

Transparency, Political Polarization, and Political Budget Cycles in OECD Countries

James E. Alt Harvard University
David Dreyer Lassen University of Copenhagen

We investigate the effects of fiscal transparency and political polarization on the prevalence of electoral cycles in fiscal balance. While some recent political economy literature on electoral cycles identifies such cycles mainly in weak and recent democracies, in contrast we show, conditioning on a new index of institutional fiscal transparency, that electoral cycles in fiscal balance are a feature of many advanced industrialized economies. Using a sample of 19 OECD countries in the 1990s, we identify a persistent pattern of electoral cycles in low(er) transparency countries, while no such cycles can be observed in high(er) transparency countries. Furthermore, we find, in accordance with recent theory, that electoral cycles are larger in politically more polarized countries.

This article examines whether and how institutional transparency and the polarization of political parties affect the scope for electoral cycles in fiscal policy. We show how access to information about fiscal policy matters for the existence of electoral cycles in public finances. Conditioning on the degree of fiscal policy transparency, we find that cycles are present in a sample of 19 advanced industrialized OECD economies, all fully developed and by no means recent democracies. We also find, consistent with the same theory, that electoral cycles are larger in more politically polarized countries. We thus provide evidence that electoral cycles in fiscal policy are not a phenomenon confined to or driven by weaker and newer democracies.

Interest in the political business cycle, deliberate manipulation of economic policy instruments or outcomes in the vicinity of elections, is persistent. Originating with work on electoral cycles in unemployment and real income in the 1970s, many economists and political scientists investigated theoretical foundations for and empirical implications of the political business cycle. Evidence was mixed, and conclusions differed. Alesina, Roubini, and Cohen (1997) found post-election cycles in many

countries, conditional on partisan preferences in both outcomes and instruments, but found no evidence of “opportunistic” pre-election monetary or budget cycles.

Writing about U.S. pre-election cycles, Beck (1987) argued that the Federal Reserve did not cause political monetary cycles, but passively accommodated such cycles when they were fiscally induced. In a penetrating review of the literature, Drazen (2001) provides a formal model of such “active fiscal, passive monetary” cycles. He concludes that while there is little evidence of electoral cycles in outcomes, one might identify electoral cycles in policy instruments. Another insightful reviewer (Franzese 2002a) found evidence for *conditional* electoral cycles in outcomes and policy instruments (Clark and Hallerberg 2000; Clark et al. 1998; Rose 2006; Shi and Svensson 2002a, 2002b). We build on these studies, examining institutional and political conditions that foster electoral cycles in fiscal policy instruments.

One purpose of a political business cycle is to signal competence to voters. At the same time, a risk is that it alienates them. Some institutional arrangements or political and economic conditions may make engineering such a cycle easier or more difficult, or more or less worthwhile.

James E. Alt is Frank G. Thomson professor of government, Harvard University, 1737 Cambridge Street, N-307, Cambridge, MA 02138 (jalt@latte.harvard.edu). David Dreyer Lassen is associate professor of economics, University of Copenhagen, Studiestræde 6, DK-1455 Copenhagen K, Denmark (david.dreyer.lassen@econ.ku.dk).

The topic of the article came up in conversations with Guido Tabellini. We thank him, Allen Drazen, Rob Franzese, Shanna Rose and seminar participants at University of Copenhagen, EEA and Juan March Institute (Madrid), and Harvard University for comments and suggestions. Further, we thank Sona Nadenichek Golder for access to her data on political polarization. Work on the article was begun when Lassen was visiting CBRSS at Harvard University. He thanks them for their hospitality and gratefully acknowledges funding through an EPRU-network grant. The activities of EPRU are financed through a grant from the Danish National Research Foundation.

American Journal of Political Science, Vol. 50, No. 3, July 2006, Pp. 530–550

©2006, Midwest Political Science Association

ISSN 0092-5853

The publication of Persson and Tabellini's careful examination and claim to have "uncovered strong constitutional effects on the presence and nature of electoral cycles in fiscal policy" (2003a, 267) provided a big stimulus to the empirical examination of such cycles.¹ They argued that cycles were prominent in but not confined to presidential regimes. They also provided a more general empirical specification that included post-election (relative surplus) as well as pre-election (deficit) effects. This innovative respecification is apparently a big part of the difference between theirs and earlier estimates.

Recent work, described in more detail below, tended to rule out macropolitical budget cycles in advanced industrial democracies, despite the conditional findings mentioned above. Instead Persson and Tabellini (2003b, 2004), Shi and Svensson (2002a, 2002b), and Brender and Drazen (2005) find evidence of cycles only in broader samples that include weak and new democracies, echoing Hallerberg, de Souza, and Clark's (2002) finding of such cycles in EU Accession candidate countries.² The interpretation of these results is that voters are better able to monitor and evaluate the fiscal policy process in strong and mature democracies. This reduces the scope for electoral manipulation of public finances in comparison with newer democracies.

However, in contrast to those who say "[t]he evidence from developed countries is particularly weak" (Akhmedov and Zhuravskaya 2004, 1301), we show that among advanced democracies significant opportunistic electoral cycles are conditional on the transparency of budget institutions, as well as the polarization of political parties. Where institutions are less transparent, the cycle in fiscal balance appears, while we find no such electorally related movements in higher-transparency countries. Independent of transparency, cycles are more pronounced where political polarization is greater. These results for cycles in fiscal balance (with relatively greater deficits before and surpluses after elections) are robust to a variety of different specifications and control variables and hold for both a binary and a continuous transparency measure, as well as a number of alternative measures of polarization.

The article proceeds as follows. The next section is the theoretical argument, based on recent career-concerns models of electoral cycles, for how polarization affects the incentives and transparency affects the ability of incumbent politicians to run an electoral cycle in fiscal balance. Section three describes the data used in the empirical analysis. Section four presents the empirical specifications

employed as well as the main results for the magnitude of electoral fiscal cycles. We show that bigger cycles are evident in less transparent and more polarized systems. We examine the effects of other variables like the number of parties in government and extent of state-owned media, replicate the estimates with alternative measures and quarterly as well as annual data, and consider government debt as a determinant of fiscal balance. Section five concludes with a discussion of implications for future theoretical and empirical work.

Fiscal Transparency, Imperfect Information, and Electoral Manipulation of Fiscal Policy

Greater transparency is a way to create what Powell and Whitten (1993) called "clarity of responsibility." It eases the task of attributing outcomes to the acts of particular politicians. Naturally, incumbents want credit for delivering things valued by voters. Equally, incumbents want to dodge blame for things disliked by voters. Transparency may increase the visibility of an action which has both liked and disliked consequences. Even so, transparency also makes observers more able to distinguish effort from opportunistic behavior or stochastic factors "primarily by providing actors with greater or lesser degrees of certainty about the present and future behavior of other actors" (Hall and Taylor 1996, 939).

Fiscal transparency allows voters, interest groups, and competing political parties to observe—or infer with better precision—causes and consequences of a government's fiscal policy, either directly or through the media.³ The ability of observers, and ultimately voters, to separate politicians' opportunistic policy choices from ones with other motivations (whether social welfare or random) depends crucially on the nature of voters' decision-making process and the information available to them. The early generation of political business cycle models featured economic agents with adaptive expectations. Later work established the possibility of such cycles in a rational expectations framework. More recently still, attempts to explain the appearance of political business cycles have been based on models of imperfect information in a rational expectations framework (Brender and Drazen 2005; Lohmann 1998; Persson and Tabellini 2000, 2003b; Rogoff 1990; and Shi and Svensson 2002a).

Recent empirical work on electoral cycles employs data from many countries, including both developed and

¹Franzese (2002a) also reports pre- and post-election effects in social spending and deficits.

²See also Block (2001), Khemani (2004), and Schuknecht (1999).

³See Besley and Prat (2004) for a theoretical analysis of media freedom and transparency in principal-agent models.

developing countries, over as many as 40 years. Clark and Hallerberg (2000), mentioned above, and Clark (2003) show that electoral fiscal cycles exist in OECD countries with fixed exchange rates. Both Persson and Tabellini and Shi and Svensson find electoral cycles in fiscal policy instruments, conditioning, respectively, on constitutional regime and degree of democracy. Shi and Svensson proxy the public's ability to monitor politicians by media access, measured by radio ownership multiplied by freedom of broadcasting, and find that cycles are more prominent where incumbents are more able to hide fiscal policy from the public. In contrast, Brender and Drazen (2005) argue that the apparent presence of electoral cycles stems from the inclusion of new democracies in the sample.

But new democracies are overrepresented among countries with weak media, and indeed (in the case of Persson and Tabellini), presidential systems. Budget process transparency could foster either increased ability to discern manipulation (and connect it with elections) that Brender and Drazen say voters learn with experience or the increased information flow that Shi and Svensson say comes from freer and more accessible media.⁴ We do not model media effects on communication separately from the effects of transparency on information flow, so we disentangle these correlated effects below, empirically.⁵

Our full model is published elsewhere (Alt and Lassen 2006). For the sake of cumulation, we present just enough here to clarify the differences between our model and its predecessors (Persson and Tabellini and Shi and Svensson), and the consequences for analysis. In all cases, incumbents produce public goods primarily financed by tax revenue.⁶ Incumbents can incur deficits,

⁴Of course, it could also be that growing freedom of the press is the source of voter learning that reduces the scope for fiscal policy manipulation. Brender and Drazen argue that all actors of the political and electoral system gain experience in handling and extracting information from data, so politicians could become more adept at manipulation as voters become more discerning. Nannestad, Paldam, and Rosholm (2003) make a related argument that economic voting could develop over time for voters not accustomed to democracy, but find almost instantaneous adjustment when comparing native Israelis with immigrant Jews from former communist countries.

⁵Akhmedov and Zhuravskaya describe the Russian regions they study as "characterized by . . . non-transparent government" (2004, 1302). Using a single-item expert rating of "government" transparency, they estimate larger shifts in expenditures in the month before elections where transparency is lower. They fail to find comparable effects for media freedom.

⁶The predictions of the model with respect to cycles in tax revenues and expenditures depend crucially on the exact specification of the utility functions and production technologies, but the result regarding fiscal balance, which is the one to be considered below, holds more generally.

which increase current public goods provision for a given tax revenue (and, hence, a given level of private consumption), but are costly in terms of foregone future consumption. Increasing incumbent ability increases public goods provision. Crucially, according to the "career-concerns" specification used in these models, incumbent ability is unknown to both voters and politicians at the time public sector decisions are taken.

