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## **The Political Budget Cycle is Where You Can't See It: Transparency and Fiscal Manipulation**

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**The Political Budget Cycle is Where You Can't See It:  
Transparency and Fiscal Manipulation<sup>†</sup>**

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Abstract:

We investigate the effects of fiscal transparency and political polarization on the prevalence of electoral cycles in fiscal balance. The 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 also of advanced industrialized economies. Using a sample of nineteen OECD countries in the 1990's, 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 more politically polarized countries.

Keywords: fiscal transparency, political polarization, fiscal policy, budget deficits, political budget cycles, electoral policy cycles

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## 1. Introduction

This paper examines whether and how the institutional transparency of fiscal policy affects the scope for electoral cycles. 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 nineteen advanced industrialized OECD economies, all fully developed and by no means recent democracies. Furthermore, we find, consistent with the same theory, that electoral cycles are larger in more politically polarized countries. Thus we show 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 et al. (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 US 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 2002) found some positive evidence for *conditional* electoral cycles in both outcomes and policy instruments (Clark et al. 1998; Clark and Hallerberg 2000;

Shi and Svensson 2002a, 2002b; Rose 2004). We build on these studies, examining the institutional conditions under which an electoral cycle in fiscal policy instruments exists.

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. 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.<sup>1</sup> They argued that such 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 re-specification is apparently a big part of the difference between theirs and earlier estimates.

More recent work by Persson and Tabellini (2003b, 2004), Shi and Svensson (2002a,b) and Brender and Drazen (2004), described in more detail below, tended to rule out macro-political budget cycles in advanced industrial democracies. Instead they find evidence of cycles only in broader samples that include weak and new democracies. This echoes Hallerberg et al.'s (2002) finding of such cycles in EU Accession candidate countries.<sup>2</sup> 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 in turn reduces the scope for manipulation of public finances for electoral purposes 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), we show that among advanced

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<sup>1</sup> Franzese (2002) also reports pre- and post-election effects in social spending and deficits.

<sup>2</sup> See also Akhmedov and Zhuravskaya (2004), Block (2001), and Khemani (2004).

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 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. Our empirical work is based on a new replicable index of the transparency of the fiscal policy process, developed in Alt and Lassen (2005). The index aggregates responses to a detailed survey focused on formal requirements and procedures related to fiscal policy. It thus represents a significant improvement over earlier proxies of fiscal, or budget, transparency.<sup>3</sup>

The paper proceeds as follows. The next section presents the theoretical argument, based on recent career-concerns models of electoral cycles, for how transparency matters for the incentives facing incumbent politicians contemplating an electoral cycle in fiscal balance, and relates it to other literature. Section three describes the data used in the empirical analysis. Section four presents the empirical specifications employed as well as the main results for electoral cycles and transparency. We also show that the magnitude of these electoral fiscal cycles depends in a theoretically meaningful way on the degree of polarization of the party system, with bigger cycles evident in more polarized systems. We also briefly consider the role of government debt as a determinant of fiscal balance and comment on the separate effects of

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<sup>3</sup> Alt and Lassen (2005) 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.

transparency and polarization on revenues and expenditures. Section five concludes with a discussion of implications for future theoretical and empirical work.

## **2. Fiscal transparency, imperfect information and electoral manipulation of fiscal policy**

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.<sup>4</sup> 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 (Lohmann, 1998; Persson and Tabellini, 2000, 2003b; Shi and Svensson, 2002a; Alt and Lassen, 2005; and Brender and Drazen, 2004).

In Persson and Tabellini, Shi and Svensson, and Alt and Lassen, incumbents produce public goods using a linear technology.<sup>5</sup> Provision of public goods is primarily financed by tax revenue, but it is also possible for incumbents to incur deficits, which are costly in terms of foregone future consumption, and thereby increase current public goods provision for a given tax revenue (and, hence, a given level of private consumption). Furthermore, public goods

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<sup>4</sup> See Besley and Prat (2002) for an analysis of media freedom and transparency in principal-agent models.

<sup>5</sup> The 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.

provision increases in incumbent ability, which, according to the career-concerns specification, is unknown by both voters and politicians at the time public sector decisions are taken.

Voters want politicians in office who, everything else equal, are more able in providing public goods. In the models of both Persson and Tabellini and Shi and Svensson, voters are concerned only with the competence of politicians, while Alt and Lassen generalize the model by explicitly considering a trade-off voters face between policy positions and competence. Even with this trade-off, Alt and Lassen find the basic insights of the other career-concerns models to be sustained.

Politicians seek re-election, either to enjoy office rents (Persson and Tabellini, Shi and Svensson) or to implement their preferred policy for a larger or smaller public sector (Alt and Lassen). Voters can observe neither incumbent ability nor 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.

We model transparency as the probability that voters observe the true level of deficits before the election. If they can indeed do this, they can in principle deduce incumbent ability from the public budget constraint. Hence, in this case, they can decide on re-election voting with full information. The greater is the level of transparency, the less incumbent politicians gain in terms of expected votes by electioneering and, since running deficits is costly, the smaller is the equilibrium level of deficits incurred by the incumbent.<sup>6</sup>

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<sup>6</sup> Besley (2003) 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 (2003).

