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Do electoral cycles differ across political systems?*

Torsten Persson[†] and Guido Tabellini[‡]

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Abstract

Do fiscal policy variables – overall spending, revenue, deficits and welfare-state spending – display systematic patterns in the vicinity of elections? And do such electoral cycles differ among political systems? We investigate these questions in a data set encompassing sixty democracies from 1960-98. Without conditioning on the political system, we find that taxes are cut before elections, painful fiscal adjustments are postponed until after the elections, while welfare-state spending displays no electoral cycle. Our subsequent results show that the pre-election tax cuts is a universal phenomenon. The post-election fiscal adjustments (spending cuts, tax hikes and rises in surplus) are, however, only present in presidential democracies. Moreover, majoritarian electoral rules alone are associated with pre-electoral spending cuts, while proportional electoral rules are associated with expansions of welfare spending both before and after elections.

1 Introduction

Do fiscal policy variables – such as overall spending, revenue, deficits and welfare-state spending – display systematic patterns before and after elections? And do these patterns differ depending on the electoral rules or the form of government? Existing empirical research has addressed the first question, but almost nothing is known about how electoral cycles vary across political systems. The goal of this paper is to fill this gap in our empirical knowledge about electoral policy cycles.

Most of the modern empirical work on electoral fiscal policy cycles can be motivated by theories originally formulated in the late 1980s: the model of opportunistic electoral cycles in Rogoff and Sibert (1988), or the model of electoral accountability in Ferejohn (1986). Rational but uninformed voters reward good

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performance in office with their vote, because they attribute good performance either to lasting skills of the incumbent government (adverse selection models) or to restraint in the use of political power (moral hazard models). Such voting behavior creates incentives for incumbent politicians to appear to be performing well before elections. The precise predictions depend on the assumptions about the policy process, the motivation of incumbents, and the information set of voters, but the general predictions are as follows. Both types of models predict that taxes are cut before elections. But their predictions for spending differ. Models emphasizing reputation building by office-seeking politicians (such as Rogoff, 1990) predict – like traditional Nordhaus-Lindbeck models – that spending is raised, whereas models emphasizing electoral accountability of rent-seeking politicians (such as Besley and Case, 1995) instead predict that wasteful (in the eyes of the voters) spending is cut.

Are these ideas consistent with the data? A sizeable empirical literature has addressed this question. Much of it has focused on *monetary* policy in OECD countries, however, with somewhat inconclusive results (see Drazen, 2000b for a critical review). Empirical work on *fiscal* policy is more recent and less systematic, and many studies are plagued by data sets from a small number of political jurisdictions. But recent research suggests that fiscal policy indeed tends to be systematically manipulated *before* elections. Moreover, some studies find these electoral cycles to be more pronounced in developing countries ruled by worse democratic institutions, or affected by other constitutional provisions. Little is known about the systematic pattern of fiscal policy *after* elections, as existing research on post-election cycles has focused almost exclusively on “partisan” (i.e., left or right) cycles.¹

Why do we pose the second question, i.e., why should we expect electoral cycles in fiscal policy to vary systematically with electoral rules and forms of government? A strong motivation can be found in a recent wave of theoretical research comparing fiscal policy outcomes under different versions of these constitutional arrangements (e.g., Austen-Smith, 2000, Persson, Roland and Tabellini, 2000, Lizzeri and Persico, 2001, Milesi-Ferretti, Perotti and Rostagno, 2002).

This recent line of theoretical research strongly suggests that *electoral rules* shape fiscal-policy incentives of politicians. Some of the work argues that electoral accountability, and hence corruption, wasteful spending and taxes, differ

¹Among the more recent studies on international data, Shi and Svensson (2001) analyze a large panel of developed and developing countries, focusing on how electoral cycles interact with voters’ access to information and incumbents’ access to rents. Schucknecht (1996) and Block (2000) study different samples of developing countries, so does Gonzalez (1999) who also focuses on the interaction with the quality of democratic institutions. Among the papers that use regional data, Besley and Case (1995) and Lowry, Alt and Ferree (1998) focus on the US states, the former asking whether cycles are stronger when governors are not up against a term limit and the latter conditioning on the form of election and the party in power. Petterson-Lidbom (2002) studies a panel of almost 300 Swedish municipalities. All these papers find evidence of pre-election cycles in fiscal policy. Alesina, Roubini and Cohen (1997), Drazen (2000a), (2000b) and Persson and Tabellini (2000) review the theoretical and empirical literature.

across “majoritarian” and “proportional” electoral rules – much in line with the common idea in political science that majoritarian elections are associated with stronger accountability than proportional elections. Persson and Tabellini (1999) show theoretically that accountability is stronger in majoritarian systems with small districts and plurality rule, as the electoral outcome becomes more sensitive to marginal changes in votes. Persson and Tabellini (2000) consider a career-concern model of policy making, where wasteful spending is smaller in plurality-rule elections, where politicians are individually accountable to the voters, than in PR elections, where they are collectively accountable via party lists. Persson, Tabellini and Trebbi (2002) show empirically that perceptions of corruption in cross-country and panel data are indeed systematically associated with electoral rules, much in the way theory suggests. But electoral accountability should be at its strongest at election time. Thus, if majoritarian elections induce stronger accountability, they should also display larger variations in spending and taxes around elections, as Persson and Tabellini (2000, Ch. 9) informally argue .

Another theme in the recent literature concerns electoral rules and the composition of public spending into broad programs (social transfers, national public goods) vs. geographically targeted programs (targeted transfers, local public goods). Several recent studies (Persson and Tabellini, 1999, Lizzeri and Persico, 2001, Milesi-Ferretti, Perotti and Rostagno, 2002), derive the result that proportional elections induce politicians to seek support from larger groups in the electorate via broad spending programs – much in line with the common idea in political science that proportional elections are associated with wider political representation than majoritarian elections. Persson and Tabellini (2001) and Milesi-Ferretti, Perotti and Rostagno (2002) find empirical support for this prediction, showing that social transfers are higher in countries with proportional electoral rules. A formal model of electoral cycles in the composition of government spending remains to be written down. But – given the above results – it is plausible to expect a stronger expansion of broad programs around elections under proportional electoral rules than under majoritarian electoral rules.

