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**Real-time determinants of fiscal policies in the euro area:  
Fiscal rules, cyclical conditions and elections**

by Roberto Golinelli and Sandro Momigliano

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# **REAL-TIME DETERMINANTS OF FISCAL POLICIES IN THE EURO AREA: FISCAL RULES, CYCLICAL CONDITIONS AND ELECTIONS**

by Roberto Golinelli\* and Sandro Momigliano\*\*

## **Abstract**

We examine the impact of four factors on the fiscal policies of the euro-area countries over the last two decades: the state of public finances, the European fiscal rules, cyclical conditions and general elections. We rely on information actually available to policy-makers at the time of budgeting in constructing our explanatory variables. Our estimates indicate that policies have reacted to the state of public finances in a stabilizing manner. The European rules have significantly affected the behaviour of countries with excessive deficits. Apart from these cases, the rules appear to have reaffirmed existing preferences. We find a relatively large symmetrical counter-cyclical reaction of fiscal policy and strong evidence of a political budget cycle. The electoral manipulation of fiscal policy, however, occurs only if the macroeconomic context is favourable.

JEL classification numbers: E61, D72, E62, H60

Keywords: fiscal policy, real-time information, euro-area countries, stabilisation policies, fiscal rules, political budget cycle

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## 1. Introduction<sup>1</sup>

Over the last decade, a large body of literature has analysed the characteristics of fiscal policies in the OECD countries (e.g. Bohn, 1998; Melitz, 2000; European Commission, 2001; Ballabriga and Martinez-Mongay, 2002; Buti, 2002 and IMF, 2004). In this paper we contribute to this area of research in three respects.

First, we use the same model to analyse the role of the following four factors: (i) the initial state of public finances, (ii) the European fiscal rules, (iii) cyclical conditions and (iv) the political budget cycle. Previous studies have often focused on one specific factor, adding a number of control variables that are often not fully discussed. By including all four factors and by carefully specifying them, we hope to avoid the risk of biased estimates arising from omitted variables. Moreover, we explicitly derive our model from a very general one, checking the restrictions that we impose on it.

Second, we focus on the euro-area countries, whereas many studies include all OECD countries for which data are available. We show that the fiscal policies in the euro area are relatively homogeneous, while this is not true for our full sample of OECD countries.

Finally, unlike most studies, this one explains fiscal policies largely on the basis of the information actually available at the time budgetary decisions were taken and not on the basis of the latest available (*ex post*) data. This choice is inspired by the work of Orphanides (1998, 2001) who has shown how important real-time data are to understand monetary policy in the United States. A few recent papers have taken the same direction, controlling for errors in forecasting when assessing the response of fiscal policy to cyclical conditions and elections (Larch and Salto, 2003; Buti and Vand den Noord, 2003 and 2004; Mink and De Haan, 2005). However, cyclical conditions are still measured on the basis of *ex post* data. Forni and Momigliano (2004) assess the budgetary reaction to cyclical conditions over the last decade in the euro area and in the OECD countries on the basis of both real-time and *ex post* estimates of output gaps. They show that the use of *ex post* data may significantly bias

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<sup>1</sup> The paper greatly benefited from the hospitality of the Burch Center, UCA Berkeley, extended to one of the authors and the many conversations he had with Alan Auerbach. A preliminary version of the paper was presented at the 8th Banca d'Italia Workshop on Public Finance (Perugia, 30 March-1 April 2006). We wish to thank Matthias Mohr for helping us in the application of the statistical filter proposed in Mohr (2005 and 2006), (continues)

the estimates. Here, we also use real-time data for the general government balance, given that in some countries (in particular, Greece) significant revisions have occurred in the sample period. Furthermore, we include election dummies among the regressors and extend the period of analysis to the years before Maastricht, which allows us to discuss the role played by the European rules.

The rest of the paper is structured as follows. In Section 2 we discuss the specification of the fiscal rule we estimate. In Section 3 we describe the data set used in our analysis, focusing largely on the construction of the real-time estimates of cyclical conditions. In Section 4 we analyse our main results and present some robustness exercises. In Sections 5-7, respectively, we discuss in detail the impact on fiscal policies of the state of public finances and the European fiscal rules, the cyclical conditions and the position in the electoral cycle. In Section 8 we examine how our estimates change if we use *ex post* instead of real-time data. Section 9 concludes. In Appendix 1 we show how we derive our base model, which implies the estimation of 25 parameters (of which 19 are time dummies), from a general specification with 91 parameters. In Appendix 2 we present additional tests of the robustness of our results.