However, the inclusion of partisan differences in the model opens up another channel of influence on electoral cycles. Assume two parties or blocs, one of which (“Left”) prefers a larger scale of public good production than the other.<sup>7</sup> Define political polarization simply as the absolute difference between platforms of the parties and, to focus on a pure polarization effect, as one that leaves the median voter’s assessment of party platforms unchanged. Then Alt and Lassen (2005) prove that for both parties equilibrium debt increases as the degree of political polarization increases. As polarization increases it becomes more desirable for each party to stay in office, as the utility loss from seeing the opponent in office increases. This leads to more debt accumulation by both parties.<sup>8</sup>

The recent empirical work on electoral cycles employs data from many countries, including both developed and developing countries, over as many as forty years. Both Persson and Tabellini and Shi and Svensson find evidence for electoral cycles in fiscal policy instruments, conditioning, respectively, on constitutional regime and degree of democracy. In their empirical work, Shi and Svensson proxy the public’s ability to monitor politicians by media access measured by radio ownership multiplied by freedom of broadcasting. Cycles are more prominent where incumbents are more able to hide fiscal policy from the public.

In contrast, Brender and Drazen (2004) argue that the apparent presence of electoral cycles stems from the inclusion of new democracies in the sample. New democracies are over-represented among countries with weak media, and indeed (in the case of Persson and Tabellini), presidential systems. In new democracies, Brender and Drazen argue, voters are not

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<sup>7</sup> This analysis assumes that governments have “left” and “right” centers of gravity. With two parties there is no ambiguity. With more, polarization reflects the distance to an average of possible successors. That is, the realized value of polarization to the incumbent, *ex post*, is the distance to the new government’s platform. However, *ex ante* the incumbent’s choice is based on an expectation of its successor’s platform. 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.

<sup>8</sup> This result was obtained by Alesina and Tabellini (1990), though with different parameter assumptions that restrict preferences.

yet accustomed to electioneering, but eventually, as voters gain more experience in the political process, the signal content of fiscal policy increases and the scope for manipulation of the public finances disappears.<sup>9</sup> At the same time, learning need not be confined to voters. As politicians acquire greater experience and knowledge of a democratic system and context, their ability to specialize in electioneering and the timing of manipulation could also increase.

Their empirical work finds electoral cycles in fiscal balance only in the sample of new democracies, suggesting that it is the age, rather than the degree, of democracy that matters. It remains unclear, however, what exactly happens in new democracies that make electoral manipulation of fiscal policy go away. The explanation offered by Brender and Drazen is that all actors of the political and electoral system gain experience in handling and extracting information from data. However, another explanation could be that it is not only a learning effect but also growing freedom of the press, as suggested by Shi and Svensson, which is important for reducing the scope for fiscal policy manipulation.<sup>10</sup> In either case, we assume that budget process transparency has similar effects, whether transparency fosters the 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.

### **3. 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

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<sup>9</sup> Nannestad, Paldam and Rosholm (2003) make the related argument that economic voting could develop over time for voters not accustomed to democracy. However, they find almost instantaneous adjustment when comparing native Israelis with immigrant Jews from former communist countries.

<sup>10</sup> Akhmedov and Zhuravskaya (2004) describe the Russian regions they study as “characterized by ... non-transparent government” (p. 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.

proposed. Greater transparency is one way to create what Powell and Whitten (1993) called “clarity of responsibility”. We assume transparency 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, and 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).

The best comprehensive definition we have found is given in Kopits and Craig (1998: 1), 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.”<sup>11</sup>

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 policymaking

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<sup>11</sup> For a more detailed discussion of aspects of transparent financial reporting, refer to the IMF web site on fiscal transparency (<http://www.imf.org/external/np/fad/trans/>).

process, and that do present consolidated information, are transparent” (Poterba and von Hagen 1999: 3-4).

Alt and Lassen (2005) construct a transparency index based on four distinct categories that all feature in the definitions and examples above: First, more transparent procedures should ease access and monitoring by processing more information, and, other things equal, do so in fewer documents or places. Second, there should be a commitment to non-arbitrary language such that words and classifications have clear, shared, unequivocal meanings. Third, the presence of more justification of decisions solidifies the basis for decision-making. Finally, the literature stresses the need for independent verification of the above.

The index comprises eleven items, ten of which are taken from a 1999 OECD questionnaire sent to all Budget Directors of OECD member countries (OECD 1999). Nine of these ten variables are a part of OECD’s *Best Practises for Budget Transparency* (OECD, 2001). To the ten were added a measure of whether financial statements are prepared using accrual accounting. The list of eleven items is in the Appendix. The eleven items are aggregated additively into a simple index. The index values range from a minimum of zero (Japan) to a maximum of eleven (New Zealand). 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.<sup>12</sup> The transparency variable has a mean of 4.2 with a standard deviation of 2.5.

To facilitate comparison with the literature referred to above, we use the dataset from Persson and Tabellini (2003a, b), to which we add variables as noted.<sup>13</sup> 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 three per cent 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.<sup>14</sup> 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.<sup>15</sup>

We start from a replication of the Persson-Tabellini specification. To this we add the stock of outstanding government debt, lagged one year. 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

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<sup>12</sup> Germany 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.