In the models of both Persson and Tabellini and Shi and Svensson, politicians seek reelection to enjoy office rents, and voters are concerned only with the competence of politicians. Voters want politicians in office who, everything else equal, have more ability in providing public goods. However, voters cannot necessarily observe either incumbent ability or the current debt level. They thus face a signal extraction problem, since they cannot separate the effects of ability and deficits on the provision of public goods. Politicians realize this and therefore find it optimal to incur deficits to appear more competent in the eyes of voters, even though they do not know their own competence.

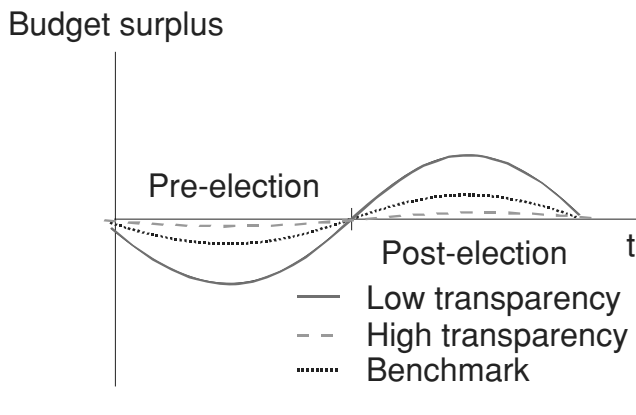
The role of transparency in these models is intuitive. Our budget process transparency measure contains items reflecting the amount, relevance, accessibility, and comprehensibility of timely information that becomes available to voters. We model transparency as the probability that voters observe the true level of deficits before the election. If that probability is one (complete transparency), voters can in principle deduce incumbent ability from the public budget constraint, and decide on reelection voting with full information. The greater the level of transparency, the less incumbent politicians gain in terms of expected votes by electioneering. Since running deficits is costly, the more budget practices are transparent, the smaller is the equilibrium level of pre-election deficits incurred by the incumbent.⁷ Figure 1 stylizes the expected time path of the budget surplus in high- and low-transparency regimes.

We also generalize the model by explicitly considering a trade-off voters face between policy positions and competence.⁸ Now, voters differ in their desired policies

⁷Besley (2004) and Besley and Smart (2003) consider the effects of transparency in a general political agency problem. Typically, transparency has both beneficial and adverse effects: beneficial, as transparency makes it easier to identify and throw out less competent politicians (the screening effect), but also adverse, as transparency reduces the incentives for less competent politicians to try to behave like more competent ones (the selection effect). In our case, only the screening effect remains as there are no personal costs to signalling for politicians; see Besley (2004).

⁸Even with this trade-off, Alt and Lassen find the basic insights of the other career-concerns models to be sustained.

FIGURE 1 A Hypothetical Example of Political Budget Cycles



and politicians seek election to implement their preferred policies as well as to gain rents. We assume parties differ with respect to a dimension of economic conflict, often treated as reflecting public ownership, redistribution, or public spending (Huber and Inglehart 1995). In our model of two parties or blocs, one (“Left”) prefers a larger scale of public good production than the other.

The inclusion of partisan differences in the model opens up another channel of influence on electoral cycles. Suppose “political polarization” is simply the distance or absolute difference between platforms of the parties. For convenience, we define that distance in a way that leaves the median voter’s assessment of party platforms unchanged, to focus the model on a pure polarization effect. A party seeing its opponent implement the opponent’s favored policy experiences a welfare loss. As polarization increases it becomes more desirable for each party to stay in office, since the utility loss felt by voters and politicians alike from seeing the opponent in office increases. The farther away the opponent’s platform, the greater is the utility loss from losing office, and the greater the debt the incumbent is willing to risk in order to be reelected.⁹

Put another way, since the loss from seeing the opponent party’s platform implemented increases with the distance between it and the incumbent’s own platform where polarization is higher, being in office is more valuable to the incumbent. This means that, other things equal, the

incumbent is willing to run a higher deficit, at higher costs, to convince the voters that she is competent. Therefore, in equilibrium politicians of both parties are willing to incur more debt to remain in office, other things equal. Thus, for both parties equilibrium debt accumulation increases as the degree of political polarization increases, and since the debt is incurred by assumption before elections, this leads to a bigger electoral cycle in deficits.

This analysis assumes that governments have “left” and “right” centers of gravity. With two parties there is no ambiguity. Some believe that our model of competition between two parties or blocs is not a good description of a system where competition often takes place among different coalitions of parties, which could matter for the dynamics of the electoral cycles. Key elements of the model are whether an incumbent can increase reelection chances by appearing competent and get the credit for appearing competent. The first should be true in coalitions and the second possibly diluted but still true. Is the logic of disliking the other “side” appropriate? Ex ante the incumbent’s choice is based on an expectation of its successor’s platform. When there are more than two parties, polarization reflects the distance to an average of possible successors. Confidence about the likely alternative is increased by frequent pre-election pacts (Golder 2004) but within a multi-party governing coalition, partners would not necessarily share the same expected distances. Also, this effect will diminish but not necessarily disappear for governments close to the “center.” Can parties bargain ex post to form a coalition? If yes, should polarization only include parties that could be in government? Probably—we deal with this problem in the empirical analysis below.¹⁰

Data

While fiscal transparency has been a key issue in fiscal policy debates for more than a decade, a comprehensive measure does not exist and attempts at definition have only recently been proposed. Our empirical work is based on a new replicable index of the transparency of the fiscal policy process, developed in Alt and Lassen (2006). The index aggregates responses to a detailed survey focused on formal requirements and procedures related to fiscal

⁹This result was obtained by Alesina and Tabellini (1990) and others, though with different parameter assumptions that restrict preferences. However, in their model debt accumulation occurs when one is likely to lose office. That is, the “pre-commitment” model has a different implication from ours for the effects of electoral closeness. However, closeness is endogenous in our model, and nonlinear in its effects, and too complex to analyze in this article.

¹⁰We avoid the issue of endogeneity of election dates in many of the countries in our sample. The sample is neither broad nor long enough to evaluate the potential endogeneity of election dates and its effects on the estimates. Brender and Drazen (2005) and Shi and Svensson (2002a, 2002b) find no differences between countries that have exogenous election dates and those that do not, and the former provide a clarifying explanation of why this is so.

policy. It thus improves significantly on earlier proxies of fiscal, or budget, transparency.¹¹ A good comprehensive definition is in Kopits and Craig, who write that

Fiscal transparency is defined . . . as openness toward the public at large about government structure and functions, fiscal policy intentions, public sector accounts, and projections. It involves ready access to reliable, comprehensive, timely, understandable, and internationally comparable information on government activities . . . so that the electorate and financial markets can accurately assess the government's financial position and the true costs and benefits of government activities, including their present and future economic and social implications.¹² (1998, 1)

The literature also provides specific examples of transparent budget reporting procedures:

“A transparent budget process is one that provides clear information on all aspects of government fiscal policy. Budgets that include numerous special accounts and that fail to consolidate all fiscal activity into a single ‘bottom line’ measure are not transparent. Budgets that are easily available to the public and to participants in the policy-making process, and that do present consolidated information, are transparent.” (Poterba and von Hagen 1999, 3–4)

Alt and Lassen (2006) construct a transparency index based on four distinct categories that all feature in the definitions and examples above. First, independent verification—for example, independently audited in-year financial reports—increases both the accuracy and persuasiveness of information. Second, more transparent procedures should ease access and monitoring by processing more information, and, other things equal, do so in fewer documents or places. In that way, supplementary budgets make it more difficult to discern total

amounts. Third, there should be a commitment to nonarbitrary language such that words and classifications have clear, shared, unequivocal meanings. The use of accrual accounting better ties expenditures and receipts together temporally. Finally, the presence of more justification of decisions solidifies the basis for decision making.

The index comprises 11 items, 10 of which are taken from a 1999 OECD questionnaire sent to all Budget Directors of OECD member countries (OECD 1999). Nine of these 10 variables are a part of OECD's *Best Practices for Budget Transparency* (OECD 2001). The original survey contained 76 items, many of which had nothing to do with transparency. Some questions were repetitive, and on others there was no cross-country variation. This left some 15 possible variables, of which 10 were ultimately included. All results are robust to the precise composition of the transparency index and the inclusion or exclusion of individual items. To the 10 were added a measure of whether financial statements are prepared using accrual accounting. The list of 11 items, with their question numbers in the original survey as appropriate, is in the appendix.

We aggregate the 11 items additively into an index, whose values range from a minimum of zero (Japan) to a maximum of 11 (New Zealand). Individual country scores are in Alt and Lassen (2006), Figure 1. The virtue of the survey data used to construct the index is that it focuses directly on transparency and is comprehensive, covering the entire spectrum of issues related to transparency. A potential drawback of the data is that it focuses on formal rules and procedures that may differ from actual practice. However, for most of the survey questions an accompanying question asked whether actual practices differed, which they did not in any case. Overall, the index is well in accord with the qualitative literature on budget procedures, including von Hagen's (1992) subjective assessment of transparency in eight European countries.¹³ The transparency variable has mean 4.2 and standard deviation 2.5.

Our main measure of party system polarization comes from an expert survey (Laver and Hunt 1992). Country specialists were asked to assign scores on a

¹¹Alt and Lassen (2006) show that transparency affected long-term debt levels. Here we focus on cyclical movements in the current fiscal balance, which relates directly to the underlying theoretical framework. Franzese (2002b) finds that for some specifications the combination of an electoral cycle in deficits with sluggish post-election adjustment entails rising long-term debt, conditional on a high frequency of elections.

¹²For a more detailed discussion of aspects of transparent financial reporting, refer to the IMF website on fiscal transparency (<http://www.imf.org/external/np/fad/trans/>).

¹³Germany receives a higher subjective assessment but relatively low scores on the index. Germany's budget process deteriorated in the 1990s, as more recent analyses stress (von Hagen and Wolff 2004). In this case, errors in the index would bias the results *against* supporting our predictions. We should add that, despite Stability and Convergence Programmes, EU countries do not report harmonized responses to items in the index, probably because the Programmes are not connected to annual budget processes (Hallerberg, Strauch, and von Hagen 2004). For example, many EU countries do not include the effects of various assumptions on budget outcomes and do not compare projected and actual expenditures, though most include projections beyond the next fiscal year.

20-point scale representing the parties' priorities between raising taxes to increase public services and cutting public services to cut taxes.¹⁴ Our variable is the standard deviation of party-by-party mean raw scores on this rating for each country.¹⁵ The index ranges from 2.2 (New Zealand) to 6.6 (United States), with a mean of 4.7 and a standard deviation of 1.1. We note that there is no statistical association at all between polarization and transparency.

To facilitate comparison with the literature referred to above, we use the data set from Persson and Tabellini (2003a, 2003b), to which we add further variables as noted. The key dependent variable is the central government fiscal surplus (and, also below, revenues and expenditures themselves) as a percentage of GDP, based on data from the *International Financial Statistics* database from the IMF. The average surplus for the period under consideration was -2.94 (that is, a deficit of just under 3% of GDP) with a standard deviation of 3.5.