## 2. Model specification and statistical validation

As in a number of studies (e.g. Taylor, 2000; European Commission, 2001; Auerbach, 2002; Cohen and Follette, 2003; Galí and Perotti, 2003),<sup>2</sup> we estimate a fiscal rule in which the discretionary fiscal action, measured by the change in the cyclically-adjusted primary balance,<sup>3</sup> is explained by the cyclical conditions (measured by the output gap) and the state of public finances (measured by the primary balance and the debt of the general government). In addition, we include two explanatory variables meant to capture the

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<sup>2</sup> Some authors, among which Galí and Perotti (2003), use as dependent variable the level of the CAPB, instead of its change. In principle, if we had included, as those authors do, the lagged level of the dependent variable among the regressors, the two specifications would be equivalent (giving the same estimates for all coefficients except for that of the lagged dependent variable, for which our estimates would be equal to those of the other specification plus 1). In fact, we use among the regressors the primary balance not adjusted for the cycle, so that there is not a strict correspondence between the two specifications.

<sup>3</sup> We are aware that the change in CAPB gauges with some error the discretionary actions taken by the fiscal authorities but, in our opinion, there is no alternative proposed in the literature that is clearly preferable.



electoral cycle and one meant to capture the impact of the European fiscal rules on the behaviour of countries that were in an excessive deficit position.

As for the latter regressor, we basically follow Forni and Momigliano (2004) in introducing a regressor,  $m_{it}$  (also referred to as the Maastricht variable) which defines a *benchmark* correction of the primary balance which is a function of the excessive deficit, the number of years in which the latter needs to be eliminated and the expected contribution from interest payments (see Box below).<sup>4</sup>

### **Box – Modelling the European fiscal rules**

*When modelling the European fiscal rules, as defined by the Maastricht Treaty and the Stability and Growth Pact, we focus only on the requirement to correct the deficit when it exceeds the 3% of GDP threshold. In particular, we do not include an explicit rule for the medium-term target of a “close to balance or in surplus budgetary position” (introduced by the Stability Pact in 1997) for two reasons. First, meeting the target is not supported by any formal sanction and it rests largely on the country’s willingness to comply. Second, the rule is not fully defined (and the same applies to the “medium-term targets” differentiated across countries in the new version of the Pact introduced in 2005).<sup>(a)</sup> As a matter of fact, the reactions to the initial conditions that we find in operation, at least since 1988, are broadly consistent with meeting the medium-term targets.*

*The Maastricht variable  $m_{it}$  is set equal to zero in the years before 1992 or if the deficit is below the 3% threshold. For the years 1992-96,  $m_{it}$  is equal to the difference between the deficit and 3% of GDP, divided by the number of years leading up to 1997<sup>(b)</sup> and then reduced by the expected change in interest expenditure in the following year. Formally:*

$$m_{it} = [ (ob_{it} - (-3\%)) / [1998 - (t+1)] ] - \Delta in_{it+1}$$

*where  $\Delta$  is the first-difference operator, all variables are defined as a ratio to GDP;  $ob$  is the overall balance (a negative value corresponds to a deficit) and  $in$  is the interest payment, subscripts  $i$  and  $t$  refer, respectively, to the individual countries and to the year. The formula implies a reduction of the excessive deficit (i.e. above the 3% threshold) inversely proportional to the number of years leading up to 1997 and net of the contribution expected from interest payments. After 1996, the provisions of the Stability and Growth Pact (in principle, also of its 2005 version) require countries to correct an*

<sup>4</sup> We differ from the proposal in Forni and Momigliano (2004) essentially in two respects. First, when computing the needed correction of the primary deficit, we subtract the expected change in interest payments. We do so because, especially for the years 1992-96, for some countries the contribution to the consolidation coming from the fall in interest rates was large and could be forecast with a significant precision. Second, as a result of specific tests (see Appendix 1), when the Maastricht variable is different from zero, we exclude all the other explanatory variables from the fiscal rule.