<sup>13</sup> We also obtained the data from Brender and Drazen (2004) and re-estimated all the main results with that dataset.

<sup>14</sup> The availability of only annual data makes the election variables unfortunately but necessarily crude.

<sup>15</sup> The countries include Australia, Austria, Belgium, Britain, Canada, Denmark, Finland, France, Germany, Iceland, Ireland, Italy, Japan, Netherlands, New Zealand, Norway, Sweden, Switzerland, and US.

cited therein). The rest of the specification allows pre- and post-election effects on public finance outcomes to differ between different constitutional regimes (majoritarian vs. proportional and presidential vs. 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.<sup>16</sup>

#### **4. Empirical specification and results**

We use the Alt-Lassen transparency index developed to investigate whether electoral cycles in debt differ between high and low transparency countries. Since our transparency index covers only the 1990's, we consider the period 1989-1998 for the group of advanced industrialized countries for which the transparency 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 in- or exclusion of particular items of the transparency index. In the other specification, we interact the election year indicator variables with the continuous transparency index.

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<sup>16</sup> GDP, 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 et al. (2004) report substituting GDP growth rates for the output gap without qualitatively altering the results.

Summary statistics are as follows. Table 1(a) presents the decomposition of the average surplus by transparency regime and election as opposed to non-election years. Consider that the average fiscal balance in all non-election years, unconditional on transparency, was a deficit of just under 2.9 percent of GDP. In election years, low transparency countries on average had a deficit of 3.3 percent of GDP. The simple decomposition in the Table shows two things: deficits were slightly larger in election years than non-election years, and high transparency countries have slightly higher deficits. However, we also see that while the deficits across election and non-election 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 non-election years.<sup>17</sup> It is also immediate from Table 1(b) that higher polarization is associated with higher average deficits in election years, but lower deficits in non-election years. To evaluate the size and significance of these differences in a multivariate setting, we turn to econometric analysis.

[Table 1 about here]

The basic specification allows the effects of lagged endogenous variables (persistence) and economic shocks (SHOCK) to differ between high and low transparency regimes. It captures differences in electoral cycles (as election year and post-election year effects) between countries in the two transparency regimes. Formally, to encompass these effects, we partition countries with variables  $H$  and  $L$  into two groups:  $H_i = 1$  for high transparency countries and zero otherwise and vice versa for  $L_i$ . Then we write the model as:

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<sup>17</sup> A 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 non-post-election years.

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

for  $N = 1, \dots, 19$  and  $T = 1989, \dots, 1998$ . The principal quantities of interest, the electoral cycle effects,<sup>18</sup> 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  $\mu_i$  and  $\lambda_t$  capture country and year fixed effects, respectively.<sup>19</sup> Below we employ the same sort of parameterization in the case of political polarization. In principle, the effects of the additional control variables could also differ by transparency 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 for transparency do not depend qualitatively on whether the electoral cycle is interacted with transparency in continuous or collapsed form. To show this, we specify a second case with the continuous transparency index. It is like the one above apart from the way the interaction of transparency and the election year variables is 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. 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 dynamic panel data model (Arellano-Bond GMM) using first-differences that Judson and Owen

<sup>18</sup> We 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.

<sup>19</sup> Because 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.

(1999), based on Monte Carlo studies, recommend for macroeconomic models with small time samples. However, Judson and Owen acknowledge that the GMM model can also be biased when the number of time periods is small.<sup>20</sup> We estimate the robust GMM model provided 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 five percent 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 five percent 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 re-election. 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 employ an alternative definition of electoral year than simply the calendar year. If the election took place within the first quarter of a given year, we define the election year to be the calendar year before the election actually took place, and, thus, the post-election year equals the actual calendar election year: this is *adjusted election year*. For example, suppose the election took place in February 1995. In one set of our specifications (and all of Persson and Tabellini

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<sup>20</sup> 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.

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”.<sup>21</sup>

Therefore, we present the results of eight specifications that vary the transparency 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, and replicate all results with the Brender-Drazen data.<sup>22</sup> 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.

#### *4.1 Transparency and political budget cycles*

Both Persson and Tabellini and Brender and Drazen find that restricting the sample to industrialized countries suggests the absence of an electoral cycle in fiscal balance. Indeed, if we do not condition the election year effects on transparency regime (see Appendix Table 1, column 1), we find that the pre-election variable has coefficient -0.20 and standard error 0.25, and 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

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<sup>21</sup> This is still clumsy in comparison with using finer-grained data (see Akhmedov and Zhuravskaya 2004 on this point) but the finer data is not available for all variables.

<sup>22</sup> One issue that we do not address is the endogeneity of election dates. Most of the countries in our sample have endogenous election dates, but our sample is neither broad enough nor long enough to evaluate the potential endogeneity of election dates and its effects on the estimates. Brender and Drazen (2004) and Shi and Svensson (2002a,b) 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.

of results from the most basic specification above, allowing electoral effects to differ between transparency regimes when we use the binary transparency indicator.