Theoretical research on how the *form of government* shapes fiscal policy outcome is less widespread, despite a large literature where political scientists compare presidential and parliamentary regimes. In Persson, Roland and Tabellini (1997, 2000), the distinction between these forms of government centers on the rules for legislative bargaining. In parliamentary democracies, bargaining between different legislative coalitions is disciplined by the threat of a government crisis. As such a crisis would result in the loss of valuable agenda-setting powers for the government coalition, party discipline and stable legislative coalitions are promoted. In a presidential system, instead, the executive cannot be brought down by the legislature, but it is directly accountable to the voters. Thus, legislators have weaker incentives to stick together and to vote according to party or coalition lines. Moreover, agenda-setting power is generally more dispersed among different committees, and checks and balances between the executive and the legislature give proposal and veto rights to several players. These differences create larger overall spending, larger broad programs (at the expense of targeted

programs) and more wasteful spending in parliamentary regimes, compared to presidential regimes. Persson and Tabellini (2001) find strong empirical support for the prediction that parliamentary regimes have larger governments.

In the absence of a specific model, we do not have precise predictions regarding electoral cycles, but it is plausible that some of the predicted policy differences across regimes should show up particularly strongly around election time. Another difference between presidential and parliamentary forms of government is the individual vs. collective nature of the executive. By analogy with the above career-concern argument that individual political accountability gives stronger incentives than collective accountability, we might expect stronger electoral cycles under presidential regimes. (Lowry, Alt and Ferree, 1998, make a similar point when they argue – and show empirically – that voters respond more vigorously to policy in gubernatorial elections than in legislative elections in the US states.) But all in all, we have weaker priors when it comes to how electoral cycles might differ between presidential and parliamentary democracies.

Based on the above motivation, we take a first look at the evidence of electoral cycles in fiscal variables under different electoral rules and forms of government. We use a data set encompassing sixty democracies over about forty years (1960-98). General fiscal policy is measured by government spending, revenues and the budget surplus. We also consider welfare-state spending, as a type of expenditure that benefits broad groups in the population and is difficult to target towards narrow geographic constituencies. Section 2 describes these policy measures, a number of control variables, as well as our measurement of constitutional rules and election dates.

As the data come in panel form, we always use fixed country and year effects. We also allow for economic shocks and for different policy dynamics in different constitutional groups, so as not to falsely attribute such differences to different electoral cycles. Section 3 discusses the econometric specification.

Section 4 reports on what we find. When we do not condition at all on the political system, our estimates identify both a pre-election and a post-election cycle in fiscal policy: taxes are cut before elections, while painful fiscal adjustments (mainly taking the form of cuts in spending and deficits) are postponed until after the elections. There is no electoral cycle in welfare-state spending. We then condition on electoral rules. In line with our priors, pre-election fiscal policy cycles are more pronounced in majoritarian countries, while welfare-state spending rises before and after elections only in proportional countries. When we instead condition on the form of government, we discover an intriguing difference between presidential and parliamentary countries: while pre-election tax cuts mainly take in place in parliamentary democracies, the post-election fiscal contractions take place only in presidential democracies. A final subsection digs deeper for the root of the results, by disaggregating our electoral data into the full four-way classification of constitutional groups.

Section 5 offers a short discussion of the results and where research should go next.

2 Data

The data used in this paper have been collected as part of a larger research program on economic policy in different political systems. It comprises annual data for 60 countries over almost 40 years (1960-98). The resulting panel includes a number of economic, social and political variables. Because of missing data and our rules for sampling (described next), however, it is an unbalanced panel. This section briefly describes our data, whereas the sources are listed in the Data Appendix. A more comprehensive discussion is found in Persson and Tabellini (2003).

We confine the study to countries with democratic political institutions. To select the sample, we mainly rely on the Polity IV data set covering independent nations with a population exceeding half a million people (both criteria refer to 1998). Specifically, we use the encompassing *POLITY* index, which assigns to each country and year an integer score ranging from -10 to +10, with higher values associated with better democracies. The index is based on the competitiveness and openness in selecting the executive, political participation, and constraints on the chief executive. For a few (small) countries where the *POLITY* index is missing, we use so-called Gastil scores from Freedom House to amend the series. (Specifically, we regress the two scores on each other and use predicted values from this regression to replace missing observations.) We then restrict our panel to those countries and years with positive values of *POLITY*. This permits a total of 60 countries in the panel, but some of them enter only in some years. For example, the rule temporarily excludes countries like Turkey (intermittently in the 70s and 80s), Argentina (until 1972 and between 1976 and 1982) and Chile (between 1974 and 1988). In the paper, we also report results based on stricter criteria of good democracy to define years and countries with democratic governance. We treat censored observations as randomly missing and do not attempt to model this aspect of sample selection.

As we study national elections, we focus on prospective cycles in four fiscal policy instruments under control of the central government. All four measures are scaled to GDP and expressed as percentages. Thus, we measure the central government's overall expenditures (inclusive of social security) and revenues (*CGEXP*, *CGREV*), budget surplus (*SPL*), and total outlays on broad welfare-state transfer programs, like pensions and unemployment insurance (*SSW*). Overall spending, revenue and deficits are available for most OECD and Latin American countries for the entire period 1960-98. For many other developing countries availability is limited to the period from the 1970s and onward. Similarly, the measure of welfare-state spending does not become available until the early 1970s. The source for all these variables is the IMF statistics (the IFS and GFS data sets).

When explaining these policy variables by panel estimation, we always include fixed country and year effects. In addition, we also hold constant some socio-economic variables likely to shape government outlays and revenues. Specifically, we control for the level of development, measured by the log of real per capita income (*LYP*), openness, measured by the trade share (*TRADE*), and

two demographic variables measuring the percentages of the population between 15 and 64 years of age (*PROP1564*), and above 65 years of age (*PROP65*), respectively. These variables have been shown to correlate with measures of fiscal policy in previous studies, such as Cameron (1978), Rodrik (1998), and Persson and Tabellini (1999). To control for fluctuations in fiscal policy induced by the business cycle, we rely on a measure of the output gap: the log difference between real GDP and its (country-specific) trend computed by help of the Hodrick-Prescott filter (*SHOCK*).