*excessive deficit in the year after its official recognition, which usually occurs with a one-year lag. Therefore, in the first year that an excessive deficit occurs, we substitute in the denominator of the formula the constant 2 to the expression  $[1998 - (t+1)]$ . If the excessive deficit persists,  $m_{it}$  equals the full difference with respect to the threshold, net of the expected contribution from interest payments.*

*Throughout the period 1992-2006, if the expected reduction in interest expenditure is larger than the correction required in  $t+1$  for the overall balance,  $m_{it}$  is set to zero. Therefore  $m_{it}$  takes either a negative sign or is equal to zero.*

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*(a) The reform of the Growth and Stability Pact, endorsed by Ecofin in April 2005, is based on two new European Council Regulations: Council Regulation 1055/2005, amending Council Regulation 1466/97, and Council Regulation 1056/2005, amending Council Regulation 1467/97.*

*(b) Participation in the Monetary Union required achieving a deficit smaller than 3% of GDP in 1997. For Greece, the reference period is extended up to 1998, the year in which the country qualified for entering the Union.*

Our base fiscal rule (hereinafter, base model) is the result of a process of reduction from a very general specification, in which it is nested. In the process, all the restrictions that we impose are validated by statistical tests, which indicate that the restricted model does not entail a loss of relevant information (the procedure followed and the test results are reported in Appendix 1).

The general unrestricted model (GUM), in addition to the six policy parameters mentioned above, allows for: (i) fixed country and time effects, (ii) different parameters for the Maastricht variable for the period 1993-97 and for the period 1998-2006, and (iii) five dummy variables for Germany for the years 1990-94, meant to control for the unification process. Moreover, the GUM allows for different values for the set of parameters (including country and time effects) depending on whether a country is or is not in a situation of excessive deficit (more precisely, on whether our Maastricht variable is negative or equal to zero) and on whether the output gap is positive or negative. In principle, this specification requires the estimation of four sets of parameters, depending on the sign of the output gap and of the Maastricht variable. However, when the latter differs from zero, output gaps are always negative and this reduces the number of sets to three. The absence of observations for other intersections of states further reduces the number of country and time effects

parameters to be estimated. Overall, the GUM has 91 parameters, including 31 individual effects and 38 time effects.

The base model resulting from the above-mentioned process of reduction from the GUM includes 25 parameters, 19 of which are time dummies. A particularly noteworthy result is represented by the elimination of the fixed effects, *i.e.* the systematic effects related to individual countries. Contrary to previous studies, we find that they are not statistically significant, indicating that fiscal policies in the euro area tend to be relatively homogeneous, once their main determinants are taken into account.<sup>5</sup>

The base model is represented by two equations, which apply depending on whether the Maastricht variable is negative or equal to zero.

If the Maastricht variable is equal to zero (*i.e.*, either the year precedes 1992 or the deficit does not exceed the threshold or the required correction in  $t+1$  of the overall balance is less than the expected contribution from interest expenditure) the specification of the fiscal rule is:

$$[1a] \quad \Delta capb_{it} = \phi_{pb} pb_{it-1} + \phi_d d_{it-1} + \phi_x x_{it-1} + \phi^p_{e1} e^p_{it} + \phi^p_{e2} e^p_{it+1} + u_{it}$$

where all the variables except the dummies for elections are defined as a ratio to GDP,  $capb$  is the cyclically-adjusted primary budget balance (a negative sign indicates a deficit),  $pb$  is the primary balance,  $d$  is the debt level,  $x$  is the output gap,  $e^p$  is a dummy variable equal to 1 in the year of regular elections (defined as those held at the end of a full term) if the output gap is positive when budgetary decisions are taken, and subscripts  $i$  and  $t$  refer, respectively, to the individual countries and to the year. Finally, the error-term  $u$  embodies time effect  $\lambda_t$  and random  $\varepsilon_{it}$  unobservable components.

The coefficients  $\phi_{pb}$  and  $\phi_d$  gauge the impact on fiscal policies of the state of public finances at the time budgetary decisions are taken ( $t-1$ ): a negative value of  $\phi_{pb}$  and a positive value of  $\phi_d$  indicate that the higher the initial levels of debt and deficit, the greater the tightening of fiscal policy.