[Table 2 about here]

All regressions in Table 2 included the full set controls. (Full results for the first column appear in Appendix Table 1, column 2.) Among what isn't shown, lagged debt is always positive, so the existence of a larger stock of debt, other things equal, predicts deficit reduction.<sup>23</sup> 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, while we consistently identify an electoral cycle in the low transparency countries. Conditional on low transparency, the electoral indicators have the correct sign (negative and positive for election year and post-election year dummies, respectively). The election-year effects are consistently significant, though the post-election effects are significant only when we omit observations with large output shocks. 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 effectively averages the different effects, as illustrated in Figure 1. Once the transparency of the budgetary process is taken into account, we find

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<sup>23</sup> Like 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.

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.

[Figure 1 about here]

Columns 3 and 4 of Table 2 report results from a similar pair of regressions, but where we have adjusted dates for elections occurring in the first quarter of a year. In terms of statistical significance, the results are identical to those 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 per cent larger when the election year is adjusted to the previous calendar year when 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 indeed engage in electoral manipulation already in the year preceding the election, and also that the adjusted coding is more appropriate.

What happens across the electoral cycle? Looking back at Figure 1, the magnitude of the estimated cycles for the two groups of countries 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 in table 2 is, for any context, 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 per cent of GDP). However, the cycle estimated above contains *two* average annual changes, so the estimated *annual* equivalent of the low-transparency cycle in the first column of Table 2 is 0.54 per cent of GDP. Across the Table, these estimates range from .54 to .69 or between 45 and 57 per cent of the average absolute change in the fiscal balance (1.21). These are substantively (as well as statistically, as we shall see) significant effects.

While the individual indicator variables are not always individually significant, we generally identify a statistically significant cycle when we take into account the covariance between the parameter estimates for the election year and post-election year dummies. We carry out the tests by simulating the “predicted” cycle, by taking 10,000 draws from a bivariate normal distribution with means  $(a, b)$  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. This takes account of the covariance, which in this case is generally non-negligible. We construct the cycle estimate as above, by taking  $-1 * (\textit{election year estimate}) + (\textit{election year estimate})$ . The average value of this quantity over the 10,000 draws is shown in the Tables as *cycle*, with the standard error observed in the simulation. The cycle is indeed usually statistically significant, and is well more than twice its standard error, four times in the case with the biggest shocks deleted and an adjusted election-year.<sup>24</sup> Note also that because *cycle* is a linear combination of two regression coefficients, the statistical significance of *cycle* is a substitute for the usual F-test on the joint significance of two coefficients.

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<sup>24</sup> Fixed 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.

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 measures, the electoral year indicators and the estimated cycles are insignificant (Appendix Table 1). 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.

[Table 3 about here]

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.<sup>25</sup> The panel on the right, reflecting smaller standard errors 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 percent 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.

[Figure 2 about here]

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<sup>25</sup> The 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 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  $var(totalcycle) = var(cycled) + TRANS^2 * var(cyclec) + 2 * cov(cycled, cyclec) * TRANS$ . See Brambor, Clark and Golder (2004) and Franzese and Kam (2005) for more on how to model and interpret interactive effects.

In results not reported, we used the public finance data collected by Brender and Drazen (2004) rather than the Persson-Tabellini data. The main difference seems to be that Brender and Drazen have collected more observations for some, but not all, countries. For example, for the binary transparency case, the estimated pre-election effect is larger and more significant, while 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 ( $n = 164$ ).<sup>26</sup> Results with their data are generally comparable to, though slightly weaker than, those reported in Tables 2 and 3. The main reason for the differences, we believe, is attributable to the fact that most of the additional data has been collected for countries with a relatively high degree of transparency.

We do not here address the issue of reverse causation, or endogeneity of transparency, that transparency is just a matter of politicians having nothing to hide. However, Alt and Lassen (2005) include an instrumental variables estimate of their cross-section results for transparency and debt, which suggests that their main results go through. We will address the incentive of politicians to create transparent institutions in a separate paper (Alt, Lassen, and Rose 2005) Furthermore, our results are robust to the exclusion of particular countries.<sup>27</sup>

Finally, lagged debt, included in all regressions reported above, increases surpluses significantly. Most important, whether lagged debt is included or not does not alter the effects of transparency and political polarization 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.<sup>28</sup>

There may be a connection to Maastricht here, since our data is for the 1990s and includes a

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<sup>26</sup> The same is basically the case for 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.

<sup>27</sup> Brender and Drazen (2004) find some results of Persson and Tabellini as well as Shi and Svensson to depend on the inclusion of Sweden. Our results are robust to (that is, do not reveal) this.

<sup>28</sup> It is possible that the output gap is itself endogenous with respect to deficits. Then its estimated zero effect could be biased.

period where 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. Further, 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.<sup>29</sup>

#### *4.2 Political polarization*

An unexplored implication of the Alt-Lassen model is that countries in which the party system is more polarized should have larger electoral cycles in fiscal balance, other things equal. In their model, a party seeing its opponent implement the opponent's favored policy experiences a welfare loss. 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 re-elected. 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.

We use data on political polarization to investigate this. One measure of party system polarization comes from an expert survey, presented in Laver and Hunt (1992). Country specialists were asked to assign scores on a 20-point scale representing the parties' priorities between raising taxes to increase public services and cutting public services to cut taxes.<sup>30</sup> Our variable is the standard deviation of party-by-party mean raw scores on this rating for each

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<sup>29</sup> Countries participating in Maastricht did pursue more active stabilization policy in the period under consideration.