The main explanatory variables, which appear in all the estimates reported below, are again based on the Persson-Tabellini data set, selected so as to facilitate comparison with the other studies. The key independent variables are binary election variables. We code PREELEX, the "(pre-)election year," as equal to one in the year of an election for the executive and zero otherwise, while POSTELEX, the "post-election year," is one in the year following an executive election and zero otherwise. Since the conjecture about the electoral cycle in fiscal balance is that deficits *relatively* expand before and contract after elections, the specification includes both pre- and post-election effects. Our sample comprises 19 OECD countries: Australia, Austria, Belgium, Britain, Canada, Denmark, Finland, France, Germany, Iceland, Ireland, Italy, Japan, Netherlands, New Zealand, Norway, Sweden, Switzerland, and the United States.

We start by replicating the Persson-Tabellini specification. To aid those replicating our work, we include these background results in Appendix A1, column 1. To this we add the stock of outstanding government debt, lagged one year (column 2). The rationale for this is that higher debt raises the interest costs on new debt, other things equal, and thus increases spending and pressure on fiscal balance (see Blais, Blake, and Dion 1996; Lowry and Alt 2001 and sources cited therein). The specification allows pre- and post-election effects on public finance outcomes

¹⁴Party-by-party raw scores appear in Laver and Hunt (1992, 136–312; Table 3 for each country). The number of respondents is in Table A1 (123). The party scale is in Table A2 (124).

¹⁵In case the standard deviation is unduly affected by the positions assigned to extreme (small) parties, we repeat the main analysis with an alternative measure that avoids this problem.

to vary across constitutional regimes (majoritarian versus proportional and presidential versus parliamentary), controlling for other factors. It also includes a lagged dependent variable (which is also interacted with indicators for presidential and majoritarian regimes), indicators for the calendar year in which an election occurred and the subsequent year. The other control variables include real income (GDP) per capita, in natural logs, share of population aged 15 to 64, and share above age 64, trade openness (exports plus imports as a share of GDP), and the output gap, a measure of country-idiosyncratic shocks constructed as the deviation from the real GDP country trend, computed using the Hodrick-Prescott filter.¹⁶

Empirical Specification and Results

How do electoral cycles in deficits differ between high- and low-transparency and more and less polarized countries? For clarity, we take the two key variables one at a time. Since the transparency index covers only the 1990s, we consider the period 1989–98 for the countries for which the index is available. For some countries not all years have complete data, resulting in a sample varying from 127 to 139 observations. We distinguish two cases. In one specification we consider a binary transparency index, achieved by splitting the sample into roughly equal groups of higher and lower transparency countries. This has the virtue of making results more robust to inclusion or exclusion of particular items of the transparency index and might also reduce measurement error from a judgment survey. In the other, we interact the election-year indicator variables with the continuous transparency index. For polarization, we employ both dichotomous¹⁷ and continuous measures in the same way.

Summary statistics are as follows. Table 1 presents the decomposition of average surplus by transparency regime and polarization, in election as opposed to nonelection years. For example, the average fiscal balance in all nonelection years was a deficit of just under 2.9% of GDP. By contrast, in election years, low-transparency countries on average had a deficit of 3.3% of GDP. The simple decomposition in Table 1(A) shows that deficits were slightly

¹⁶GDP, trade, and demographic data are from the World Bank's World Development Indicators. The Hodrick-Prescott filter is a statistical procedure that allows for detrending of macroeconomic time series, by separating them into a trend component and a cyclical component. Streb, Lema, and Torrens (2004) report substituting GDP growth rates for the output gap without qualitatively altering the results.

¹⁷The dichotomous measure yields two equal-sized groups of countries. Higher polarization countries include Britain, Denmark, France, Iceland, Ireland, Italy, Japan, Netherlands, and the United States. Data is unavailable for Switzerland.

TABLE 1(A) Surplus in Percent of GDP, by Transparency Regime and Election Year

	Election Year	Nonelection Year	All Years
High transparency	-3.05 (n = 24)	-3.01 (n = 69)	-3.02 (n = 93)
Low transparency	-3.32 (n = 17)	-2.62 (n = 44)	-2.82 (n = 61)
All countries	-3.16 (n = 41)	-2.86 (n = 113)	-2.94 (n = 154)

TABLE 1(B) Surplus in Percent of GDP, by Polarization Regime and Election Year

	Election Year	Nonelection Year	All Years
High polarization	-3.82 (n = 18)	-2.98 (n = 53)	-3.20 (n = 71)
Low polarization	-2.16 (n = 31)	-3.05 (n = 52)	-2.97 (n = 73)
All countries	-3.26 (n = 39)	-3.02 (n = 105)	-3.08 (n = 144)

larger in election years than nonelection years, and that high-transparency countries had slightly higher deficits. However, we also see that while the deficits across election and nonelection years are more or less the same for high-transparency countries, they differ more markedly for low-transparency countries, with deficits in election years being larger, if not significantly so, than in nonelection years.¹⁸ It is also immediate from Table 1(B) that higher polarization is associated with higher average deficits in election years, but lower deficits in nonelection years. To evaluate the size and significance of these differences in a multivariate setting, we turn to econometric analysis.

The basic specification allows the effects of lagged endogenous variables (persistence), economic shocks (SHOCK), and electoral cycles (as election year and post-election year effects) to differ between high and low regimes for transparency and polarization. Formally, to encompass these effects, we partition countries with variables H and L into two groups: $H_i = 1$ for high-

¹⁸A similar pattern obtains if we consider the post-election year variable (POSTELEX). In this case, in high-transparency countries there is no change in the deficit in post-election years, while low-transparency countries experience a lower deficit in post-election years relative to nonpost-election years.

transparency (or polarization) countries and zero otherwise and vice versa for L_i . Then we write the model as

$$Y_{it} = \alpha^0 Y_{it-1} + L_{it} \alpha^1 Y_{it-1} + \beta X_{it} + \gamma^0 SHOCK_{it} + L_{it} \gamma^1 SHOCK_{it} + H_{it} [\delta^0 PREELEX_{it} + \delta^1 POSTELEX_{it}] + L_{it} [\eta^0 PREELEX_{it} + \eta^1 POSTELEX_{it}] + \mu_i + \lambda_t + u_{it}$$

for $N = 1, \dots, 19$ and $T = 1989, \dots, 1998$. The principal quantities of interest, the electoral cycle effects,¹⁹ differ if $\delta^j \neq \eta^j, j = 0, 1$. X_{it} represents additional (time-varying) control variables noted above (including interactions between lagged balance and constitutional regime), and μ_i and λ_t capture country and year fixed effects, respectively.²⁰ In principle, the effects of the additional control variables could also differ by regime, but through repeated trials we found no signs that they do, echoing findings by Persson and Tabellini for the constitutional variables.

Ideally, the results do not depend qualitatively on whether the electoral cycle is interacted with transparency or polarization in continuous or collapsed form. To show this, we specify a second case with the continuous index. In the case of transparency, this is like the specification above apart from how the interaction and the election year variables are included:

$$Y_{it} = \alpha^0 Y_{it-1} + L_{it} \alpha^1 Y_{it-1} + \beta X_{it} + \gamma^0 SHOCK_{it} + L_{it} \gamma^1 SHOCK_{it} + \bar{\delta}^0 PREELEX_{it} + \bar{\delta}^1 POSTELEX_{it} + \bar{\eta}^0 PREELEX_{it} TRANS_i + \bar{\eta}^1 POSTELEX_{it} TRANS_i + \mu_i + \lambda_t + u_{it}$$

We use these two basic specifications to investigate the effects of transparency and polarization.

We still face a number of estimation issues. First, when the time dimension T is small, the standard fixed-effects estimator is biased in the presence of a lagged endogenous variable, which should be included in this case as fiscal balance is persistent over time. Therefore, we estimate the Arellano and Bond (1991) dynamic panel data model using first-differences that Judson and Owen (1999), based on Monte Carlo studies, recommend for macroeconomic models with small time samples.²¹ Note that since the

¹⁹We interact the election-year variables with *both* H and L (rather than just one or the other) for ease of presentation of the results, as will become clear below.

²⁰Because the estimation strategy we use employs first differences of both sides of the equation, the individual observation fixed effects disappear, but the year fixed effects remain and are jointly statistically significant.

²¹Judson and Owen (1999) acknowledge that the GMM model can be biased when the number of time periods is small. They argue that the bias is mainly on the lagged variable itself, with almost no bias on the X matrix, which is of more concern to us.

estimation is in first differences, all fixed effects are differenced out and do not enter into the calculation of conditional effects. We estimate the robust GMM model in Stata 8 and present the results. We also estimate but do not present the corresponding fixed-effects results and comment on differences between methods in the text.

Second, following Persson and Tabellini (2003a), a few observations display very large output shocks (more than 5% of GDP). Such extraordinary circumstances could possibly affect the “normal-period” estimates that we are seeking. Indeed, deleting these few extreme observations (typically no more than four in our sample) has some impact on results. Hence, for each pair of specifications we report results for two samples: an all-inclusive sample and a sample omitting observations with output shocks in excess of 5% of GDP.

Finally, we consider two different definitions of the electoral year. Suppose that incumbent governments manipulate public finances in order to increase the chances of reelection. Whether we can observe the evidence for such manipulation in public budget accounts is likely to depend on when the election was held. Persson and Tabellini define the election year as the calendar year in which the election was held and the subsequent calendar year as the post-election year. We refer to this as *calendar election year*. In a second set of results we redefine the *adjusted election year* as follows. If the election took place within the first quarter of a given year, the election year is the calendar year before the election actually took place, and, thus, the post-election year equals the actual calendar election year; other cases are not adjusted. For example, suppose an election took place in February 1995. In one set of our specifications (and all of Persson and Tabellini 2003b), the election year is 1995 and 1996 is the post-election year. In our alternative, “adjusted” definition, in this case, 1994 is the “election year” and 1995 the “post-election year.” This is still clumsy, so we attempt a partial replication below with quarterly data, but the finer data is not available for all countries or variables.

Therefore, we present the results of eight specifications that vary the transparency or polarization indicator (binary-continuous) by the sample (inclusive-omit largest shocks), and by electoral-year definition. We back these up with a full replication using fixed-effects regression. We also replicate all results using the data obtained from Brender and Drazen (2005) as an alternative. As specification checks, we compute Hansen’s *J*-statistic for the test for overidentifying restrictions, an extension of the Sargan-test statistic to include a robust error structure. We test for second-order serial correlation in the error terms which, if present, would render the estimator inconsistent. We report only the serial correlation test in tables,

as the former, possibly due to the large number of degrees of freedom, always indicates valid instruments (no overidentification). In fact, the empirical analysis, as can be seen from the tables, never rejects the hypothesis of no second order serial correlation in the errors.

