United States	7	Old	All	*a,q*

a. The 296 elections during the 1980–2005 period are reduced to 282 elections when the condition *demo* ≥ 0 is imposed; "Old" refers to countries that are established democracies; the rest are new democracies where at least one of the first four democratic elections appears. The symbols *a* and *q* denote annual and quarterly frequency; only countries with both annual and quarterly fiscal data are included in the study, so seven of the forty-six countries are dropped.

b. Barbados, Guyana, Jamaica, and Trinidad and Tobago have parliamentary systems, the rest presidential systems. Mexico, a member of the OECD since 1994, is included in Latin America.

c. Korea and the United States have presidential systems, the rest parliamentary systems.

nominal GDP series. The annual sum of the estimates of nominal GDP differs from the original series, so we apply a correction factor using the ratio between the estimated nominal GDP and the nominal GDP reported by the IFS to divide the estimated series. This correction factor ensures that the annual sum of the estimated series adds up to the actual annual figure; to make sure there were no jumps in the series, we reviewed the annual correction factors, finding them practically constant for each country.

Comment

Marcela Eslava: A few years ago, Lorena G. Barberia and George Avelino presented their paper "Do Political Budget Cycles Differ in Latin American Democracies?" in one of the panels that this journal holds twice a year. Jorge Streb and I shared the role of discussants for that paper. In the open floor discussion, several participants asked why the authors of the paper were using annual data, rather than quarterly data, to identify political budget cycles. Their very reasonable concern was that annual data were masking patterns of electoral manipulation that occurred at frequencies that did not match calendar years. Those of us who had been working on political budget cycles more closely knew that this was the common practice in the literature, so we pointed out the generalized use of this approach and the fact that it was due to the data being more generally available at the annual frequency. Though some attendants pointed out that the IMF did collect quarterly data, most of us simply moved on, satisfied with the answer that annual estimations were the literature's optimal approach, given constraints.

As it turned out, however, there was a better option, and Jorge Streb and his coauthors decided to try it. Here is the paper that does just that. The authors put together a data set on quarterly fiscal balances for a set of countries for which this was possible, and they then reestimated the effect of elections on those balances. They did it with a few twists with respect to the rest of the literature: they explored what the data say about the actual timing of political budget cycles, explored both pre-electoral and post-electoral changes in the balance, and checked whether the presence of effective checks and balances affects the pattern. They also looked at potential differences between Latin American and OECD countries—which, I speculate, are the two groups of countries for which they were able to gather data.

Their findings support the very motivation behind going into greater time disaggregation: political budget cycles are more precisely estimated when quarterly data are used, especially for the two separate subsamples of countries

under analysis. Results in the paper suggest that the eight quarters around elections are different compared to other quarters, which the paper refers to as normal times. In particular, compared with those times, there is a contraction of the surplus in the four quarters that lead to an election and a subsequent expansion in the quarters after the election. Political budget cycles, as characterized by this boom-bust pattern around the election, are concentrated in countries without effective checks and balances, a result that may help explain differences found in the literature between developed and developing countries or between new and established democracies. Meanwhile, the contrasting pre- versus post-election pattern is driven by what happens in Latin America; as for the OECD sample, the authors identify pre-electoral contractions without post-electoral recoveries.

All of the aforementioned results are interesting and important in terms of contributing to our understanding of political budget cycles. Compared with the rest of the cross-country literature, they provide much greater detail about the phenomenon on at least two dimensions: the exact timing of electoral manipulation of fiscal policy and the role of checks and balances. There are a few spots, however, where the paper seems to stretch conclusions and statements a bit beyond what would be granted by the empirical approach.

The most prominent of these statements is the authors' claim that "our results contradict a widespread consensus that PBCs are only a developing country phenomenon or a phase experienced by young democracies, . . . conclusions [that] might have been affected by temporal aggregation" and that their results therefore "imply that studies of electoral cycles should be based on quarterly, not annual, data." While it is indeed the case that, in contrast to much of the literature, the authors identify a pre-electoral contraction in their subsample of developed countries, there is no reason to believe that this apparent contradiction with the rest of the literature is driven by the use of quarterly data: the pattern of pre-electoral contraction is also present in their estimates with annual data. The contrast with previous findings is even more puzzling when one considers that what this paper estimates is a contraction of the fiscal surplus in the pre-electoral quarters, *with respect to quarters that are neither pre-electoral nor post-electoral.* Meanwhile, the literature has found no clear evidence that before elections the deficit rises *compared to the rest of times* in developed countries. If the comparison in this paper were between pre-electoral times and all other quarters, as in the rest of the literature, estimates would show an even starker contraction before elections (since in this sample post-electoral deficits are the lowest). I do not know what the origin for the contrast with previous findings in the literature is (it could be,

for example, the different country coverage, the different time period, or a different estimation strategy), but precisely because the paper does not identify what is driving those differences, readers must be extra cautious about treating the paper as effectively challenging the widespread view that pre-electoral times are not too different from the rest of times in developed economies.

A precise reading of the results also invites caution about other specific statements found throughout the paper. First, point estimates are compared across tables 5 and 6 to draw conclusions on the relative size of electoral changes, but the baseline level of the surplus is different across regions, making simple comparisons inappropriate. Second, the authors conclude that "if there is weak compliance with the law or unified government, the deficit in election years increases more in the OECD than in Latin America," but we do not know whether the weak levels of compliance described in this statement are indeed ever present for the sample of OECD countries. Finally, the authors state, "The reason the PBCs appear smaller in the OECD than in Latin America, in the estimates with annual data (column 5 of tables 5 and 6), is that in the OECD the coefficient of *pbc* averages out the significant effect of *ele*(0) with the insignificant one of *ele*(1)." However, to show that this apparent difference between the OECD and Latin America disappears if the post-election effect is not averaged out, they only eliminate the post-election dummy in the OECD estimation (and so compare an OECD estimation without the post-election dummy to a Latin American estimation with the post-election dummy).

These words of caution do not detract from the baseline message: this paper makes an important contribution by demonstrating that temporal aggregation masks political budget cycles, thus showing that the use of quarterly data is a promising and feasible strategy we should all consider following.

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