<sup>30</sup> Party-by-party raw scores appear in Laver and Hunt (1992, Table 3 for each country, pp. 136-312). The number of respondents is in Table A1, p. 123. The party scale is in Table A2, p. 124.

country.<sup>31</sup> The index ranges from 2.2 (New Zealand) to 6.6 (US), with a mean of 4.7 and a standard deviation of 1.1. As in the case of transparency, we employ both a dichotomous measure for the sake of simplicity<sup>32</sup> and the continuous measure based directly on the data. We note that there is no statistical association at all between polarization and transparency.

As in the case of transparency, we begin by looking at a simple decomposition of the average surplus by polarization regime and election vs. non-election year. Table 1(b), above, 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 measure, with the specifications similar to those of tables 2 and 3.<sup>33</sup> All four columns provide strong evidence for the hypothesis 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

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<sup>31</sup> 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. In case the standard deviation is unduly affected by the positions assigned to extreme (small) parties, we repeat the analysis with an alternative measure that avoids the problem.

<sup>32</sup> The dichotomous measure, which might also reduce measurement error from a judgment survey, yields two equal-sized groups of countries. Higher polarization countries include Britain, Denmark, France, Iceland, Ireland, Italy, Japan, Netherlands, and US. Data is unavailable for Switzerland. Note 14 lists all countries.

<sup>33</sup> See Appendix Table 1. In the case of dichotomous measures, we cannot include interaction terms for election dates, both fiscal transparency and political polarization, and all controls at the same time.

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.

[Table 4 about here]

Does the polarization effect hold up independent of transparency? Table 5 presents results for the continuous polarization measure and the continuous transparency measure. In this case, unlike Table 4, if we do not control for transparency but include the continuous polarization measure on its own (results not shown), the results are correctly signed but nowhere near significant. However, once the continuous measure of transparency is included in the regression, 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 various alternative samples and definitions in the Table (and see Appendix Table 1 for an example).

[Table 5 about here]

Figure 3 presents an illustration of the results. Briefly, 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. 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 do remain significant – the 95 per cent confidence interval just touches zero --and in both cases confirm the hypotheses suggested by the theory. In results not shown, we find broadly similar, and often stronger (but sometimes weaker) results employing the Brender and Drazen data set.

[Figure 3 about here]

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 re-elected). 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.

Nevertheless, we find that using Golder's measure of polarization yields many qualitatively similar results that are 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 control for both polarization 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.

#### *4.3 Transparency, polarization, revenues and expenditures*

We also investigated, by repeating the entire analysis with different dependent variables, whether the electoral policy cycles identified above are driven by manipulating expenditures or revenues. The short answer is only a few cases do we observe significant effects of fiscal transparency and political polarization on electoral cycles in revenues and expenditures separately.<sup>34</sup> Results for the case of (dichotomous) transparency suggest that revenues decrease in election years in both high and low transparency countries, but that subsequent post-election adjustment in form of increasing revenues takes place only in high transparency countries.<sup>35</sup> This resonates with the finding in Alt and Lassen (2005) that low transparency countries accumulate higher long run debt. We find no effects of transparency on expenditure levels. For the case of (dichotomous) polarization, high polarization countries have higher expenditures in election years, and lower expenditures in post-election years, while the opposite is the (weakly) case for low polarization countries. In terms of revenues, both high and low polarization countries appear to decrease revenues in election years and readjust accordingly.

Given that we estimate clearer cycles in fiscal balance, how should we interpret the non-finding for the separate components? Do governments engineer the electoral cycle by adjusting revenues and expenditures only marginally, such that only the combined effort, the

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<sup>34</sup> Rose (2004) and Streb et al. (2005) find electoral cycles in expenditures; Akhmedov and Zhuravskaya (2004) find shifts in revenues and in the composition of expenditures.

<sup>35</sup> Looking at revenues and expenditures separately (results not reported) reveals that the dampening effect of the debt level on deficits is through increasing tax revenues: lagged debt increases revenues substantially and significantly, while the effect on expenditures, though negative, is small and insignificant.

fiscal balance, shows significant distortions? Possibly voters observe the changes in revenues and expenditures more readily than they observe fiscal balance; the latter imposes the added difficulty of combining information about revenues and expenditures. If transparency increases information flow relatively more for fiscal balance than for its components, this could create the situation that cyclical effects are absent where transparency is higher because they would become too detectable in those contexts. This is a subject to which we will return in future research.

## **5. Discussion and Concluding Remarks**

In a relevant, hardly light-hearted, effort to award a cycling prize between the current incumbent and an important figure from the past, Nixon, Rogoff (2004) 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.

The main result of this paper is that one does not have to look to weak and new democracies to find evidence of electoral cycles, as suggested in a growing body of recent research. Our key argument is that the scope for electoral cycles in fiscal balance depends on the degree of fiscal policy transparency, and we show using a recent transparency index measure on panel data for nineteen advanced industrialized OECD economies over the 1990's 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 most cases reveals itself only after controlling for the level of transparency. 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.

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 Torres 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 US 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 re-election. We plan to examine this, too, in future research.