The coefficient  $\phi_x$  (positive if policies are countercyclical) captures the response of budgetary actions to *current* cyclical conditions, *i.e.* the cyclical conditions of the year in

which budgetary decisions are taken ( $t-1$ ). The variable  $x_{t-1}$  is a plausible alternative to  $x_t$ , as Galí and Perotti (2003) also recognize, given the inertia and complexity of the decision-making process. Moreover, the values of output gaps are highly persistent, so that the two choices lead to similar results, as shown in Forni and Momigliano (2004).<sup>6</sup> We have also estimated our base model with  $x_t$  instead of  $x_{t-1}$  (Appendix 2) without significant differences in the results. The two parameters  $\phi^p_{e1}$  and  $\phi^p_{e2}$  measure the effects of regular general elections, provided that the output gap is positive, in the year in which they are held and in the previous year, respectively. If the sign of these parameters is negative, it implies that, *ceteris paribus*, the fiscal stance loosens in the presence of elections. In the tests performed on the general unrestricted model, of all the parameters only the reactions to elections is found to be statistically different depending on the output gap being positive or negative. As the value of the election parameters in case of adverse economic conditions is not significantly different from zero (Tables 3 and A1.2), we exclude the corresponding regressors from our base model. In Section 7 we explore some alternative specifications for the electoral variables which take into account the month, or the quarter, in which elections are held.

As for the distinction between the countries having and not having an excessive deficit, the tests performed on the general unrestricted model indicate a significant difference in all the relevant policy parameters (Table A1.2). If the Maastricht variable differs from zero (*i.e.* if it is necessary to correct the primary balance in order to eliminate the excessive overall deficit), all the other explanatory variables in our model are not statistically significant and can be excluded from the model without loss of relevant information.<sup>7</sup> Therefore, if  $m_{it}$  differs from zero our base specification of the fiscal rule is:

$$[1b] \quad \Delta capb_{it} = \phi_m m_{it-1} + \varepsilon_{it}$$

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<sup>5</sup> A full proof of this claim, obviously, would require formally testing for poolability with respect to individual countries. This is not possible, as the number of observations is too limited.

<sup>6</sup> We prefer using  $x_{t-1}$  instead of  $x_t$  largely for statistical reasons. First, the latter requires the recourse to instrumental variables, as the output gap is affected by fiscal policy, which opens up to a number of equally acceptable alternatives, with a potential indeterminacy on the results. Second, our estimates of the output gap in real time are less subject to a possible end-point bias in the case of  $x_{t-1}$  rather than in the case of  $x_t$  (see Section 3).

<sup>7</sup> These results confirm and extend those of van den Noord (2002), who finds that the euro-area countries that needed to consolidate their public finances tended to neglect the stabilization function.

A value of  $-1$  for  $\phi_m$  would suggest that policymakers strictly followed the proportional correction formula shown in the Box.

Throughout the paper we usually report results for both our base model and a specification in which equation [1a] is applied to all the observations (hereinafter, Eq. [1a] model). In our view, the base model has the advantage of avoiding possible misspecification problems, as the data indicate that countries with an excessive deficit significantly modified their policies. On the other hand, the Eq. [1a] model does not have the shortcoming of including a somewhat *ad hoc* regressor, such as the Maastricht variable. In all cases, the two models give the same indications.

### 3. The data

The full sample covers 19 OECD countries, including 11 countries of the euro area (only Luxembourg is excluded for lack of data), 3 other European countries (the United Kingdom, Sweden and Denmark) and 5 non-European countries (the United States, Japan, Canada, Australia and New Zealand).<sup>8</sup> All the economic variables are from OECD publications (except in some of the exercises which test for robustness). The data set on elections (reported in Table 6) is constructed using the data base of the International Institute for Democracy and Electoral Assistance (IDEA) and the information available in [www.electionguide.org](http://www.electionguide.org), integrated and checked with Routledge (2005).

Our dependent variable ( $\Delta capb$ ) is, for each country, the currently available estimate published by the OECD (from the OECD December 2005 Economic Outlook, hereinafter *EO*). We use the latest vintage of data because they represent, by definition, the most precise assessment of the discretionary fiscal actions taken by governments. For robustness, we also use the latest available estimates of the International Monetary Fund, from the March 2006 WEO, and of the European Commission, from the Autumn 2005 Forecast (see Table 1, Section 4).

As for the explanatory variables, we use real-time estimates to compute the Maastricht variable, for the primary balance (or, in alternative specifications, for the overall balance, see Section 5) and for the cyclical conditions. We do so because all these variables are subject to

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<sup>8</sup> The current information on our dependent variable (Annex Table 30 of the December 2005 EO) refers to 24 OECD countries. However, 5 countries were included only very recently.

large revisions over time. We use the latest available information on the general government debt, as the OECD did not publish comparable data on the debt until recently. The use of *ex post* data for the debt should not lead to significant distortions, as over the last years the revisions to the initial estimates have been a small fraction of the debt level and it is likely that this holds true also for the years for which we do not have this information.