Transparency and Political Budget Cycles

If we do not condition the election year effects on transparency regime (see Table [A]1, column 1), we find that the pre-election variable has coefficient -0.20 and standard error 0.25, while the post-election coefficient is 0.03 with standard error 0.24. The signs are consistent with theory, but the coefficients are not significant, echoing others’ results. Table 2 reports a summary of results from the most basic specification above, allowing electoral effects to differ between transparency regimes when we use the binary transparency indicator.

All regressions in Table 2 included the full set of controls. Full results for the first column of Table 2 appear in Table 1(A), column 2. Among what isn’t shown in Table 2, lagged debt is always positive, so the existence of a larger stock of debt, other things equal, predicts deficit reduction.²² Lagged debt is observable (at least as a forecast) when budgets are being done. The lagged dependent variable and its interactions with constitutional regime type are usually significant with expected signs, as is the log of income (positive for fiscal balance). Trade and demographic effects vary. In contrast to Persson and Tabellini, we find that the effect of the output gap is generally insignificant, maybe reflecting the inclusion of lagged debt.

Consider the first two columns of Table 2. The main result is that we find no indication of an electoral cycle in fiscal balance for the high-transparency countries. We consistently identify an electoral cycle in the low-transparency countries. Conditional on low transparency, the electoral indicators have the correct sign (negative for election year, positive for post-election year).²³ Election-year effects are consistently significant, though

²²Like all those whose data we use, we find that current year debt has no impact on the fiscal balance. Readers have pointed out that, as deficits are changes in debt, ours is an error-correction model. In that case the coefficient of lagged debt is of the right sign to indicate cointegration, but is not statistically significant in every specification (Dickey-Fuller test). Nothing in our model depends on deficits and debt being cointegrated, however.

²³We also omitted six countries with index values equal to 4 (Austria, Canada, Finland, France, Iceland, Sweden), to contrast the remaining five “high” countries (Australia, Netherlands, New Zealand, United Kingdom, United States) with the rest. In this “errors in variables” replication, coefficients increased by 10% and significance was unaltered.

TABLE 2 Electoral Cycles in Fiscal Balance: Binary Transparency Measure

	Surplus/GDP	Surplus/GDP	Surplus/GDP	Surplus/GDP
Pre-election year, low transparency	-0.5582* (0.2987)	-0.6359*** (0.2447)	-0.7595** (0.2987)	-0.8484*** (0.2523)
Post-election year, low transparency	0.5157 (0.3820)	0.6970* (0.3678)	0.4067 (0.2947)	0.5250** (0.2646)
Pre-election year, high transparency	0.0229 (0.3761)	0.1007 (0.3568)	0.0079 (0.3174)	0.2767 (0.3444)
Post-election year, high transparency	-0.0829 (0.2587)	-0.2715 (0.3088)	-0.1632 (0.3248)	0.0114 (0.3055)
	Estimated cycles			
Cycle, low transparency	1.0728** (0.4690)	1.3364*** (0.3956)	1.1721*** (0.4215)	1.3744*** (0.3461)
Cycle, high transparency	0.1054 (0.3387)	0.3742 (0.3777)	0.1699 (0.3001)	0.2856 (0.5629)
Sample Election Year	Full Calendar	ygap < 5 Calendar	Full Adjusted	ygap < 5 Adjusted
N	143	139	139	135
Serial correlation (p-value)	0.74 (0.43)	0.79 (0.43)	0.87 (0.38)	0.85 (0.40)
Method	GMM robust	GMM robust	GMM robust	GMM robust

Estimated using `xtabond` in Stata 8.0. Robust standard errors in parentheses. ***, **, * denote significance at 99%, 95%, and 90% levels, respectively. Controls included but not shown as described in main text.

post-election effects are significant only when we omit observations with large output shocks (column 2). The intuition behind the results is straightforward. Studies that do not find significant electoral cycles in fiscal balance in industrial countries lump together the data for high- and low-transparency countries. This averages the different effects, as was illustrated by the “benchmark” line in Figure 1. Once the transparency of the budgetary process is taken into account, we find electoral cycles in fiscal policy where we would expect: exactly where it is difficult for voters to see them, in countries whose budget-process transparency is lower.

Table 2, columns 3 and 4, report results from a similar pair of regressions, but with adjusted dates for elections occurring in the first quarter of a year. Statistical significance is identical to the results based on calendar year (columns 1 and 2). However, the size of the estimated effects is quite different. The pre-election decrease in the surplus is estimated to be between 35% and 50% larger when the election year is adjusted to the previous calendar year because the election occurs in the first quarter, and the post-election effect appropriately declines, though not by quite as much. The larger coefficient on the pre-electoral decrease in the deficit suggests that incumbents do in-

deed engage in electoral manipulation already in the year preceding the election, and our adjusted coding is more appropriate.

The main quantity of interest is what happens across the electoral cycle. Looking back at Figure 1, the magnitude of the electoral cycle is equal to how much the deficit expands (or the surplus declines) in the year before the election plus how much the deficit contracts (surplus expands) in the post-election year. The estimate of this quantity, which we will call “cycle,” for any context in Table 2, is minus the estimated pre-election shift in balance plus the post-election shift. For example, for the low-transparency countries in column 1, this sum of the interaction coefficients is $(-1) * (-.56) + (.51)$ or 1.07. For the high-transparency countries the analogous sum is $-.11$: small and in the wrong direction. Estimated this way, the magnitude of the cycle for low-transparency countries varies between 1.07 and 1.37 for the cases in Table 2.

How important, substantively or economically, is the estimated cycle? Bear in mind that the average (across all countries and years) absolute (regardless of direction) annual change in the fiscal balance has a magnitude of 1.21 (as a percent of GDP). However, the cycle estimated above contains *two* annual changes, so the estimated *annual*

TABLE 3 Electoral Cycles in Fiscal Balance: Continuous Transparency Measure

	Surplus/GDP	Surplus/GDP	Surplus/GDP	Surplus/GDP
Pre-election year	-1.1233 (0.7481)	-1.2174 (0.6972)	-1.3743** (0.6869)	-1.3942** (0.6135)
Post-election year	0.5334 (0.5231)	0.4337 (0.5472)	0.3187 (0.3994)	0.4948 (0.3900)
Pre-election year <i>times transparency</i>	0.2088 (0.1651)	0.2398 (0.1547)	0.2609* (0.1554)	0.3002** (0.1422)
Post-election year <i>times transparency</i>	-0.0793 (0.0904)	-0.0764 (0.0931)	-0.0508 (0.0735)	-0.0603 (0.0712)
Sample	Full	ygap < 5	Full	ygap < 5
Election Year	Calendar	Calendar	Adjusted	Adjusted
N	139	135	139	135
Serial correlation (p-value)	0.83 (0.41)	0.81 (0.42)	0.77 (0.44)	0.82 (0.41)
Method	GMM robust	GMM robust	GMM robust	GMM robust

Estimated using `xtabond` in Stata 8.0. Robust standard errors in parentheses. ***, **, * denote significance at 99%, 95%, and 90% levels, respectively. Controls included but not shown as described in main text.

equivalent of the low-transparency cycle in the first column of Table 2 is 0.54% of GDP. Across the table, these estimates range from .54 to .69 or between 45% and 57% of the average absolute change in the fiscal balance (1.21). These are substantively (as well as statistically) significant effects.

The indicator variables are not always individually significant. However, a correct significance test on “*cycle*,” the quantity of interest, requires taking into account the covariance between the parameter estimates for the election year and post-election year dummies which in this case is generally nonnegligible.²⁴ We carry out these tests by taking 10,000 draws from a bivariate normal distribution with means equal to the estimated coefficients for *election year* and *post-election year*, and variance and covariance equal to the relevant elements in the variance-covariance matrix of the estimates. We calculate from each draw a predicted cycle estimate as $-1 * (\textit{election year estimate}) + (\textit{election year estimate})$. The average value of this quantity over the 10,000 draws is shown in the tables as *cycle*, with its standard error observed across 10,000 draws. The *cycle* is indeed usually statistically significant and is well more than twice

²⁴Fixed-effects estimates reveal a similar pattern. In the FE estimations, however, the covariance between election and post-election year dummies is very large, leading to incorrect inferences based on individual coefficients. The estimated cycle, however, remains significant for all the specifications in Table 2.

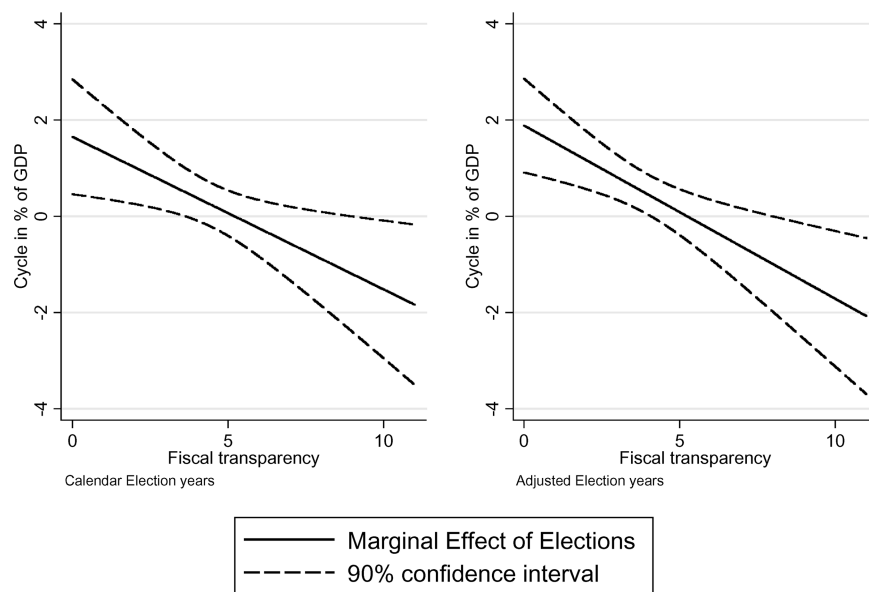
its standard error, four times in the case with the biggest shocks deleted and an adjusted election year. Note that because *cycle* is a linear combination of two regression coefficients, the statistical significance of *cycle* is a better substitute for the usual F-test on the joint significance of two coefficients, since it takes account of correct signs as well as magnitudes.

Table 3 reports a similar picture if we interact the continuous index, rather than the binary transparency measure, with the election year and post-election year indicators. Recall that in the specification without transparency, the electoral-year indicators and the estimated cycles are insignificant (Table 1[A]). However, once we include interactions with the continuous index, the election year and post-election effects become stronger, and the estimated interactions have the correct sign and are significant, conditional on using adjusted election years.