Electoral rules and forms of government are classified by means of two indicator (dummy) variables: *MAJ* and *PRES*. Majoritarian countries ($MAJ = 1$) are those that relied exclusively on plurality (or majority) rule in its most recent election to the legislature (lower house). Mixed and proportional electoral systems are lumped together and classified with $MAJ = 0$ (only a few countries have mixed electoral systems, hence it is difficult to tell them apart from either strictly majoritarian or strictly proportional countries). With regard to the form of government, presidential ($PRES = 1$) countries are those where the executive is not accountable to the legislature, and parliamentary countries ($PRES = 0$) are those where it is, irrespective of whether or not there is a directly elected president. As emphasized by Persson and Tabellini (2003), this indicator captures an institution producing stable legislative majorities. Specifically, we ask whether the president has legislative powers in the realm of fiscal policy. If not, and the government is accountable to Parliament through a confidence requirement, the country is classified as a parliamentary regime. Thus, France and Finland are classified as parliamentary countries despite having a directly elected president, since economic policy is controlled by a government that can be brought down by a legislative vote of no confidence. In the years covered by our sample, these constitutional classifications change very seldom: *PRES* does not vary at all, whereas *MAJ* changes only a few times (in France, Cyprus, New Zealand, Japan, Fiji and the Philippines). This stability reflects an inertia of political institutions well known to political scientists.

Finally, we need information on election dates. In parliamentary democracies, elections of the legislature and the executive coincide. In presidential democracies, the executive is separately elected, but almost always the legislature is also elected in that same year (in our sample there are only about ten presidential elections that do not coincide with elections of the legislature). Nevertheless, in presidential regimes there are also many “mid-term” legislative elections that take place in between years of simultaneous presidential and legislative elections. Our prior is that the incentives created by these mid-term elections are weaker relative to the election years in which both the president and the legislature are elected. Indeed, this is what the data suggest: when estimating electoral-cycle models for our different policy instruments, we have never found these mid-term elections to be significant determinants of policy. In the following, we therefore limit attention to the years of presidential elections.²

²Another reason for leaving out the mid-term elections is more pragmatic, namely we want to study both pre-election and post-election years. In some countries this poses problems with

That is, in both parliamentary and presidential regimes we code the year in which the executive is elected. The resulting variable (labeled *ELEX*) is equal to 1 in the years of presidential elections in presidential countries and in the years of legislative elections (for the lower house) in parliamentary countries; it is equal to zero in all other years. As explained in the introduction, we study fiscal policy behavior both before and after elections so we also use the one-year lags of the executive election dates (labeled *LELEX*).

3 Econometric specification

Below, we report parameter estimates based on different versions of the following regression equation:

$$\begin{aligned}
Y_{it} = & \alpha^0 Y_{it-1} + \beta \mathbf{X}_{it} + \gamma^0 SHOCK_{it} + \\
& \sum_{\substack{i \in MAJ=1, \\ PRES=1}} \{S_i[(\alpha^1 - \alpha^0)Y_{it-1} + (\gamma^1 - \gamma^0)SHOCK_{it}] + \\
& [(1 - S_{it})\delta^0 + S_{it}\delta^1]EL_{it} + [(1 - S_{it})\eta^0 + S_{it}\eta^1]EL_{it-1}\} + \\
& \mu_i + \lambda_t + u_{it} .
\end{aligned} \tag{1}$$

In this expression, Y_{it} denotes one of our four policy instruments in country i and year t , and EL_{it} corresponds to the election date indicator defined in the previous section. As already mentioned, we include country-specific and year-specific intercepts – the fixed effects μ_i and λ_t entering on the fourth line together with the random error term u_{it} . For the rest, the specification of the estimating equation is largely based on the findings in Persson and Tabellini (2001).

Thus, we hold constant some country-specific and time-varying socio-economic variables (per-capita income, trade, and the two population variables) in the vector \mathbf{X}_{it} . As we want to find evidence of electoral cycles, it is important to allow for reasonably rich dynamics in the policy variables. Because the fiscal instruments display a great deal of inertia, we include the lagged dependent variable Y_{it-1} on the right hand side. And, because fiscal instruments tend to be highly cyclical, we also include our measure of cyclical deviations from trend $SHOCK_{it}$.

On the second and third lines of equation (1), S_{it} is an indicator (taking values 1 or 0) corresponding to our binary constitutional indicators, (*MAJ* and *PRES*) – as noted above these indicators mainly vary across countries. By the formulation on the second line, we allow the dynamics to differ across constitutional groups; specifically, we include interaction terms between both indicators *MAJ* and *PRES* and the lagged dependent variable in the regression. This is important to avoid confounding different general policy dynamics with different electoral cycles in different constitutional groups. In principle, all the controls in \mathbf{X}_{it} could also interact with the constitution. In practice, this does

too much crowding. If presidential elections are held every four years and legislative elections every other year, e.g., each year would either be a pre-election year or a post-election year.

not seem to be the case: for most variables and most specifications, we cannot reject the null hypothesis that the β coefficients are the same, irrespective of the constitutional state. Thus, besides the lagged dependent variable, we only interact our two constitutional indicators with income shocks. (See Persson and Tabellini, 2001 for an extensive discussion and the notes at the bottom of each Table to follow for more details on the precise specification.)

The main parameters of interest in this paper are the values of δ and η on the third line of (1). As explained above, we try to uncover systematic effects on each fiscal instrument, both before and after elections. And we also allow these effects to depend on the constitution. But here (unlike for the interaction with \mathbf{X}_{it} and Y_{it-1}), we sometimes interact the election dates with only one constitutional indicator at a time).

As suggested by the formulation, we generally estimate (1) by the method of (country and year) fixed effects. It is well-known, however, that if α^0 (and α^1) $\neq 0$, our estimate of this parameter remains biased even as the number of countries tends to infinity. The reason is that the initial condition, Y_{i0} , is correlated with the fixed country-component μ_i , which creates correlation of order $1/T$ between the lagged dependent variable and the random error term, u_{it} . This bias could spill over to our parameters of interest. Note, however, that the bias becomes smaller as the length of the panel, T , increases. When policy corresponds to the size of government or the budget deficit, the average country panel in our 40-year, 60-country data set is 26 years, and the bias is probably negligible. For welfare spending, we have, on average, 16 years per country, and the bias problem could be more relevant. The alternatives commonly used in the literature, such as estimation in first differences with IV or GMM estimators (originating in the work by Anderson and Hsiao, 1982, Arellano and Bond, 1991) also suffer from bias in small samples, however, especially when the number of panels N is small.³ In the following, we therefore report only the results from conventional fixed-effects estimation.