Budget documents are, in principle, the most direct source of the real-time information available to policy-makers, but they often do not report the data we need and, more generally, the estimates included may be distorted for political reasons (connected with the possibility of “announcement effects”) or not comparable, reflecting differences in risk aversion (see, for a discussion on these aspects, Forni and Momigliano, 2004). For this reason, we rely for all countries on the estimates included in the December EOs published by the OECD.

In the countries that we examine, the budget for year  $t+1$  is usually finalised at the end of year  $t$ . Therefore, the December *EOs* are based on an information set which is temporally aligned to that available to national policymakers when taking budgetary decisions for the following year. Considering also that OECD estimates and forecasts for fiscal variables and for GDP are extensively discussed with national experts, it seems reasonable to assume they should be close to those on which budgetary decisions are based.<sup>9</sup>

From the various issues of December EOs, starting from 1989, we directly use the real-time estimates of the general government primary and overall balance and interest payments. For the years 1987 and 1988, for which real-time budgetary data are not available, we rely on the information available in 1989. The use of the 1989-information set for budgetary data should not lead to significant distortions, as it is temporally close to the real-time information set and, in our knowledge, large revisions of the initial estimates have been registered only in more recent years.

As for cyclical conditions, the OECD started to publish estimates of the output gap only in the *EO* of December 1995.<sup>10</sup> To overcome this limitation we compute the output gaps

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<sup>9</sup> The EOs are made available to the general public at the beginning of December, but a preliminary version of the Report is discussed with national delegates (usually from the Finance Ministries) between the end of October and the beginning of November.

<sup>10</sup> In Forni and Momigliano (2004), the estimates of the output gaps implicit in the 1993 and 1994 EOs are approximately computed on the basis of the estimates of the cyclical component of the budget.

on the basis of the series of GDP growth, published in the December EO since 1987. Therefore, we can calculate implicitly-available estimates for output gaps for the years 1987-2005, which bear on policy actions for the years 1988-2006. To compute the gap we employ the filter proposed in Mohr (2005). The filter, which represents an extension of the widely used Hodrick-Prescott filter, avoids the bias in end-of-sample estimates which characterises the latter. This is very important, as we need to estimate the cyclical component of year  $t$  with a series ending in  $t+2$ .<sup>11</sup> In Appendix 2 we present results based on the more traditional Hodrick-Prescott filter.

We consider among explanatory variables the regular national elections (*i.e.* those held at the end of a full term)<sup>12</sup> as they could be expected by policymakers when budgeting, both for the year in which they were held and for the previous year. We consider only parliamentary elections, the only exception being the U.S., where we regard the presidential elections as more relevant. In Section 5, for comparability with other studies where all elections were considered (e.g. Mink and De Haan, 2005), we present the results of a model which includes an additional regressor for early elections.<sup>13</sup>

Our analysis covers three distinctly different periods: (i) the years 1988-92, preceding the Maastricht Treaty (which was signed in February 1992 and went into force in 1993); (ii) the years 1993-97, when participation in the Monetary Union required achieving, in 1997, a deficit below the 3% of GDP; and (iii) the years 1998-2006, during which fiscal policies have been conducted within the framework established by the Stability and Growth Pact (signed in 1997).

In terms of cyclical developments, we are able to fully encompass at least two full business cycles. The period includes, in particular, two almost generalised downturns: at the beginning of the nineties and at the turn of the century. The sample is almost evenly split

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<sup>11</sup> To avoid the end-of-sample bias of the Hodrick-Prescott (HP) filter, series are usually extended further, at least to  $t+4$ . Using the filter proposed by Mohr (2005) in place of the HP filter, we achieve the same objective without introducing an element of arbitrariness in our procedure. For a number of years and countries, the OECD publishes, in addition to the growth in year  $t+2$ , an estimate for the growth in its last semester or quarter. In these cases, we use the latter estimates as proxies for expected growth in  $t+3$ .

<sup>12</sup> We consider an election being “regular” if it takes place in the year in which the term ends or if the anticipation with respect to the end-of-term date does not exceed 6 months.

<sup>13</sup> While it is true that in many cases these early elections could not have been expected when budgeting for the year in which they were held, they could be regarded as a lagged proxy of the political difficulties that led to them.

between positive and negative output gaps. For the euro area we have, respectively, 101 and 108 observations (69 and 83, respectively, for the other 8 countries).