To clarify how transparency constrains incumbents from engaging in fiscal manipulation around elections, consider Figure 2. The two panels in the figure show, for the two definitions of election year, how the effect of elections on a political budget cycle in fiscal balance varies with the degree of transparency.²⁵ The panel on the right,

²⁵The effect of elections on the cycle was calculated in the following manner: First, as above, we simulated 10,000 draws from a four-dimensional normal distribution, with means and variance-covariance matrix equal to the estimated ones. From this, we

FIGURE 2 Transparency and Political Budget Cycles



reflecting smaller standard errors in the case of adjusted election years, shows that having an extreme score of zero on the transparency index implies that elections cause a cycle in the deficit of almost 2% of GDP, one which is significantly different from zero. The size of the estimate of *cycle* decreases as transparency increases. The electoral cycle disappears—that is, either the estimate has the wrong direction, or the confidence interval includes zero, or both—a little above a level of transparency of about 5, which in turn is just a little above the average level of transparency.

In results not reported, we replaced the Persson-Tabellini data with public finance data collected by Brender and Drazen (2005), with more observations for some, but not all, countries. In the binary transparency case, the estimated pre-election effect is larger and more significant, but the post-election effect is smaller and generally insignificant. However, the size and significance of the cycle are at roughly the same levels as before ($n = 164$).

constructed two cycle measures, *cycled* based on PREELEX and POSTELEX, capturing the direct effect of elections, and *cyclec* based on PREELEX * TRANS and POSTELEX * TRANS, capturing the conditional effect (TRANS is the continuous transparency index). From these, we calculated a total cycle measure, *totalcycle* = *cycled* + TRANS * *cyclec*, which is the solid line shown in Figure 2. The confidence interval was calculated on the total cycle using $\text{var}(\text{totalcycle}) = \text{var}(\text{cycled}) + \text{TRANS}^2 * \text{var}(\text{cyclec}) + 2 * \text{cov}(\text{cycled}, \text{cyclec}) * \text{TRANS}$. See Brambor, Clark, and Golder (2005) and Franzese and Kam (2005) for more on how to model and interpret interactive effects.

Results with the Brender-Drazen data are generally comparable to, though slightly weaker than, those reported in Tables 2 and 3. The main reason for the difference, we believe, is the fact that most of the additional data has been collected for countries with a relatively high degree of transparency.

Finally, lagged debt, included in all regressions above, significantly increases surpluses. However, whether lagged debt is included or not does not alter the effects of transparency on the electoral cycle. Including lagged debt does imply, in almost all specifications, that the output gap does not have a significant effect on deficits.²⁶ There may be a connection to Maastricht here, since our data is for the 1990s and includes a period when a few high-debt countries worked extra hard to reduce deficits and debt, but this does not show up in the data. The effect of lagged debt on deficits is not larger, or smaller, in countries participating in Maastricht. An investigation of whether countries participating in the Maastricht treaty behaved in a different way than non-Maastricht countries revealed nothing of significance on the electoral variables.²⁷

²⁶The output gap could itself be endogenous with respect to deficits. Then its estimated zero effect could be biased.

²⁷Countries participating in Maastricht did pursue more active stabilization policy in the period under consideration.

TABLE 4 Electoral Cycles in Fiscal Balance: Binary Political Polarization Measure

	Surplus/GDP	Surplus/GDP	Surplus/GDP	Surplus/GDP
Pre-election year, high polarization	-0.6764** (0.2265)	-0.6517** (0.2618)	-0.8596*** (0.2442)	-0.7998*** (0.2541)
Post-election year, high polarization	0.4263 (0.3021)	0.5709* (0.3420)	0.2689 (0.2707)	0.3481 (0.2852)
Pre-election year, low polarization	0.0787 (0.4522)	0.0763 (0.4426)	0.0556 (0.3805)	0.2910 (0.3777)
Post-election year, low polarization	-0.2271 (0.2542)	-0.5086 (0.3127)	-0.3379 (0.2892)	-0.1254 (0.2302)
	Estimated cycles			
Cycle, high polarization	1.1044*** (0.4231)	1.2223** (0.4846)	1.1292*** (0.3878)	1.1490*** (0.4473)
Cycle, low polarization	0.3040 (0.3767)	0.5878 (0.3785)	0.3933 (0.3102)	0.4173 (0.3231)
Sample Election Year	Full Calendar	ygap < 5 Calendar	Full Adjusted	ygap < 5 Adjusted
N	131	127	131	127
Serial correlation (p-value)	0.93 (0.35)	0.85 (0.40)	1.03 (0.30)	0.91 (0.36)
Method	GMM robust	GMM robust	GMM robust	GMM robust

Estimated using `xtabond` in Stata 8.0. Robust standard errors in parentheses. ***, **, * denote significance at 99%, 95%, and 90% levels, respectively. Controls included but not shown as described in main text.

Political Polarization

Does polarization promote electoral cycles in fiscal policy? As in the case of transparency, we begin by looking at a simple decomposition of the average surplus by polarization regime and election versus nonelection year. Table 1(B) suggests that deficits differ across polarization regimes: high-polarization countries have slightly higher deficits than low-polarization countries. The differences are more pronounced when conditioning also on election year. High-polarization countries have higher deficits in election years, while the opposite is true for the low-polarization countries. To see how that is affected by transparency and other controls, we again turn to econometrics.

Table 4 reports the regression results for the dichotomous polarization measure, with the specifications similar to those of Tables 2 and 3. All four columns provide strong evidence that more polarized countries experience larger electoral cycles in fiscal policy. Highly polarized countries increase public deficits before an election and decrease them afterwards, whereas low-polarization countries exhibit no significant effects. The value of *cycle*, for example, calculated in the same way

as above, is 1.22 with a standard error of .48 in the high-polarization countries in the smaller, calendar year sample with no large output shocks. This is strongly significant (the ratio of *cycle* to its standard error is above 2.5 in all four columns) and only slightly smaller in absolute terms than the effect of low transparency. The value of *cycle* for the low-polarization countries is usually greater than its standard error, but not significantly so. This is driven mostly by the post-election adjustment for these countries.

Does the polarization effect hold up independent of transparency? If we include both dichotomous transparency and dichotomous polarization, using the calendar year, large-shocks-omitted data, we obtain the cycle estimates shown in the first panel of Table 5. The electoral cycle appears prominently in the cases defined by high polarization and low transparency, which in our sample includes Denmark, Ireland, Italy, Japan, and Switzerland. The value of *cycle*, calculated as before, is larger than when either variable is considered on its own, and is nearly four times its standard error. In no other case is there a significant electoral cycle of the sort we contemplate. In fact, in the opposite case, low polarization with high transparency, the cycle goes (significantly) the wrong way, with

TABLE 5 Electoral Cycles Across Regimes

	High Transparency	Low Transparency	
High polarization	0.37 (0.53)	2.28*** (0.61)	N = 127
Low polarization	-0.90** (0.36)	0.58 (0.50)	
High State TV	-0.03 (0.78)	1.71*** (0.49)	N = 135
Low State TV	-0.61 (0.49)	0.57 (0.61)	
High NOP	-0.40 (0.49)	1.36*** (0.47)	N = 135
Low NOP	-0.70 (0.59)	1.24** (0.61)	

Each section is based on an individual regression, as described in the text. The regression featured the standard set of controls, a constant, and year effects. N denotes the number of observations. They were carried out using Stata's xtabond. The cycles were generated as explained in the text. Standard errors are reported in parentheses.

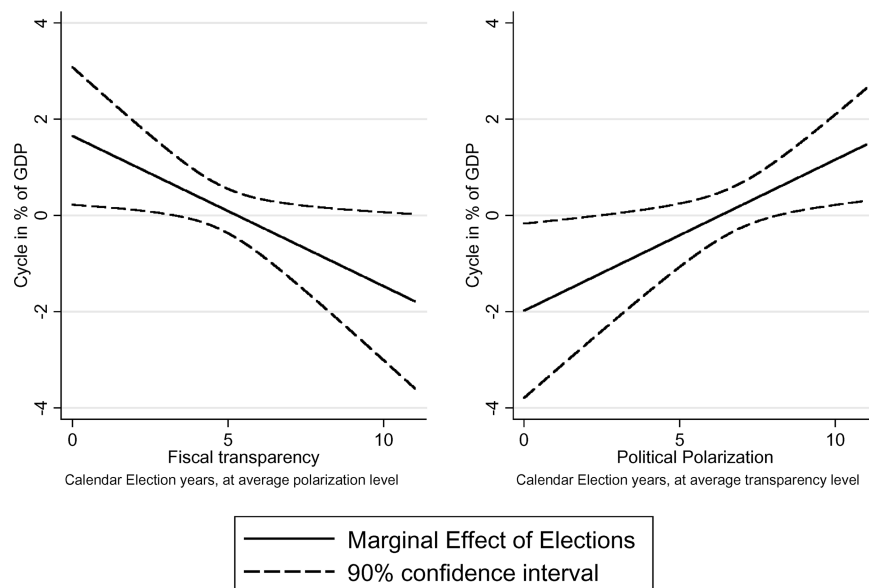
***, ** denote significance at the 99% and 95% levels, respectively.

surpluses rising before and falling after elections. Nevertheless, despite this, the separate but reinforcing effects of polarization and transparency are evident in the table: independent of polarization, the cycle is bigger where transparency is lower, and independent of transparency, the cycle is bigger where polarization is higher.

We have estimated (but do not show, except for an example in Table 1[A]) results for continuous measures of polarization and transparency analogous to Table 4. In this case, unlike the binary case, if we do not control for transparency but include the continuous polarization measure on its own, the results are correctly signed but nowhere near significant. However, once the continuous measure of transparency is included in the regression, the effect of transparency on the electoral cycle holds up when controlling for polarization, and the effect of polarization holds up when controlling for transparency. Higher polarization countries, everything else equal, have significantly higher deficits in election years, while higher transparency countries, again holding everything else equal, have significantly higher surpluses. These results hold up across the usual alternative samples and definitions.