Another prospective econometric problem is that the election dates may not be exogenous. This is less important in presidential regimes, where elections are typically held on a fixed schedule with, say, four or six years in between elections. The concern is greater for parliamentary democracies, where the election date often reflects tactical choices of incumbents or government crises. Specifically, endogenous election dates may be correlated with the economic cycle: incumbent governments calling early elections when the economy is doing well, or government crises – and new elections – erupting when it is doing badly. That may bias our estimates of electoral cycles, as our policy instruments are expressed as percentages of GDP. But these prospective problems are addressed by our inclusion of the income shocks ($SHOCK_{it}$) among the controls, both alone and interacted with the constitutional indicators. These variables should account for any regime-specific correlation between the policy variable of interest and the election date induced by the economic cycle. And this, in turn, should rule out simultaneity bias from the error term being correlated with the election

³See Wooldridge (2001) for a discussion and comparison of the two methods.

date.

In the next section, we report our estimation results. We begin by constraining the coefficients of the election date indicators to be the same for all countries, irrespective of their constitution, and characterize the nature of unconditional electoral cycles in fiscal policy. Next, we allow these coefficients to differ with the electoral rule, contrasting majoritarian and proportional elections. We then allow the coefficients of the election date indicators to also vary with the form of government, contrasting presidential and parliamentary countries, before going to a full four-way split according to both constitutional rules.

4 Empirical results

4.1 Electoral cycles in fiscal policy

Table 1 shows the results when all constitutional groups are constrained to respond to the election date in the same way: we thus set $\delta^1 = \delta^0$ and $\eta^1 = \eta^0$ in equation (1). As mentioned above, we report the results for elections to the executive (the *ELEX* and *LELEX* indicators). Our broadest sample includes 522 executive elections, but that number is reduced a bit depending on data availability for the policy variables (especially welfare-state spending), and whether we study the sample of better democracies. The results corresponding to legislative elections are very similar. As already mentioned, the similarity is likely to reflect the coincidence of these two functions of elections in all parliamentary regimes, the coincidence of electoral dates in many presidential regimes and the lesser importance of mid-term elections.

We consider all fiscal policy variables defined in Section 2, namely overall spending (*CGEXP*), overall revenue (*CGREV*), budget surplus (*SPL*) and welfare spending (*SSW*). For each policy variable, we report the results from three different regressions. First, we estimate a minimalist specification, which only includes fixed country and year effects plus the lagged dependent variable, in addition to the current and lagged election indicator. In terms of equation (1), we thus set $\alpha^1 = \alpha^0$, $\gamma^1 = \gamma^0 = 0$ and $\beta = 0$. We then add all the covariates, and the constitutional interaction effects with Y_{it-1} and $SHOCK_{it}$. Finally, we keep the covariates, but impose a stricter definition of democracy where the threshold value of *POLITY* is raised from 0 to 8.

A number of regularities stand out. First, there is no significant effect on overall spending in the election year, but the estimated coefficient of lagged elections (*LELEX*) on spending is about -0.3 in all specifications and samples (columns 1-3). It is statistically significant, except in the sample of better democracies. Thus, on average, spending is reduced by 0.3% of GDP in the year after the elections. It appears that incumbent executives procrastinate over painful cuts in spending until the year after the election – alternatively, newly elected executives carry out necessary fiscal adjustments early on in their term. Second, taxes are cut by about 0.3% of GDP during an election year. Revenues are also raised after the elections, adding further evidence that painful adjust-

ments are postponed; but a significant post-election tax hike is only present in the better democracies (columns 4-6). Third, the budget surplus improves in the year after the election by about the same order of magnitude. It also deteriorates in the election year, but this pre-election effect is small and not statistically significant (columns 7-9). Finally, no electoral cycle is evident in social-security and welfare spending (columns 10-12). Contrary to the findings of earlier studies, there is no systematic evidence that worse democracies have larger electoral cycles.

These findings are broadly in line with our priors and the predictions of the literature on electoral cycles mentioned in the introduction. According to these models, both opportunistic and rent-seeking incumbents want to appear competent in the eyes of imperfectly informed voters just before the elections, and they do this by manipulating policy in the election year. Government revenues do indeed fall during an election year, as predicted by both the opportunistic and agency models of cycles. But government spending does not change in an average election year, the data are thus silent on the point where the two models deliver different predictions. Instead, spending cuts are postponed until after the elections. The latter effect seems to dominate on the government budget balance, since the surplus also improves after the elections. One interpretation of these findings is that tax revenue is easier to manipulate in a discretionary way, while aggregate government spending is more rigid so that its timing is harder to fine tune; in the wake of unpleasant spending cuts, politicians procrastinate and do not impose them until after the elections.⁴ Another possible explanation is that these unconditional results conceal systematic differences across different political systems. We now turn to this possibility.

4.2 Proportional vs. majoritarian democracies

Are the electoral cycles similar under proportional and majoritarian elections? To answer this question, we use the specification suggested by the third line of equation (1), splitting the two indicator variables for election years (current and lagged) into four, two for proportional and two for majoritarian electoral systems. For example, the *EL_MAJ* variable is defined as $MAJ * ELEX$, while the *EL_PRO* variable is defined as $(1 - MAJ) * ELEX$, and similarly for the lagged election variables. *Table 2* reports the results when we use these new indicators to estimate the same regression package as in the previous section. The table also reports the *F*-statistic for a test of the hypothesis that the coefficients on the election indicators are equal across electoral rules; in terms of equation (1), we thus test whether $\delta^1 = \delta^0$ and $\eta^1 = \eta^0$ can be rejected.

Different electoral rules do indeed seem to induce quite different electoral

⁴The finding of tax cuts in an election year is also in line with the empirical research quoted in the introduction. But the existing literature typically only estimated the coefficient of a single election dummy variable, not distinguishing between pre-election and post-election cycles (or imposing the restriction that the coefficients are the same but with opposite signs). Thus, the finding that painful fiscal adjustments tend to be delayed until after the election is new, to the best of our knowledge.

cycles. Starting with the aggregate variables, we find that the election-year tax cuts identified in the previous subsection seem to be common to both types of elections (columns 4-6). But the estimated tax cuts in majoritarian countries are more aggressive, amounting to about 0.6% of GDP. In proportional countries the tax cuts are smaller and not statistically different from zero. Even though the pre-election tax cuts in majoritarian countries are statistically significant, we cannot reject the stronger hypothesis that the policy shifts are the same majoritarian and proportional countries.