And, as a lighter, final thought, going back to Rogoff's conjectures about the current American election, consider this. On the Persson-Tabellini analysis, the US, as a presidential system, should feature large electoral cycles in deficits. This amounts to the prediction that, other things equal, the deficit will be significantly reduced in 2005 whether Bush is re-elected or not. On our analysis, the US, as a relatively transparent system, has small electorally-induced cycles. That amounts to the prediction that, other things equal, the US deficit will *not* be significantly reduced in 2005. We'll see what happens.

## Appendix

The transparency index contains:

- *More information, other things equal, in fewer documents*
  - Whether non-financial performance data is routinely included in the budget documentation presented to the legislature (yes = transparent)
  - Whether special reports on the fiscal outlook are released prior to an election (yes = transparent)
  - Whether the government regularly produces a report on the long term (10-40 years) outlook for public finances as a whole (yes = transparent)
  - Whether the government is required to report contingent liabilities on a regular basis (yes = transparent)
  - Whether the government generally presents more than one supplementary budget to the legislature in each fiscal year (no = transparent)
- *Independent verification*
  - Whether the in-year financial reports are audited (yes = transparent)
  - Whether the economic assumptions used in the budget are subject to independent review (yes = transparent)
- *Non-arbitrary language*
  - Whether the government uses accrual accounting in its financial statements (yes = transparent)
- *More justification*
  - Whether there is a legal requirement that the budget documentation contain a projection of expenditure beyond the next fiscal year (yes = transparent)
  - 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)
  - Whether the budget discusses the impact that variations in the key economic assumptions would have on the budget outturn (yes = transparent)

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**Table 1(a): Surplus in percent of GDP, by transparency regime and election year**

	Election year	Non-Election 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	Non-Election 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)

**Table 2. Electoral Cycles in Fiscal Balance: Binary Transparency Measure**

	Surplus/GDP	Surplus/GDP	Surplus/GDP	Surplus/GDP
Pre-election year,	-0.56*	-0.64***	-0.76**	-0.85***
Low transparency	(0.30)	(0.24)	(0.30)	(0.25)
Post-election year,	0.52	0.67*	0.41	0.53**
Low transparency	(0.38)	(0.37)	(0.29)	(0.26)
Two-year Cycle,	1.07**	1.34***	1.17***	1.37***
Low transparency	(0.47)	(0.40)	(0.42)	(0.35)
Pre-election year,	0.02	0.10	0.01	0.26
High transparency	(0.38)	(0.36)	(0.32)	(0.34)
Post-election year,	-0.08	-0.27	-0.16	0.01
High transparency	(0.26)	(0.31)	(0.32)	(0.31)
Two-year Cycle,	-0.11	-0.37	-0.17	-0.26
High transparency	(0.34)	(0.38)	(0.30)	(0.56)
Sample	Full	ygap <5	Full	ygap <5
Election year	Calendar	Calendar	Adjusted	Adjusted
N	139	135	139	135
Serial correlation	0.74	0.79	0.87	0.85
(p-value)	(0.43)	(0.43)	(0.38)	(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.

**Table 3. Electoral Cycles in Fiscal Balance: Continuous Transparency Measure**

	Surplus/GDP	Surplus/GDP	Surplus/GDP	Surplus/GDP
Pre-election year	-1.12 (0.75)	-1.22 (0.67)	-1.37** (0.69)	-1.39** (0.61)
Post-election year	0.53 (0.52)	0.43 (0.55)	0.32 (0.40)	0.49 (0.39)
Pre-election year <i>times</i> transparency	0.21 (0.17)	0.24 (0.15)	0.26* (0.16)	0.30** (0.14)
Post-election year <i>times</i> transparency	-0.08 (0.09)	-0.08 (0.09)	-0.05 (0.07)	-0.06 (0.07)
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.

*Table 4. Electoral cycles in Fiscal Balance: Binary Political Polarization Measure*

	Surplus/GDP	Surplus/GDP	Surplus/GDP	Surplus/GDP
Pre-election year, High polarization	-0.68** (0.23)	-0.65** (0.26)	-0.86*** (0.24)	-0.80*** (0.25)
Post-election year, High polarization	0.43 (0.30)	0.57* (0.34)	0.27 (0.27)	0.35 (0.29)
Cycle, High polarization	1.10*** (0.42)	1.22** (0.48)	1.13*** (0.39)	1.15*** (0.45)
Pre-election year, Low polarization	0.08 (0.45)	0.08 (0.44)	0.06 (0.38)	0.29 (0.38)
Post-election year, Low polarization	-0.23 (0.25)	-0.51 (0.31)	-0.34 (0.29)	-0.13 (0.23)
Cycle, Low polarization	0.30 (0.38)	0.59 (0.38)	0.39 (0.31)	0.42 (0.32)
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