Figure 3 presents an illustration of these results. The left panel shows, as above, the marginal effect of elections on the presence of a cycle as a function of transparency evaluated at the *average level of political polarization* (4.7). In comparison with Figure 2, the slope is more or less unaffected, but the effects are somewhat less precisely estimated, the confidence intervals being wider at the endpoints of the domain of transparency. The standard errors are only a little larger, consistent with the smaller sample and larger number of variables in the regression. In a similar way, the right panel shows the marginal effect of political polarization at the *average level of fiscal transparency* (4.2). The results remain significant—the 90%

FIGURE 3 Transparency, Polarization, and Political Budget Cycles



confidence interval does not touch zero—and both cases confirm the hypotheses suggested by the theory.²⁸

In addition to the Laver-Hunt measure of political polarization, we explored the data collected by Huber and Inglehart (1995), who follow the same methodology in sending out questionnaires to country experts. The correlation between the Laver-Hunt and Huber-Inglehart measures is positive but insignificant. We calculate the standard deviation of expert party scores for a country as an alternative measure of polarization. We find the regression results for the Huber-Inglehart measures (used in binary form for comparison) to be similar to those for the LH measure in terms of signs and size of the coefficients: the cycle in high-polarization countries is again strongly significant. Also, in results not shown, we find broadly similar, and often stronger (but sometimes weaker), results employing the Brender and Drazen data set.²⁹

Polarization should reflect positions of all parties except those that could never conceivably take office and whose positions do not influence the platforms of other parties. The Laver-Hunt measure of political polarization includes all parties and therefore may be biased as an estimate of an incumbent's platform's distance from that of the expected successor government (in the event of not being reelected). To deal with this we also explored a recent, alternative measure of political polarization, constructed by Golder (2004). She calculates political polarization as the absolute ideological distance between the *largest* left- and right-wing parties in the party system, which must all be possible, if not always probable successors, based on data from the Manifesto Research Group, which evaluates parties on a scale from -100 to $+100$ (Budge et al. 2001). The correlation between the two measures of polarization is not high because they are based on different concepts. The correlation between the Golder and Huber-Inglehart measures is lower still.

Nevertheless, we find that using Golder's measure of polarization yields many qualitatively similar results,

²⁸One referee suggested that low-transparency countries would have more veto players, or higher within-government polarization (that is correct) which would dispose incumbents toward (more targetable) fiscal cycles. Replacing party-system polarization with within-government polarization (Laver-Hunt measures) never gives stronger results and sometimes gives weaker ones, and leaves the transparency results unaffected. It might be the case, however, that incumbents with more within-government polarization maintain the pre-election stimulus, but cannot agree on the post-election correction. That intriguing possibility deserves further research.

²⁹For binary polarization, the pre-election effect is larger and $p < .0005$, while the post-election effect is numerically smaller but still close to significant ($n = 151$). Controls are largely unaffected, though both lagged debt and the output gap appear significant.

consistent with the Alt-Lassen model, whether we use calendar year or adjusted year concepts. In particular, whether we use the measure in discrete or continuous form, more polarized countries adjust fiscal policy towards lower deficits in the year following an election, consistent with the theory. This estimated post-election adjustment appears also when we control for measures of transparency. Indeed, when we include both Golder's polarization measure and transparency, we estimate a significant pre-election deficit driven by election timing. The picture is not quite as neat in every respect as we would like, and we have nothing firm to say about how transparency and polarization interact. Nevertheless, the preponderant evidence is that the political budget cycle is more pronounced in countries where the parties are further apart ideologically. Moreover, we can also infer that, at least for the case of continuous measurement, the magnifying effect of polarization on the political budget cycle exists alongside the dampening effect of transparency.

Further Controls, Alternative Data and Measures

This section considers two substantive confounding effects, control of media and multiparty government. It also reconsiders the validity of annual data by examining electoral fiscal policy cycles in a more limited sample of quarterly data.

The question is often raised of whether high transparency is sufficient to produce the effects we describe above, or whether there also has to be a free, active communications media to transmit to the public the information that transparency makes available, or indeed whether such media is itself sufficient regardless of institutional transparency. For example, extensive state ownership of media might present incumbents with the opportunity to control budget information in the same way we have attributed to low institutional transparency. Indeed, Shi and Svensson model transparency but use a proxy measure of media freedom in their empirical work.

To examine how media ownership and control relates to the effects of transparency, we consider the share of state-owned television stations in the aggregate market share of the five largest television stations by viewership in 1999 (from Djankov et al. 2003). In our sample of countries this variable ranges from 0% for the United States to 80% for Denmark. We again dichotomize this variable at the mean to yield two even-sized groups. The second panel in Table 5 shows that the "action" is mostly where there is high state-owned television and low transparency. In our sample, these countries include Denmark,

Germany, Italy, Switzerland, and Ireland. There is clearly some overlap between the effects of state-owned media and of transparency, though the form of the electoral cycle is still there with low state-owned television and low transparency.³⁰ However, high state-owned media independently increases the magnitude of the cycle, especially where there is low transparency. To better disentangle cause and effect, we will need panel measures of each.

Finally, it is also ultimately an empirical issue whether the reasoning of the model works for coalition governments. To explore this, we partition the high- and low-transparency countries into those with an above average number of parties in government (NOP) and those with below average NOP, using data taken from Franzese (2002b).³¹ This measure is time-varying, so the number of parties relates to the government in office during the year in which the election was held. Briefly, as the third panel of Table 5 shows, there is no difference between electoral cycles in cases where more or fewer parties are in office. The frequent occurrence of coalition governments cannot be taken on its own as a reason to reject our model.

Our main results are based on annual data. This is almost a universal standard in the political budget-cycle literature. We use annual data to facilitate comparison with the literature, to be certain that our results are not the product of different data but rather of the introduction of fiscal transparency and political polarization measures into the empirical specification. Annual data are not perfect, however. Several countries have fiscal years that differ from calendar years, and even though fiscal reporting typically is done such as to compare countries across calendar years inaccuracies can remain. Moreover, while our use of adjusted election years appeared to remove measurement error in some cases, it too was not perfect. Hence, we repeat some of the analysis with quarterly data. Quarterly data increases the number of time periods and makes it possible to align better fiscal data with the date of the election. On the other hand, sufficient data are not available for all countries in the sample for a reasonable set of control variables, or only for a very limited number of years. Therefore, the analysis below is based on 15 countries.³²

³⁰The continuous results weaken transparency and polarization somewhat, and state control of television is insignificant.

³¹We do likewise for the *effective* number of parties in government and find indistinguishable results.

³²Belgium, Denmark, Switzerland, and the Netherlands are excluded from the analysis altogether for lack of sufficient quarterly data. As the first three are low-transparency countries, the sample becomes unevenly divided across high- and low-transparency countries. Below, we investigate different classification schemes. Ireland and Iceland are in the sample, but only with a small number of cases; results do not depend on whether they are in or out.

Even with more precise data available, we must take into account that incumbents cannot always implement fiscal policy changes that take effect, or are reported to the public, exactly before the election takes place, or whose effects cease immediately when the election is over. Therefore, we implement a specification that distinguishes the three quarters centered on the election from the average of the next three on either side.³³ In this way, the fiscal variables are aligned with elections by quarters, but we still allow for some inaccuracy in the implementation of a possible budget cycle. The dependent variable is government net lending in percent of GDP, taken from the OECD Analytical Database, and we include controls for openness, the share of young and old in the population, (log of) GDP per capita and the output gap from the OECD Economic Outlook database;³⁴ general government debt is available on a quarterly basis only for a few countries in our sample and therefore excluded.

Table 6 reports the results. The first column shows results for the simple electoral cycle model. Unlike the case of annual data, there is evidence of a trough in the nine-month window surrounding an election even in the absence of a measure of transparency. The next column reports results for the full sample where we distinguish low- and high-transparency countries. We find that both types of countries decrease their surplus around elections, but that the effect is larger, if less precisely estimated, for low-transparency countries. If we leave out the greatly insignificant average measures from the quarters around the election window, the deficit induced by elections in the low-transparency countries is twice that of high-transparency countries; again, though, the difference is insignificant.

In columns 4 and 5, we report results from the smaller sample, where cases with large output gaps are excluded. Column 4 reports the case with averages before and after the election period. Here, the signs are as above and

³³When appropriate, we drop the surrounding quarters and consider the three election quarters only. The coded election quarters, in two-digit year/quarter format, are Australia (90.1, 93.1, 96.1, 98.4), Austria (90.4, 94.4, 95.4), Belgium (91.4, 95.2), United Kingdom (92.2, 97.2), Canada (88.4, 93.4, 97.2), Denmark (88.2, 90.4, 94.3, 98.1), Finland (91.1, 95.1), France (88.2, 93.1, 97.2), Germany (90.4, 94.3, 98.3), Iceland (91.2, 95.2), Ireland (89.2, 92.4, 97.2), Italy (92.2, 94.1, 96.2), Japan (90.1, 93.3, 96.4), Netherlands (89.3, 94.2, 98.2), New Zealand (90.4, 93.3, 96.4), Norway (89.3, 93.3, 97.3), Sweden (88.3, 91.3, 94.3, 98.3), Switzerland (91.4, 95.4), and the United States (88.4, 92.4, 96.4). Elections in 1988 were used only as post-election variables. All are for parliamentary elections except in the United States.

³⁴In the case of annual data, the output gap was based on Persson and Tabellini's measure, constructed using an HP-filter as described above. For the quarterly data, we take the output gap directly from *Economic Outlook*, where it is measured using a production function approach to potential output.

TABLE 6 Electoral Cycles in Fiscal Balance: Quarterly Data

	Surplus/ GDP	Surplus/ GDP	Surplus/ GDP	Surplus/ GDP	Surplus/ GDP	Surplus/ GDP	Surplus/ GDP
Election window	-0.17*** (0.06)					-0.27*** (0.08)	-0.17* (0.09)
Pre- and post- election window	-0.01 (0.04)						
Election window and low transparency		-0.24 (0.15)	-0.24** (0.09)	-0.34 (0.23)	-0.28** (0.13)		
Pre- and post- election window and low transparency		-0.00 (0.11)		-0.09 (0.16)			
Election window and high transparency		-0.14** (0.06)	-0.13*** (0.04)	-0.05 (0.06)	-0.03 (0.09)		
Pre- and post- election window and high transparency		-0.03 (0.05)		-0.03 (0.06)			
Election window <i>times</i> transparency						0.02** (0.01)	0.01 (0.01)
Sample Data	Full Quarterly	Full Quarterly	Full Quarterly	ygap < 5 Quarterly	ygap < 5 Quarterly	Full Quarterly	ygap < 5 Quarterly
N	428	428	439	371	381	439	371
Serial correlation (p-value)	0.72 (0.47)	0.73 (0.47)	0.76 (0.45)	0.77 (0.44)	0.81 (0.42)	0.75 (0.45)	0.81 (0.42)
Method	GMM robust	GMM robust	GMM robust	GMM robust	GMM robust	GMM robust	GMM robust

Estimated using xtabond in Stata 8.0. Robust standard errors in parentheses. ***, **, * denote significance at 99%, 95%, and 90% levels, respectively. Controls included but not shown as described in main text.

the difference between high- and low-transparency countries is now very large, but none of the coefficients are significant. Again, if we concentrate the analysis on the election quarters (column 5), the electoral deficit in low-transparency countries is significant, and the difference between deficits in the two types of countries is also significant, at the 10% level.³⁵

³⁵The results differ across the full sample and the sample with large shocks left out because the former includes cases from the high-transparency countries Finland and Sweden during their economic crises in the early 1990s. Lack of quarterly data for some countries makes the classification of countries more uneven. We carried out the analysis classifying countries with a transparency index value of 4 as low, rather than high. This yields stronger results for the low-transparency countries. We experimented with a three-way split into high-, medium-, and low-transparency countries. In this case, high-transparency countries never had significant electoral deficits, medium-transparency countries (including Finland and Sweden) had significant deficits in the large, but not in the small, sample, while low-transparency countries always had significant deficits.