Majoritarian countries cut spending during election years – though the estimated coefficients are smaller and less precisely estimated than those of the tax cuts (columns 1-3). Here, the election date has no effect in proportional countries, and the difference between majoritarian and proportional countries is now marginally significant. The post-election cycle with spending and deficit cuts estimated in the previous subsection, does not seem to be perceptibly different across electoral systems, even though the coefficients are more precisely estimated (and only reach statistical significance) in proportional countries (columns 7-9).

The results for welfare-state spending (columns 10-12) are more stark. Proportional elections are associated with hikes in welfare-state spending: transfers increase by 0.2% of GDP in the election year and by almost as much in the post-election year. If anything, this component of spending falls under majoritarian elections, and the difference across electoral rules is highly significant (particularly for the pre-election cycle). These results contrast sharply with the cycle in aggregate fiscal variables.

Our findings in *Table 2* can be interpreted in light of the priors outlined in the introduction. On the one hand, majoritarian elections do induce more pronounced cycles in aggregate fiscal policy compared to proportional elections. This is in line with the idea that electoral accountability and incentives to perform well are stronger under plurality rule. The pre-election tax *cum* spending cuts in majoritarian countries are consistent with agency models of political cycles, such as Besley and Case (1995) and Persson and Tabellini (2000). Interestingly, our results for majoritarian countries are similar to Besley and Case's (1995) findings of pre-election tax and spending cuts in US-state executive elections. If anything, the pre-election cycle estimated in proportional countries is more consistent with the opportunistic/traditional political business cycle. On the other hand, expansions in welfare-state spending in the proximity of elections are only observed in proportional countries. This spending component includes broad programs like pensions and unemployment insurance. This finding is thus consistent with the theoretical idea that proportional electoral rules induce politicians to seek support among broad coalitions of voters, while majoritarian electoral rules instead induce them to target spending to smaller (geographical) groups, once we assume that these incentives are particularly strong at election time.

Overall, the results in this subsection rhyme well with another general idea mentioned in the introduction, namely that majoritarian elections is mainly a vehicle for promoting accountability while proportional elections are mainly a

vehicle for promoting representation.

4.3 Parliamentary vs. presidential democracies

We next turn to differences in electoral cycles among democracies with different forms of government. In analogy with the approach in the previous subsection, we create four different indicator variables, interacting the election dates with the regime indicator: $EL_PRE = PRES * ELEX$, $EL_PAR = (1 - PRES) * ELEX$, and analogously for the lagged election dates. Using these new indicators in the estimation for our four fiscal instruments generates the results displayed in *Table 3*.

The results strongly suggest that the post-election cycle in overall government spending, taxes and the surplus identified in subsection 4.1 is due predominantly to the presidential countries. Governments in presidential regimes cut spending considerably just after the election, by between 0.5 and 1% of GDP. They also postpone tax hikes by about the same magnitude, with corresponding large effects on the surplus, which improves by about 0.8 % of GDP after a typical presidential election. Some post-election spending and deficit adjustments also appear to take place among parliamentary regimes, but these effects are smaller and not statistically significant. The post-election differences between the two regime types are strongest (and highly significant) for taxes.

As already suggested by the split according to electoral rules, systematic pre-election tax cuts are common to all countries. They are stronger and more precisely estimated in the parliamentary regimes, however, where the estimates suggest tax cuts of about 0.6% of GDP in an average election year. The results for welfare-state spending do not indicate pronounced effects anywhere, except perhaps among the better democracies where parliamentary governments raise this component of spending after elections while presidential governments seem to cut it along with aggregate spending.

The post-election cycles in presidential countries are intriguing and existing theory does not suggest a straightforward explanation. One difference between these two regimes is that election dates in presidential regimes are generally fixed, while they are endogenous in most parliamentary countries (Norway is one of few exceptions). As mentioned above, however, we deal with the potential simultaneity problem by including income shocks in our econometric specification. The difference between the regimes is thus not likely to be a statistical artifact.

The different rules for legislative bargaining recalled in the introduction may provide an interpretation of the post-election cycle. Presidential regimes tend to have more decision makers with proposal and veto rights than parliamentary regimes – for instance, in many countries both the president and the legislature have to approve the budget. The possibility of fiscal deadlock might accordingly be more serious, particularly in the case of divided government, i.e., when the president and congress belong to different parties, or when the congress does not have a well-defined majority party. Each decision maker may be able to veto painful adjustments before elections, but none may have the strength to pass

deliberate fiscal expansions or tax cuts. In parliamentary democracies, instead, the same majority typically controls the executive and approves the budget, and is thus better able to fine-tune fiscal policy to its electoral concerns.⁵ Testing this explanation would require careful data collection and coding of the partisan identity of presidents and legislative majorities.

But this is not the only plausible interpretation. Another possibility, suggested by empirical research, is that presidential countries are more likely to face binding government borrowing constraints. Persson and Tabellini (2001) show that presidential countries tend to have procyclical, rather than countercyclical, fiscal policy. If governments in presidential countries do face tighter borrowing constraints, they may also have to undertake more painful fiscal adjustments than parliamentary democracies. Perhaps it is optimal to postpone such painful adjustments until after the elections. Indeed, empirical research by Frieden and Stein (2001) has found robust evidence that exchange-rate devaluations tend to be postponed until after the presidential elections in many Latin American countries – a continent where presidential regimes are over-represented. The results in the next section presents some indirect evidence regarding this interpretation.

4.4 A four-way constitutional split

So far, we have chosen to look for system-dependent electoral cycles in parsimonious specifications, where we only condition on one constitutional difference at a time. While the tests for different cycles are valid under the null hypothesis of no differences, the reader may legitimately ask whether both specifications can be true at the same time, to the extent that we find differences across constitutional features. The answer is probably in the negative: even under our implicit assumption that any constitutional differences are additive, the estimates will still be biased if the frequency of the left-out constitutional feature, say the form of government, differs across the included feature, say the electoral rule. The likely culprit here is that our sample includes very few elections in presidential countries with majoritarian electoral rules. For 475 elections, where we also have data on government spending, only 22 are thus associated with these constitutional features, whereas the other three types are much better represented (for presidential countries with proportional elections, we have 117 elections, while for parliamentary-majoritarian and parliamentary-proportional, we have 133 and 200 elections, respectively). This means that our estimates of the cycle under majoritarian elections in Section 4.2 may be biased in the direction of the cycle found for parliamentary countries in Section 4.3 (if different from the presidential cycle). Conversely, estimates of the cycle in presidential countries in Section 4.3 may be biased in the direction of the cycle found for proportional elections in Section 4.2 (if different from the majoritarian cycle).