Our GDP-growth-based estimates of output gaps are generally close to those published by the OECD, for the years for which this comparison is possible (including the years 1993-94 for which indirect estimates of the OECD data are available). The standard deviation of the two sets of data, for the euro area, is similar: 1.4 and 1.8 respectively; their coefficient of correlation is 0.7. There is a slight difference in the average value, equal to  $-0.4$  in our estimates and to  $-1.0$  in those of the OECD. The number of positive and negative gaps is more balanced in our estimates. In Table A2 of Appendix 2 we compare our estimates for the period 1994-2006 with those obtained using, in our base model, the estimates of output gaps published by the OECD. The results are qualitatively similar.

#### 4. Main results

In this section we discuss the main results of our model for the euro area and the indications gathered from some exercises meant to test robustness (additional exercises are presented in Appendix 2). We also examine how the same model fares if applied to the 8 countries outside the area included in our sample.

Our base model (column “BASE” of Table 1), applied to the euro-area countries, explains approximately 38 per cent of the variability of budgetary actions between countries and over time. The model satisfies the standard misspecification tests (see Table A1.1 in Appendix 1); furthermore, the Chow test for parameter constancy over the three sub-periods 1988-92, 1993-97 and 1998-2006 does not identify any structural breaks (with a  $p$ -value of 57.0%). All the estimated parameters have the expected sign. They are also highly significant, except those capturing the 1-year-before effect of elections. However, the two election parameters are jointly significant, with a  $p$ -value of 0.02%.

The estimates of the coefficients of the primary balance and the debt indicate that fiscal policies react to the initial state of public finances in a stabilizing manner. Given the absence of individual fixed effects, fiscal policies aim in the long run at reducing to zero the level of both variables and, implicitly, of the overall balance. As for the reaction to the primary balance, the coefficient ( $-0.19$ ) indicates that, *ceteris paribus*, one fifth of the



Table 1

**MAIN RESULTS AND ROBUSTNESS<sup>(1)</sup>**

Models Parameters	11 countries of the euro area					Other samples	
	BASE	Eq. [1a] <sup>(2)</sup>	BASE-sy <sup>(3)</sup>	BASE-IMF	BASE-EC	8 OECD	19 OECD
$\phi_{pb}$	-0.192	-0.222	-0.188	-0.169	-0.172	-0.141	-0.173
	<i>-4.39</i>	<i>-5.72</i>	<i>-4.21</i>	<i>-3.65</i>	<i>-3.83</i>	<i>-3.63</i>	<i>-6.53</i>
$\phi_d$	0.011	0.012	0.011	0.008	0.007	0.005	0.008
	<i>3.03</i>	<i>3.93</i>	<i>3.00</i>	<i>2.19</i>	<i>1.91</i>	<i>1.26</i>	<i>3.00</i>
$\phi_m$	-0.619		-0.619	-0.621	-0.543		-0.619
	<i>-6.09</i>		<i>-5.99</i>	<i>-5.79</i>	<i>-5.16</i>		<i>-6.06</i>
$\phi_x$	0.427	0.302	0.345	0.320	0.426	0.086	0.309
	<i>3.82</i>	<i>3.43</i>	<i>3.13</i>	<i>2.72</i>	<i>3.75</i>	<i>0.94</i>	<i>4.72</i>
$\phi^{(p)}_{e1}$	-1.366	-1.283		-1.349	-1.419	-0.311	-0.797
	<i>-4.16</i>	<i>-3.96</i>		<i>-3.89</i>	<i>-4.23</i>	<i>-0.87</i>	<i>-3.32</i>
$\phi^{(p)}_{e2}$	-0.551	-0.482		-0.444	-0.606	-0.274	-0.321
	<i>-1.77</i>	<i>-1.59</i>		<i>-1.36</i>	<i>-1.91</i>	<i>-0.75</i>	<i>-1.38</i>
$\phi_{e1}$			-0.953				
			<i>-3.37</i>				
$\phi_{e2}$			-0.342				
			<i>-1.39</i>				
N. of obs. <sup>(4)</sup>	209	209	209	209	196	152	361
RMSE <sup>(5)</sup>	1.118	1.141	1.137	1.179	1.128	1.087	1.124
$R^2$	0.381	0.352	0.360	0.345	0.358	0.396	0.326
$R^2$ adjusted	0.297	0.268	0.273	0.256	0.265	0.277	0.265

<sup>(1)</sup>  $T$ -statistics are reported below the estimates. The estimates of 19 time-dummies are not reported.

<sup>(2)</sup> Eq. [1a] is applied to all observations.