Finally, results for the continuous transparency measure are reported in columns 6 and 7, concentrated on the three election quarters. The results on the full sample (column 6) show that more transparent countries have smaller electoral deficits; the coefficient on the interacted transparency variable implies that the electoral cycle in deficits all but disappears when the transparency index reaches its maximum level of 11.

As for polarization, we do not get clear results in the quarterly data (results not reported, available upon request). For the Huber-Inglehart measures, the results generally support the hypothesis that highly polarized countries have significantly larger electoral deficits. For the Laver-Hunt measure, the effect of elections on the deficit is virtually the same, and negative, for high- and low-polarization countries, in particular in the case when we consider only the three-quarter electoral window.

Discussion and Concluding Remarks

The main results of this article contribute to the growing body of evidence of conditional political budget cycles in industrial countries. A key finding is that the scope for electoral cycles in fiscal balance depends on the degree of fiscal policy transparency: the political budget cycle is where you can't see it. We show using a recent transparency-index measure on panel data for 19 advanced industrialized OECD economies over the 1990s that electoral cycles exist in low(er) transparency countries and that such cycles are statistically and economically significant. At the same time, we find substantive evidence of electoral cycles being induced by political polarization, a finding that in some cases reveals itself only after controlling for the level of transparency, but that turns out to reinforce the effects of low transparency. We also show that state-controlled media reinforce the effect of low transparency, but that the presence of electoral cycles does not depend on the number of parties in government. The results hold for alternative codings and data sets and furthermore are robust to the exclusion of particular countries. Much remains to be done, however.

One additional implication of the model still to be explored involves partisan choices of debt. The Alt-Lassen model allows for differences in parties' choice of debt, given transparency and platforms. Comparative statics depend on parameter values in the model, but in most cases the low-spending (right-wing) party chooses a higher equilibrium level of debt. That implies that the size of the cycle at an election would be conditional on the sequence of parties: bigger where Right parties give way to Left, for example.

We repeated the analysis with different dependent variables to see whether the electoral policy cycles identified above were driven by manipulating expenditures or revenues. We found few consistent and significant effects of fiscal transparency and political polarization on electoral cycles in revenues and expenditures separately.³⁶ We find no effects of transparency on expenditure levels. Possibly revenues decrease in election years in both high- and low-transparency countries, but subsequent post-election adjustment takes place only in high-transparency countries. High-polarization countries have higher expenditures in election years, and lower expenditures in post-election years, while the opposite is (weakly) true for low-polarization countries. Possibly voters observe changes in revenues and expenditures more readily than fiscal balance; the latter imposes the added difficulty

of combining information about revenues and expenditures. This is a subject to which we will return in future research.

We do not here address the issue of reverse causation, or endogeneity of transparency, the possibility that transparency is just a matter of politicians having nothing to hide. However, Alt and Lassen (2006) include an instrumental variables estimate of their cross-section results for transparency and debt, which suggests that their main results go through. We are addressing the incentive of politicians to create transparent institutions in a separate paper (Alt, Lassen, and Rose 2006) but any serious research into this issue requires a panel of transparency and polarization measures.

Why do incumbent governments in high-transparency countries refrain from running deficits before elections? If people can observe the deficits for what they really are, that is, opportunistic dead-weight losses, they should not reward such behavior on the part of incumbents. However, in the case of low-transparency countries, is it possible that voters do in fact reward deficit spending? An interesting extension would be to examine incumbent popularity conditioning on transparency regime. Just as the view of voters of fiscal conservatives (Peltzman 1992) may not hold up when conditioning on party governments (Lowry, Alt, and Ferree 1998) since voters expect parties to pursue different goals with respect to the size of government, the conclusion that voters do not reward deficit spending (Alesina, Perotti, and Tavares 1998) may be amended when conditioning on the political and institutional context. Allowing the impact of fiscal policy on approval ratings and electoral results to differ between high- and low-transparency regimes would make it possible to assess whether perceptions held on the demand side of the political economy (by the voters) match those on the supply side (politicians). This could be done both on our 19 country OECD sample, as well as on the U.S. states, where we have constructed a similar though not identical transparency index (Alt, Lassen, and Skilling, 2002). This is another topic for future research.

An additional subject long neglected in the analysis of political budget cycles is the role of regional and local governments. In many countries, the degree of decentralization of expenditures is greater than that of tax revenues, suggesting that central governments allocate expenditures to state and local governments even in countries where local taxation is mainly a matter for local governments. If central governments would engage in electioneering, it would be natural for them to distort the temporal pattern of both central and local spending, in order to maximize the probability of reelection. We plan to examine this, too, in future research.

³⁶Rose (2006) and Streb, Lema, and Torrens (2005) find electoral cycles in expenditures; Akhmedov and Zhuravskaya (2004) find shifts in revenues and in the composition of expenditures.

Finally, in a relevant, hardly lighthearted, effort to award a cycling prize between the current incumbent and an important figure from the past, Nixon, Rogoff wrote:

Presidents seeking a preelection boost can also run big deficits to increase domestic demand. Bush's high spending results from homeland security and "Iraqistan," whereas Nixon experienced the mother of all financial pits: Vietnam. Both presidents slashed taxes before their reelection campaigns (although Nixon recognized that the economy would pay for his profligacy later). The Nixon budget deficit in 1971 and 1972 was around 2 percent of gross domestic product (GDP); Bush's deficit exceeded 4 percent in 2003 and will likely reach 4 percent again in 2004. Advantage: Bush. (2004, 80)

As a final thought, consider the recent American election. On a Persson-Tabellini analysis, the United States, as a presidential system, should feature large electoral cycles in deficits. This amounts to the prediction that, other things equal, the deficit would be significantly reduced in 2005 (whether Bush was reelected or not). On our analysis, the United States, as a relatively transparent, not very polarized system, has smaller electorally induced cycles. On that view, the pre-election deficit was more structural than tactical. Hence our analysis predicts that, after controlling for other factors, the U.S. deficit will *not* be significantly reduced in 2005. We'll see what happens.

Appendix

The transparency index (question numbers from original survey report in parentheses) contains:

- *More information*, other things equal, in fewer documents
 - (Q. 18) Whether nonfinancial performance data is routinely included in the budget documentation presented to the legislature (yes = transparent)
 - (Q. 41) Whether special reports on the fiscal outlook are released prior to an election (yes = transparent)
 - (Q. 54) Whether the government regularly produces a report on the long-term (10-40 years) outlook for public finances as a whole (yes = transparent)
 - (Q. 67) Whether the government is required to report contingent liabilities on a regular basis (yes = transparent)
 - (Q. 15) Whether the government generally presents more than one supplementary budget to the legislature in each fiscal year (no = transparent)
- *Independent verification*
 - (Q. 37) Whether the in-year financial reports are audited (yes = transparent)
 - (Q. 48) Whether the economic assumptions used in the budget are subject to independent review (yes = transparent)
- *Nonarbitrary language*
 - Whether the government uses accrual accounting in its financial statements (yes = transparent)
- *More justification*
 - (Q. 27) Whether there is a legal requirement that the budget documentation contain a projection of expenditure beyond the next fiscal year (yes = transparent)
 - (Q. 29) Whether it is a legal requirement that the budget include an ex post comparison between projected expenditure in future years and the actual expenditures in those years (yes = transparent)
 - (Q. 52) Whether the budget discusses the impact that variations in the key economic assumptions would have on the budget outturn (yes = transparent)

APPENDIX A1 Selection of Full Regression Results

	(1)	(2)	(3)	(4)	(5)	(6)
	PT Replication Surplus/GDP	Unconditional (Our Sample) Surplus/GDP	Binary Transparency Surplus/GDP	Continuous Polarization Surplus/GDP	Binary Polarization Surplus/GDP	Continuous Transp. and Polar Surplus/GDP
Lagged surplus	0.74 (0.03)	0.60 (0.07)	0.57 (0.09)	0.60 (0.08)	0.55 (0.13)	0.85 (0.44)
Lagged surplus * majoritarian	-0.00 (0.04)	0.25 (0.11)	0.25 (0.11)	0.27 (0.11)	0.35 (0.16)	0.29 (0.13)
Lagged surplus * presidential	-0.26 (0.06)	-0.72 (0.15)	-0.66 (0.15)	0.69 (0.15)	-0.70 (0.23)	-0.59 (0.26)
Lagged surplus * (low) polarization					0.01 (0.13)	-0.06 (0.09)
Lagged surplus * transparency						0.16 (0.12)
Pre-election year	-0.19 (0.16)	-0.26 (0.27)		-1.12 (0.75)		0.25 (0.94)
Post-election year	0.42 (0.16)	0.13 (0.23)		0.53 (0.52)		-0.42 (1.42)
Pre-election year * low conditioning variable			-0.56 (0.30)		0.08 (0.44)	
Post-election year * low conditioning variable			0.52 (0.38)		-0.51 (0.31)	
Pre-election year * high conditioning variable			0.02 (0.38)		-0.65 (0.17)	
Post-election year * high conditioning variable			-0.08 (0.26)		0.57 (0.34)	
Pre-election year * continuous transparency				0.21 (0.17)		0.31 (0.15)
Post-election year * continuous transparency				-0.08 (0.09)		-0.00 (0.13)
Pre-election year * continuous polarization						-0.38 (0.21)
Post-election year * continuous polarization						0.12 (0.23)
Income per capita	0.35 (0.71)	21.2 (7.95)	22.24 (8.95)	20.79 (7.22)	13.27 (7.75)	14.18 (9.46)
Trade openness	0.02 (0.01)	-0.08 (0.04)	-0.06 (0.05)	-0.09 (0.04)	-0.08 (0.04)	-0.08 (0.05)
Population share between 15 and 64	0.09 (0.06)	-0.53 (0.63)	-0.48 (0.67)	-0.42 (0.58)	-0.00 (0.55)	-0.14 (0.68)
Population share above 65	0.11 (0.11)	0.10 (0.41)	0.13 (0.42)	0.08 (0.40)	0.29 (0.44)	-0.06 (0.54)
Shock	0.03 (0.05)	0.13 (0.14)	0.21 (0.19)	0.32 (0.29)	-0.12 (0.24)	0.59 (0.81)
Shock * majoritarian	0.01 (0.06)					
Shock * presidential	-0.17 (0.06)					
Shock * low conditioning variable			-0.37 (0.34)		0.54 (0.37)	
Shock * continuous transparency				-0.04 (0.05)		-0.38 (0.45)
Shock * continuous polarization						-0.06 (0.15)
Lagged OECD debt		0.11 (0.03)	0.10 (0.04)	0.12 (0.04)	0.08 (0.04)	0.07 (0.06)
Estimation	FE	GMM	GMM	GMM	GMM	GMM
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
N	1063	139	139	139	127	127
F/Wald	31.20	10881.22	1104.11	698.19	164.85	570.06
Sample	All shocks	All shocks	All shocks	All shocks	Shock < 5	Shock < 5