⁵This reasoning is similar to the idea in the literature on US state fiscal policy that legislative institutions – such as a governor’s line-item veto – has more bite on taxes, spending and deficits under divided government, an idea that has received some electoral support. See Besley and Case (2002) for an extensive survey of this literature.

To address this issue and to further understand the roots of our results, we condition the electoral-cycle estimates on four separate constitutional groups (labeled *EL_MAJPRE*, and so on, in obvious notation). *Table 4* shows the estimates of pre-and post-election cycles in these four groups.

Among the key findings in *Table 2* are the unique pre-election spending cuts and stronger pre-election tax cuts under majoritarian elections. Are these driven by the higher frequency of parliamentary countries and the regime differences found in *Table 3*, as the above discussion suggests might be the case? The results in *Table 4* indicates that the answer is no (see the upper part of columns 1-6). The coefficients show that election-year spending and tax cuts are present in both the presidential (called *EL_MAJPRE*) and parliamentary (called *EL_MAJPAR*) subgroups of majoritarian countries. Moreover, the cuts are larger among the majoritarian-presidential democracies for all the specifications. A more balanced sample (with more presidential countries) would thus have produced even larger estimates (in absolute value) in *Table 2*.

The key finding in *Table 3* is the uniqueness of the post-election fiscal adjustment to the presidential democracies. Here, the results in *Table 4* (the lower part of columns 1-9) do indeed suggest that the results are driven by the higher frequency of proportional-presidential than majoritarian-presidential democracies. While the post-election fiscal adjustments go in the same direction in both these groups, they are always larger in the proportional-presidential subgroup. Since this group is predominant in Latin-America, the results give some indirect support for the interpretation offered at the end of the previous subsection.

A final result worth noting concerns the finding in *Table 2* of electoral cycle in welfare-state spending being uniquely associated with proportional electoral rules. The estimates in *Table 4* (columns 10-12) show that the results for the pre-election cycle reflect hikes in the parliamentary and presidential subgroups alike. But post-election spending in welfare spending are found exclusively among proportional-parliamentary countries (which include many of the European welfare states).

5 Final remarks

We have discovered strong constitutional effects on the presence and nature of electoral cycles in fiscal policy. Only countries with majoritarian elections cut spending during election years, and cut taxes more than countries on proportional elections. Only proportional democracies raise welfare spending before elections, with further commitments for the post-election year. Only presidential regimes postpone unpopular fiscal policy adjustments until after the elections.

According to our findings, electoral cycles are prominent under both types of electoral rules, but they take different forms. As already discussed, these results are not inconsistent with our theoretical priors. They are also consistent with the general notion among political scientists that majoritarian elections are geared towards keeping politicians accountable, whereas proportional elections are geared towards keeping voters represented. The findings on cycles under

different forms of government are equally stark, but provide a more genuine puzzle. While both types of government on average follow the expected pattern of cutting taxes ahead of elections, it is only presidential governments that hold off spending cuts and tax increases until the incumbent has been re-elected or a new president installed. Why do we observe this difference?

The findings suggest an interesting research agenda. To better understand the patterns in the data, we need to work on both theoretical and empirical fronts. One task is to extend the static models in the recent literature on the comparative politics of policymaking to a genuine dynamic setting. These extensions ought to be consistent with our findings in this exploratory paper, but they should also generate new testable hypotheses. Our empirical findings suggest that it may also be worthwhile to dig deeper into the institutional details, studying the effects of term limits for elected presidents, specific veto rights, or the specific rules for breaking up parliamentary governments. But we also need to combine the data on fiscal policy with data on political outcomes, especially voting results and party affiliations of incumbent presidents and legislative majorities. While such data are readily available for the developed democracies, studying the full 40-year, 60-country panel in this paper requires a substantial investment in data collection.

DATA APPENDIX

CGEXP : central government expenditures as a percentage of GDP. Constructed using the item Government Finance - Expenditures in the IFS, divided by the GDP at current prices and multiplied by 100. Source: IMF - IFS CD-Rom and IMF - IFS Yearbook.

CGREV : central government revenues as a percentage of GDP. Constructed using the item Government Finance - Revenues in the IFS, divided by the GDP at current prices and multiplied by 100. Source: IMF - IFS CD-Rom and IMF - IFS Yearbook.

ELEX : executive elections. Dummy variable which equals 1 in a year when the executive is elected, and 0 otherwise. Takes into consideration both presidential elections and legislative elections. Source: <http://www.ifes.org/eguide/elecguide.htm> plus other national sources.

ELLEG : legislative elections. Dummy variable which equals 1 in the year the legislature is elected, independently from the form of government. Source: <http://www.ifes.org/eguide/elecguide.htm> plus other national sources.

ELMAJ : $MAJ * ELEX$. Dummy variable showing the interaction between executive elections and majoritarian electoral systems. Source: see *ELEX* and *MAJ*.

ELPRO : $(1 - MAJ) * ELEX$. Dummy variable showing the interaction between executive elections and proportional electoral systems. Source: see *ELEX* and *MAJ*.

ELPAR : $(1 - PRES) * ELEX$. Dummy variable showing the interaction between executive elections and parliamentary regimes. Source: see *ELEX* and *PRES*.

ELPRE : $PRES * ELEX$: Dummy variable which showing the interaction between executive elections and presidential regimes. Source: see *ELEX* and *PRES*.

LYP : natural log of the per capita real GDP. Sources: Penn World Tables - mark 5.6 (PW); Easterly's series on www.worldbank.org; The World Bank's World Development Indicators (WDI).

LELEX : One-period lagged series of *ELEX*. Source: see *ELEX*.

MAJ: dummy variable for electoral systems. Equals 1 in presence of (exclusively) either a majority or a plurality rule, 0 otherwise. Only legislative elections (lower house) are considered. Source: Persson and Tabellini (2003)

POLITY : The *POLITY* score is computed by subtracting the *AUTO*C score from the *DEMO*C score; the resulting unified polity scale ranges from +10 (strongly democratic) to -10 (strongly autocratic). Source: Polity IV Project (<http://www.cidcm.umd.edu/inscr/polity/index.htm>).