<sup>(3)</sup> Base model but election parameters independent of the sign of the output gap ( $p$ -value of the restrictions = 5.1%).

<sup>(4)</sup> 13 observations are missing in the data from the European Commission (EC).

<sup>(5)</sup> Root Mean Squared Error.

imbalance is corrected in the following year. The reaction to the debt is equal to 1% of the outstanding stock.

For a cost of the debt (5.5%) close to the average value in our sample (5.1%) the estimate of the parameter for the debt implies a reaction to interest payments equal to that estimated for the primary balance. This suggests the need for explicitly comparing this specification with a more parsimonious one, including only the overall balance. This analysis is conducted in Section 5.

The Maastricht variable estimate ( $-0.62$ ) would suggest that Governments have chosen a more back-loaded strategy than our proportional benchmark, though the result may also be partly due to approximations in our formula.<sup>14</sup>

The coefficient for the output gap is positive, pointing to a counter-cyclical reaction of fiscal policy to economic conditions, as assessed at the time budgetary decisions were taken. The reaction is sizeable, as the estimated coefficient implies that a 1 per cent negative output gap induces, *ceteris paribus*, a discretionary expansion amounting to 0.43 per cent of GDP.

Finally, we find a large impact of regular elections, conditional on cyclical conditions being assessed as being favourable when budgetary decisions are taken: they induce a loosening of the fiscal stance equal to 1.4 per cent of GDP in the year in which they are held and of 0.6 per cent in the year before (the latter estimate is only 10% significant).

In columns 2-4 of Table 1 we check the robustness of our estimates to, respectively, (i) the exclusion of the Maastricht variable (“Eq. [1a]” column), (ii) the imposition that the effects of elections be constant across good and bad times (“BASE-sy” column) and (iii) the use of alternative estimates of the dependent variable.

The exclusion of the Maastricht variable, *i.e.* allowing Eq. [1a] to be applied to all observations, induces a slight worsening in the explanatory power of our model, but leaves the estimates of the other parameters and their levels of significance largely unaffected. There is only a slight reduction in the point estimate of the reaction to the output gap and a slight increase in those to the initial state of the public finances. Analogously to what we found for the base model, the Chow test for parameter constancy over the three sub-periods specified above does not identify any structural breaks (with a p-value of 44.6%).

Assuming that the effects of elections are constant across good and bad times alike has negligible effects on the values of the other parameters (in particular, it does not significantly modify the estimate of the coefficient for the output gap) but, obviously, lowers the estimated impact of elections.

Finally, the results do not change significantly if the latest available OECD estimates of our dependent variable are substituted with those of the International Monetary Fund (from the March 2006 WEO, “IMF” column) and the European Commission (Autumn 2005 Forecast, “EC” column).

In column 5 (“8 OECD”) of Table 1 we follow the same estimation procedure outlined in Section 2 – *i.e.*, from a general model to a restricted one - to assess the determinants of the fiscal policies of the 8 countries of our sample outside the euro area.<sup>15</sup> The estimates of the restricted model (which includes an individual effect for Japan) suggest the absence of systematic reactions to cyclical conditions and of an electoral budget cycle. The responses to the initial state of public finances are slightly smaller and, especially in the case of the debt, less precisely estimated.<sup>16</sup>

Finally, in column 6 (“19 OECD”) we assess the determinants of the fiscal policies of our full sample of 19 OECD countries, following once more the procedure outlined in Section 2. The results, based on a model which includes individual effects for the 5 non-European countries of the sample, are broadly in line with those for the euro area, masking the substantial heterogeneity of the two groups of countries (as shown by the comparison between columns 2 and 6). Clearly, the good performance of the model for the sample of 19 OECD countries is explained exclusively by the information included in the euro-area data. This result shows the potential risks of pooling groups of countries with different characteristics without checking for parameter constancy between them.

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<sup>14</sup> For simplicity, we do not take into account the expected contribution of the cycle in the following years. Moreover, when defining the Maastricht variable we assume that policymakers expect that the contribution from interest payments in year  $t+1$  to the overall correction remains unchanged in the following years.

<sup>15</sup> We cannot reject the hypothesis of parameters poolability of the 3 non-EMU European countries with the 5 non-European OECD countries (the  $p$ -value of the relevant test is equal to 41.2%). The 3 countries are considerably less poolable with the 11 euro area countries (the  $p$ -value of the test is 8.9%).