References

- Akhmedov, Akhmed, and Ekaterina Zhuravskaya. 2004. "Opportunistic Political Cycles: Test in a Young Democracy Setting." *Quarterly Journal of Economics* 119(4):1301–38.
- Alesina, Alberto, and Guido Tabellini. 1990. "A Positive Theory of Budget Deficits and Government Debt." *Review of Economic Studies* 57:403–14.
- Alesina, Alberto, Roberto Perotti, and José Tavares. 1998. "The Political Economy of Fiscal Adjustments." *Brookings Papers on Economic Activity* 1998(1):197–266.
- Alesina, Alberto, Nouriel Roubini, with Gerald Cohen. 1997. *Political Cycles and the Macroeconomy*. Cambridge: MIT Press.
- Alt, James E., and David Dreyer Lassen. 2005. "Fiscal Transparency, Political Parties and Fiscal Policy Outcomes in OECD countries." *European Economic Review*, forthcoming.
- Alt, James E., David Dreyer Lassen, and Shanna Rose. 2006. "The Causes of Fiscal Transparency." *IMF Staff Papers*, forthcoming.
- Alt, James E., David Dreyer Lassen, and David Skilling. 2002. "Fiscal Transparency, Gubernatorial Popularity, and the Scale of Government: Evidence from the States." *State Politics and Policy Quarterly* 2(3):230–50.
- Arellano, Manuel, and Stephen Bond. 1991. "Some Tests of Specification for Panel Data: Monte Carlo Evidence and an Application to Employment Equations." *Review of Economic Studies* 58(2):277–97.
- Beck, Nathaniel. 1987. "Elections and the Fed: Is There a Political Monetary Cycle?" *American Journal of Political Science* 31(1):194–216.
- Besley, Timothy. 2004. *Principled Agents: Motivation and Incentives in Government*. Draft book manuscript. London School of Economics.
- Besley, Timothy, and Anne Case. 2003. "Political Institutions and Policy Choices: Evidence from the United States." *Journal of Economic Literature* 41(1):7–73.
- Besley, Timothy, and Andrea Prat. 2004. "Handcuffs for the Grabbing Hand? Media Capture and Government Accountability." *American Economic Review*, forthcoming.
- Besley, Timothy, and Michael Smart. 2003. "Fiscal Restraints and Voter Welfare." Unpublished manuscript. London School of Economics.
- Blais, Andre, Donald Blake, and Stephane Dion. 1996. "Do Parties Make a Difference: A Reappraisal." *American Journal of Political Science* 40(2):514–20.
- Block, Steven A. 2001. "Elections, Electoral Competitiveness, and Political Budget Cycles in Developing Countries." Harvard University CID Working Paper No. 78, October.
- Brambor, Thomas, William Roberts Clark, and Matt Golder. 2005. "Understanding Interaction Models: Improving Empirical Analyses." *Political Analysis* 13(1):1–20.
- Brender, Adi, and Allan Drazen. 2005. "Political Budget Cycles in New versus Established Democracies." *Journal of Monetary Economics*, forthcoming.
- Budge, Ian, Hans-Dieter Klingemann, Andrea Volkens, Judith Bara, and Eric Tanenbaum. 2001. *Mapping Policy Preferences: Estimates for Parties, Electors, and Governments 1945–1998*. Cambridge: Oxford University Press.
- Clark, William R. 2003. *Capitalism, not Globalism: Capital Mobility, Central Bank Independence, and the Political Control of the Economy*. Ann Arbor: University of Michigan Press.
- Clark, William R., and Mark Hallerberg. 2000. "Mobile Capital, Domestic Institutions, and Electorally Induced Monetary and Fiscal Policy." *American Political Science Review* 94(2):323–46.
- Clark, William R., Usha Reichert, Sandra Lomas, and Kevin Parker. 1998. "International and Domestic Constraints on Political Budget Cycles in OECD Economies." *International Organization* 52(1):87–120.
- Djankov, Simeon, Caralee McLiesh, Tatiana Nenova, and Andrei Shleifer. 2003. "Who Owns the Media?" *Journal of Law & Economics* 46(2):341–82.
- Drazen, Allan. 2001. "The Political Business Cycle after 25 Years." *NBER Macroeconomics Annual 2000*. Cambridge: MIT Press.
- Franzese, Robert J., Jr. 2002a. "Electoral and Partisan Cycles in Economic Policies and Outcomes." *Annual Review of Political Science* 5:369–421.
- Franzese, Robert J., Jr. 2002b. *Macroeconomic Policies of Developed Democracies*. New York: Cambridge University Press.
- Franzese, Robert J., Jr., and Cindy Kam. 2005. "Modeling and Interpreting Interactive Hypotheses in Regression Analysis: A Brief Refresher and Some Practical Advice." Unpublished manuscript. University of Michigan.
- Golder, Sona Nadenichek. 2004. "Pre-Electoral Coalition Formation in Parliamentary Democracies." Unpublished manuscript. University of Iowa.
- Hall, Peter, and Rosemary Taylor. 1996. "Political Science and the Three New Institutionalisms." *Political Studies* 44(4):936–57.
- Hallerberg, Mark, Lucio Vinhas de Souza, and William Clark. 2002. "Political Business Cycles in EU Accession Countries." *European Union Politics* 3(2):231–50.
- Hallerberg, Mark, Rolf Strauch, and Jürgen von Hagen. 2004. "The Design of Fiscal Rules and Forms of Governance in European Union Countries." ECB Working Paper No. 419.
- Huber, John, and Ronald Inglehart. 1995. "Expert Interpretations of Party Space and Party Locations in 42 Societies." *Party Politics* 1(1):73–111.
- Judson, Ruth, and Ann Owen. 1999. "Estimating Dynamic Panel Models: A Guide for Macroeconomists." *Economics Letters* 65(1):9–15.
- Khemani, Stuti. 2004. "Political Cycles in a Developing Economy: Effect of Elections in the Indian States." *Journal of Development Economics* 73(1):125–54.
- Kopits, George, and Jon Craig. 1998. "Transparency in Government Operations." IMF Occasional Paper 158.
- Laver, Michael, and W. Ben Hunt. 1992. *Policy and Party Competition*. New York: Routledge.
- Lohmann, Suzanne. 1998. "Rationalizing the Political Business Cycle: A Workhorse Model." *Economics & Politics* 10(1):1–17.
- Lowry, Robert C., and James E. Alt. 2001. "A Visible Hand? Bond Markets, Political Parties, Balanced Budget Laws, and State Government Debt." *Economics & Politics* 13(1):49–72.

- Lowry, Robert C., James E. Alt, and Karen E. Ferree. 1998. "Fiscal Policy Outcomes and Electoral Accountability in American States." *American Political Science Review* 92(4):759–74.
- Nannestad, Peter, Martin Paldam, and Michael Rosholm. 2003. "System Change and Economic Evaluations: A Study of Immigrants and Natives in Israel." *Electoral Studies* 22(3):485–501.
- OECD. 1999. *Survey of Budgeting Developments—Country Responses*. PUMA/SBO (98)3/ANN.
- OECD. 2001. *OECD Best Practices for Budget Transparency*. PUMA/SBO (2000)6/FINAL, May.
- Peltzman, Sam. 1992. "Voters as Fiscal Conservatives." *Quarterly Journal of Economics* 107(2):327–61.
- Persson, Torsten, and Guido Tabellini. 2000. *Political Economics: Explaining Economic Policy*. Cambridge: MIT Press.
- Persson, Torsten, and Guido Tabellini. 2003a. *The Economic Effects of Constitutions*. Cambridge: MIT Press.
- Persson, Torsten, and Guido Tabellini. 2003b. "Do Electoral Cycles Differ Across Political Systems?" IGIER Working Paper n. 232.
- Persson, Torsten, and Guido Tabellini. 2004. "Fiscal Effects of Constitutions." *American Economic Review* 94(1):25–45.
- Poterba, James, and Jürgen von Hagen. 1999. "Introduction." In *Fiscal Institutions and Fiscal Performance*, ed. J. M. Poterba and J. von Hagen. Chicago: University of Chicago Press, pp. 1–12.
- Powell, G. Bingham, and Guy Whitten. 1993. "A Cross-national Analysis of Economic Voting: Taking Account of the Political Context." *American Journal of Political Science* 37(2):391–414.
- Rogoff, Kenneth. 1990. "Equilibrium Political Business Cycles." *American Economic Review* 80(1):21–36.
- Rogoff, Kenneth. 2004. "Bush Throws a Party." *Foreign Policy* 141(March/April):80–81.
- Rose, Shanna. 2006. "Do Fiscal Rules Dampen the Political Business Cycle?" *Public Choice*, forthcoming.
- Schuknecht, L. 1999. "Fiscal Policy Cycles and the Exchange Rate Regime in Developing Countries." *European Journal of Political Economy* 15(3):569–80.
- Shi, Min, and Jakob Svensson. 2002a. "Conditional Political Business Cycles." CEPR Discussion Paper #3352.
- Shi, Min, and Jakob Svensson. 2002b. "Political Business Cycles in Developed and Developing Countries." Unpublished manuscript. Institute for International Economic Studies.
- Streb, Jorge, Daniel Lema, and Gustavo Torrens. 2005. "Discretionary Political Budget Cycles and Separation of Powers." Unpublished manuscript. Universidad del CEMA.
- von Hagen, Jürgen. 1992. "Budgeting Procedures and Fiscal Performance in the European Community." *EEC Economic Papers* 96.
- von Hagen, Jürgen, and Guntram B. Wolff. 2004. "What Do Deficits Tell Us About Debt? Empirical Evidence on Creative Accounting with Fiscal Rules in the EU." Discussion paper 37/2004, Deutsche Bundesbank.