PRES : dummy variable for government regimes. Equals 1 in presence of presidential regimes, 0 otherwise (Parliamentary). Only those regimes where the confidence of the assembly is not necessary for the executive (even if the president is not chief executive, i.e., assembly-independent) are included among presidential regimes. Premier-presidential (semi-presidential like France) and

president-parliamentary systems (like Ecuador) are generally classified as parliamentary. Source: Persson and Tabellini (2003).

PROP1564: percentage of population between 15 and 64 years old in the total population. Source: World Development Indicators CD-Rom 1999.

PROP65 : percentage of population over the age of 65 in the total population. Source: World Development Indicators CD-Rom 1999.

SHOCK : deviation of aggregate output from its trend value in percent. Difference between the natural log of the real GDP in the country and its country-specific trend (computed using the Hodrick-Prescott filter).

SPL : central government surplus (deficit if negative) as a percentage of GDP. Constructed using the item Government Finance - Deficit and Surplus in the IFS, divided by the GDP at current prices and multiplied by 100. Source: IMF - IFS CD-Rom and IMF - IFS Yearbook.

TRADE : sum of exports and imports of goods and services measured as a share of GDP. Source: The World Bank's World Development Indicators CD-Rom 2000.

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Table 1
Electoral cycles in fiscal policy 1960-1998
Executive elections

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep var	<i>CGEXP</i>	<i>CGEXP</i>	<i>CGEXP</i>	<i>CGREV</i>	<i>CGREV</i>	<i>CGREV</i>	<i>SPL</i>	<i>SPL</i>	<i>SPL</i>	<i>SSW</i>	<i>SSW</i>	<i>SSW</i>
<i>ELEX</i>	-0.06 (0.16)	-0.04 (0.16)	0.03 (0.19)	-0.34 (0.14)**	-0.36 (0.14)**	-0.37 (0.16)**	-0.13 (0.15)	-0.14 (0.15)	-0.12 (0.16)	0.08 (0.06)	0.06 (0.06)	0.08 (0.07)
<i>LELEX</i>	-0.33 (0.16)**	-0.32 (0.16)**	-0.24 (0.18)	0.18 (0.14)	0.16 (0.14)	0.35 (0.16)**	0.36 (0.14)**	0.35 (0.15)**	0.40 (0.16)**	0.08 (0.06)	0.05 (0.06)	0.07 (0.07)
Sample	Full	Full	Narrow	Full	Full	Narrow	Full	Full	Narrow	Full	Full	Narrow
Covariates	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	1621	1546	1213	1561	1497	1174	1574	1520	1179	969	929	752
Countries	60	60	53	59	59	53	60	60	53	57	56	48
Adj. R2	0.81	0.81	0.83	0.81	0.81	0.83	0.51	0.53	0.59	0.77	0.77	0.79

Standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

R2 refers to within R2

All regressions include fixed country and year effects and the lagged dependent variable

Covariates include: *LYP*; *TRADE*; *PROP1564*; *PROP65*; *SHOCK*, alone and interacted with *MAJ* and *PRES*; lagged dependent variable interacted with *PRES* and *MAJ*.

Table 2
Electoral cycles in fiscal policy 1960-1998
Alternative electoral rules

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep var	<i>CGEXP</i>	<i>CGEXP</i>	<i>CGEXP</i>	<i>CGREV</i>	<i>CGREV</i>	<i>CGREV</i>	<i>SPL</i>	<i>SPL</i>	<i>SPL</i>	<i>SSW</i>	<i>SSW</i>	<i>SSW</i>
<i>EL_MAJ</i>	-0.43 (0.27)	-0.44 (0.28)	-0.43 (0.32)	-0.62 (0.24)***	-0.60 (0.24)**	-0.54 (0.28)*	-0.14 (0.25)	-0.18 (0.25)	-0.07 (0.27)	-0.12 (0.10)	-0.12 (0.10)	-0.16 (0.12)
<i>EL_PRO</i>	0.15 (0.20)	0.16 (0.20)	0.26 (0.23)	-0.18 (0.18)	-0.23 (0.18)	-0.29 (0.20)	-0.12 (0.18)	-0.12 (0.18)	-0.15 (0.19)	0.19 (0.08)**	0.17 (0.08)**	0.21 (0.09)**
<i>LEL_MAJ</i>	-0.29 (0.28)	-0.23 (0.28)	-0.10 (0.33)	0.06 (0.24)	0.13 (0.25)	0.34 (0.28)	0.25 (0.25)	0.29 (0.25)	0.41 (0.28)	-0.07 (0.10)	-0.05 (0.10)	-0.07 (0.12)
<i>LEL_PRO</i>	-0.33 (0.20)*	-0.36 (0.20)*	-0.31 (0.22)	0.25 (0.17)	0.19 (0.18)	0.36 (0.20)*	0.43 (0.18)**	0.39 (0.18)**	0.40 (0.19)**	0.16 (0.08)**	0.11 (0.08)	0.15 (0.09)*
<i>F: MAJ=PRO</i>	2.98*	2.99*	3.08*	2.21	1.48	0.56	0.01	0.04	0.07	6.03**	5.27**	6.29**
<i>F: LMAJ=LPRO</i>	0.01	0.15	0.27	0.42	0.04	0.01	0.32	0.11	0.0	3.45*	1.68	2.13
Sample	Full	Full	Narrow	Full	Full	Narrow	Full	Full	Narrow	Full	Full	Narrow
Covariates	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	1621	1546	1213	1561	1497	1174	1574	1520	1179	969	929	752
Countries	60	60	53	59	59	53	60	60	53	57	56	48
Adj. R2	0.81	0.81	0.83	0.81	0.81	0.83	0.51	0.53	0.59	0.77	0.78	0.79

Standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

R2 refers to within R2

All regressions include fixed country and year effects and the lagged dependent variable

Covariates include: *LYP*; *TRADE*; *PROP1564*; *PROP65*; *SHOCK*, alone and interacted with *MAJ* and *PRES*; lagged dependent variable interacted with *PRES* and *MAJ*.