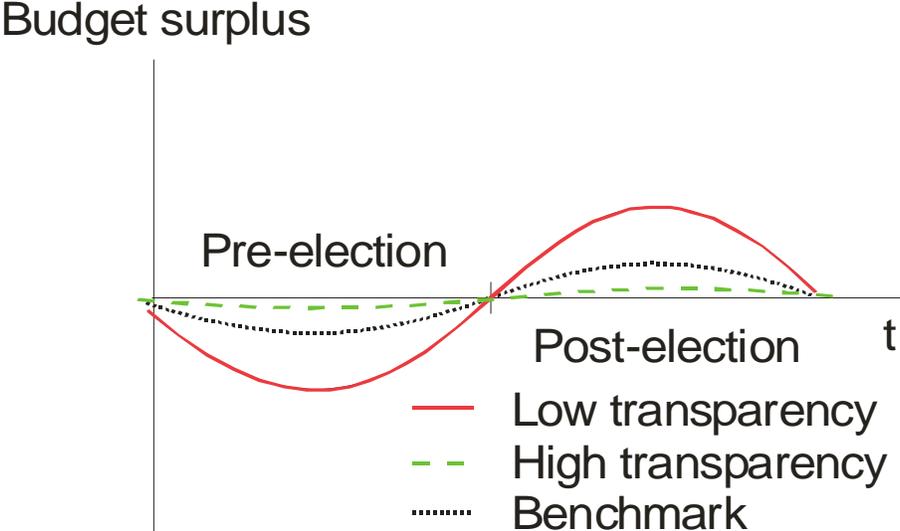
Estimated using xtabond in Stata 8.0. Robust standard errors in parentheses. \*\*\*, \*\*, \* denote significance at 99 %, 95 % and 90 % levels, respectively. Controls included but not shown as described in main text.

Table 5. Electoral Cycles in Fiscal Balance: Continuous Transparency and Polarization Measures

	Surplus/GDP	Surplus/GDP	Surplus/GDP	Surplus/GDP
Pre-election year	0.41 (1.01)	0.25 (0.92)	0.33 (1.01)	0.80 (0.83)
Post-election year	0.04 (1.30)	-0.42 (1.42)	-0.15 (1.18)	0.63 (1.04)
Pre-election year <i>times</i> transparency	0.28* (0.16)	0.31** (0.15)	0.34** (0.13)	0.37*** (0.11)
Post-election year <i>times</i> transparency	-0.00 (0.13)	-0.00 (0.13)	0.02 (0.11)	-0.01 (0.10)
Pre-election year <i>times</i> polarization	-0.38* (0.22)	-0.38* (0.21)	-0.44** (0.20)	-0.54*** (0.16)
Post-election year <i>times</i> polarization	0.04 (0.21)	0.12 (0.23)	0.03 (0.19)	-0.07 (0.18)
Sample	Full	ygap <5	Full	ygap <5
Election year	Calendar	Calendar	Adjusted	Adjusted
N	131	127	131	127
Serial correlation (p-value)	0.85 (0.39)	0.78 (0.43)	0.8 (0.42)	0.79 (0.43)
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.

Figure 1:



**Figure 2**

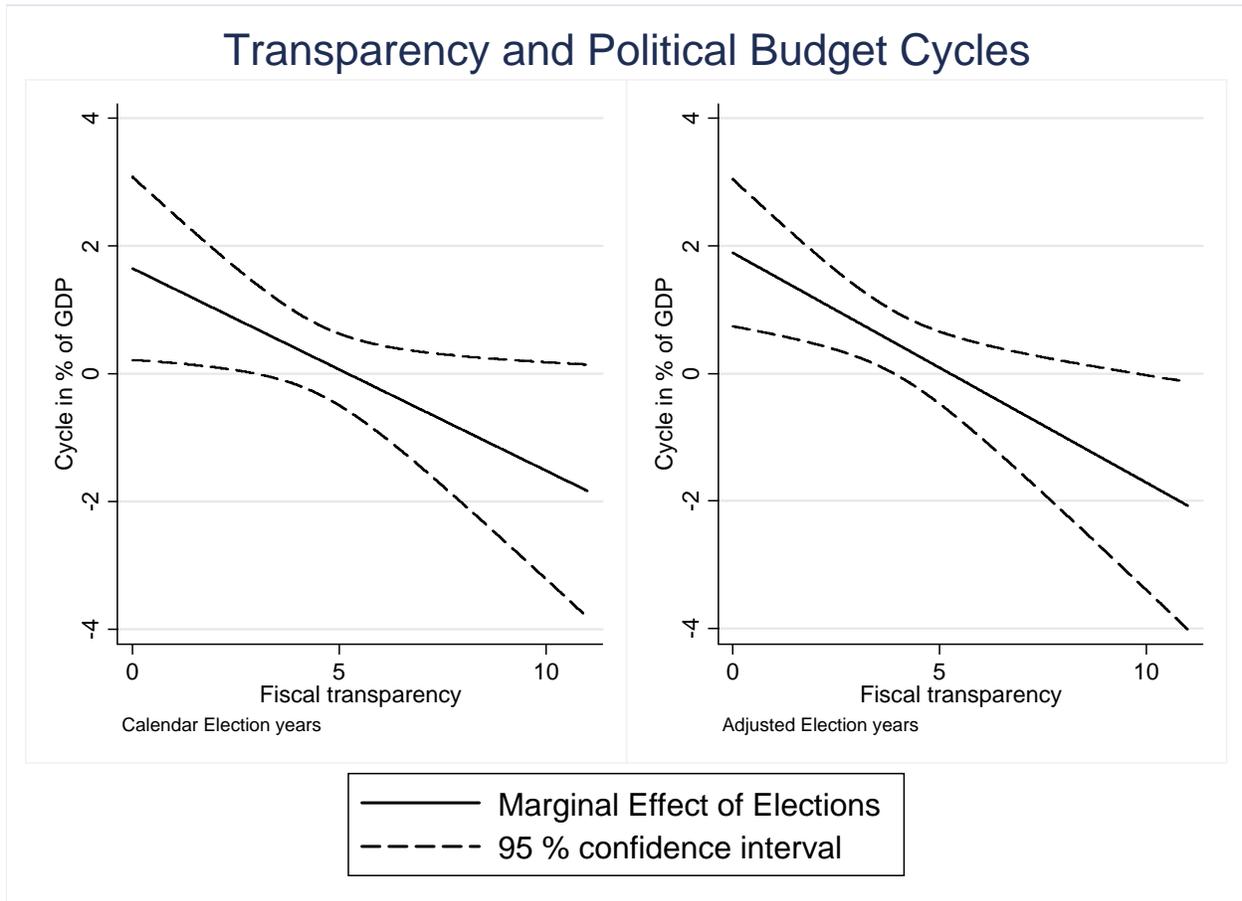
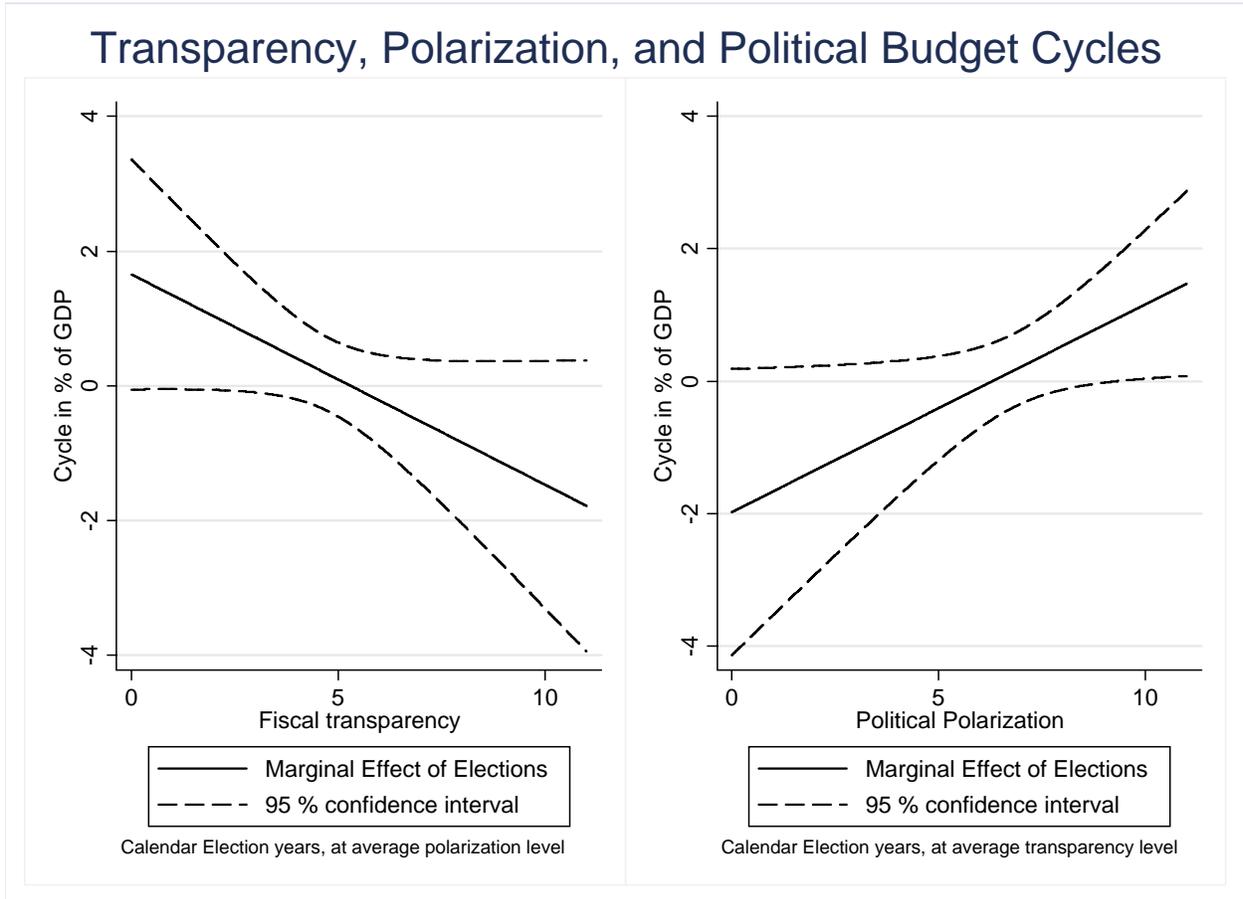


Figure 3



**Appendix Table 1, Selection of full regression results**

	(1)	(2)	(3)	(4)	(5)	(6)
	PT replication	Unconditional (our sample)	Binary transparency	Continuous transparency	Binary polarization	Continuous transp and polar
	Surplus/GDP	Surplus/GDP	Surplus/GDP	Surplus/GDP	Surplus/GDP	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)	21.12 (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	1063.00	139.00	139.00	139.00	127.00	127.00
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