Table 3
Electoral cycles in fiscal policy 1960-1998
Alternative forms of government

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep var	<i>CGEXP</i>	<i>CGEXP</i>	<i>CGEXP</i>	<i>CGREV</i>	<i>CGREV</i>	<i>CGREV</i>	<i>SPL</i>	<i>SPL</i>	<i>SPL</i>	<i>SSW</i>	<i>SSW</i>	<i>SSW</i>
<i>EL_PRE</i>	-0.26 (0.32)	-0.18 (0.32)	-0.18 (0.43)	-0.20 (0.28)	-0.26 (0.29)	-0.02 (0.37)	-0.23 (0.28)	-0.29 (0.28)	-0.08 (0.36)	0.10 (0.13)	0.07 (0.13)	0.07 (0.16)
<i>EL_PAR</i>	0.01 (0.19)	0.02 (0.19)	0.09 (0.21)	-0.40 (0.16)**	-0.40 (0.17)**	-0.46 (0.18)***	-0.11 (0.17)	-0.10 (0.17)	-0.14 (0.18)	0.08 (0.07)	0.06 (0.07)	0.08 (0.08)
<i>LEL_PRE</i>	-0.58 (0.31)*	-0.70 (0.32)**	-0.93 (0.42)**	0.77 (0.28)***	0.61 (0.29)**	1.02 (0.36)***	0.79 (0.28)***	0.71 (0.28)**	0.83 (0.35)**	-0.03 (0.12)	-0.10 (0.12)	-0.17 (0.16)
<i>LEL_PAR</i>	-0.23 (0.19)	-0.19 (0.19)	-0.07 (0.21)	-0.02 (0.16)	0.01 (0.17)	0.19 (0.18)	0.21 (0.17)	0.23 (0.17)	0.29 (0.18)*	0.11 (0.07)*	0.10 (0.07)	0.13 (0.08)*
<i>F: PRE=PAR</i>	0.57	0.31	0.30	0.35	0.19	1.12	0.14	0.32	0.02	0.03	0.00	0.00
<i>F: LPRE=LPAR</i>	0.89	1.88	3.39*	6.11**	3.28*	4.20**	3.10*	2.15	1.85	1.00	1.84	2.84*
Sample	Full	Full	Narrow	Full	Full	Narrow	Full	Full	Narrow	Full	Full	Narrow
Covariates	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	1621	1546	1213	1561	1497	1174	1574	1520	1179	969	929	752
Countries	60	60	53	59	59	53	60	60	53	57	56	48
Adj. R2	0.81	0.81	0.83	0.81	0.81	0.83	0.51	0.53	0.59	0.77	0.77	0.79

Standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

R2 refers to within R2

All regressions include fixed country and year effects and the lagged dependent variable

Covariates include: *LYP*; *TRADE*; *PROP1564*; *PROP65*; *SHOCK*, alone and interacted with *MAJ* and *PRES*; lagged dependent variable interacted with *PRES* and *MAJ*.

Table 4
Electoral cycles in fiscal policy 1960-1998
Alternative constitutional groups

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep var	CGEXP	CGEXP	CGEXP	CGREV	CGREV	CGREV	SPL	SPL	SPL	SSW	SSW	SSW
<i>EL_MAJPRE</i>	-1.22 (0.71)*	-1.03 (0.70)	-0.84 (0.81)	-1.16 (0.60)*	-1.07 (0.61)*	-0.72 (0.69)	0.23 (0.63)	0.20 (0.63)	0.16 (0.63)	0.04 (0.26)	0.00 (0.26)	-0.08 (0.29)
<i>EL_PROPRES</i>	-0.01 (0.36)	0.05 (0.36)	0.10 (0.51)	0.07 (0.32)	-0.02 (0.33)	0.27 (0.45)	-0.35 (0.32)	-0.42 (0.32)	-0.19 (0.35)	0.12 (0.14)	0.10 (0.15)	0.15 (0.19)
<i>EL_MAJPAR</i>	-0.29 (0.30)	-0.32 (0.31)	-0.35 (0.35)	-0.53 (0.25)**	-0.52 (0.27)*	-0.52 (0.30)*	-0.21 (0.27)	-0.26 (0.28)	-0.14 (0.31)	-0.14 (0.11)	-0.14 (0.11)	-0.18 (0.13)
<i>EL_PROIPAR</i>	0.24 (0.24)	0.23 (0.24)	0.31 (0.25)	-0.31 (0.21)	-0.33 (0.22)	-0.43 (0.22)*	-0.02 (0.22)	0.00 (0.22)	-0.18 (0.22)	0.22 (0.09)**	0.20 (0.09)**	0.22 (0.10)**
<i>LEL_MAJPRE</i>	0.12 (0.71)	-0.25 (0.71)	-0.29 (0.78)	0.37 (0.61)	0.30 (0.61)	0.53 (0.67)	0.19 (0.65)	0.52 (0.65)	0.51 (0.65)	-0.03 (0.27)	-0.09 (0.27)	-0.08 (0.28)
<i>LEL_PROPRES</i>	-0.76 (0.35)**	-0.81 (0.36)**	-1.21 (0.50)**	0.88 (0.31)***	0.71 (0.33)**	1.23 (0.44)***	0.93 (0.31)***	0.76 (0.32)**	0.75 (0.36)**	-0.02 (0.14)	-0.09 (0.14)	-0.21 (0.19)
<i>LEL_MAJPAR</i>	-0.36 (0.30)	-0.22 (0.31)	-0.06 (0.36)	-0.00 (0.25)	0.09 (0.27)	0.29 (0.30)	-0.06 (0.27)	0.26 (0.27)	0.25 (0.30)	0.29 (0.11)	-0.07 (0.11)	-0.06 (0.14)
<i>LEL_PROIPAR</i>	-0.13 (0.24)	-0.16 (0.24)	-0.07 (0.25)	-0.04 (0.21)	-0.04 (0.21)	0.14 (0.22)	0.19 (0.22)	0.23 (0.22)	0.27 (0.22)	0.24 (0.09)***	0.19 (0.09)**	0.24 (0.10)**
Sample	Full	Full	Narrow	Full	Full	Narrow	Full	Full	Narrow	Full	Full	Narrow
Covariates	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	1621	1546	1213	1561	1497	1174	1574	1518	1325	969	929	752
Countries	60	60	53	59	59	53	60	60	58	57	56	48
Adj. R2	0.81	0.81	0.83	0.81	0.81	0.83	0.51	0.53	0.54	0.77	0.77	0.79

Standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

R2 refers to within R2

All regressions include fixed country and year effects and the lagged dependent variable

Covariates include: *LYP*; *TRADE*; *PROP1564*; *PROP65*; *SHOCK*, alone and interacted with *MAJ* and *PRES*; lagged dependent variable interacted with *PRES* and *MAJ*.