<sup>16</sup> The difficulty of applying our fiscal rule to the 3 non-euro-area countries may be due to the fact that for two of them the budget is influenced by revenues from oil production.

## 5. The reactions to the state of public finances and the role of Maastricht

As shown in the previous section, our estimates indicate that fiscal policy reacts in a stabilizing manner to the levels of the primary balance and of the debt. These results are robust to the changes examined in Table 1 (Section 4). Moreover, if we allow for different values of  $\phi_{pb}$  and  $\phi_d$ , depending on whether cyclical conditions are favourable or adverse, the two sets of parameters do not significantly differ (see Table 3 in Section 6).

In Table 2, we split our sample period in the three sub-periods 1988-92, 1993-97 and 1998-2006, presenting for robustness the estimates both for the base model and for the model in which Eq. [1a] is applied to all observations.

**Table 2**

**Estimation Results over Sub-periods<sup>(1)</sup>**

PARAMETER	BASE MODEL					Eq. [1a] MODEL <sup>(2)</sup>			
	1988-2006 <sup>(3)</sup>	1988-92	1993-97	Obs. <sup>(5)</sup>	1998-06	1988-2006 <sup>(3)</sup>	1993-97	Obs. <sup>(4)</sup>	1998-06
$\phi_{pb}$	-0.192	-0.165	-0.756	(13)	-0.208	-0.222	-0.330	(55)	-0.239
	-4.39	-2.41	-2.73		-2.82	-5.72	-3.55		-3.86
$\phi_d$	0.011	0.011	0.078	(13)	0.009	0.012	0.024	(55)	0.010
	3.03	1.93	3.01		1.64	3.93	3.49		2.15
$\phi_m$	-0.619		-0.603	(42)	-0.821				
	-6.09		-6.14		-2.45				
$\phi_x$	0.427	0.378	0.098	(13)	0.445	0.302	0.109	(55)	0.452
	3.82	1.66	0.22		3.21	3.43	0.72		3.58
$\phi^{(p)}_{e1}$	-1.366	-1.790	1.834	(1)	-1.290	-1.283	0.183	(1)	-1.265
	-4.16	-2.59	1.27 <sup>5</sup>		-3.33	-3.96	0.15		-3.41
$\phi^{(p)}_{e2}$	-0.551	-0.196	-0.069	(3)	-0.822	-0.482	-0.046	(3)	-0.830
	-1.77	-0.34	-0.05		-2.03	-1.59	-0.06		-2.06
No. of obs.	209	55	55		99	209	55		99
RMSE <sup>(5)</sup>	1.118	1.360	1.038		1.011	1.141	1.123		1.009
$R^2$	0.381	0.345	0.545		0.388	0.352	0.455		0.383
$R^2$ adjusted	0.297	0.200	0.432		0.279	0.268	0.334		0.281

<sup>(1)</sup>  $T$ -statistics are reported below the estimates. The estimates for the time-dummies are not reported.

<sup>(2)</sup> Eq. [1a] is estimated over the whole euro-area countries sample. For the 1988-92 column see the corresponding BASE column.

<sup>(3)</sup> The  $p$ -values of test for parameters constancy over time (Chow test) are, respectively, 45.7% for the base model, and 35.9% for Eq. [1a] model.

<sup>(4)</sup> Number of non-zero observations for the corresponding regressor.

<sup>(5)</sup> Root Mean Squared Error.

Focusing on the reactions to the primary balance and the debt, the estimates tend to remain, even in the sub-periods, significant. The point estimates of the initial and last sub-periods, both for variables and models, are also relatively close. A larger stabilizing reaction to the state of public finances can be detected, for the period 1993-97, both in the case of the base model (where, however, the estimates of these parameters are based on thirteen observations only) and in the specification Eq. [1a]. The larger reaction to imbalances, and the simultaneous loss of significance for the effects of cyclical conditions and elections, is consistent with the political climate of that period, particularly favourable to the pursuit of sustainable public finances.

It is still highly controversial whether the Maastricht Treaty simply reaffirmed pre-existing preferences or, instead, it created its own political dynamics inducing governments to undertake consolidations they would not have effected otherwise. Von Hagen *et al.* (2002), on the basis of the comparison of the estimates of a fiscal rule for the year 1972-89 and for the years 1990-98, argue that the Treaty had an impact on fiscal policies as they find a positive shift in the intercept term between the two periods in the direction of surpluses. Our results, though not strictly comparable (we examine only five years of policies preceding Maastricht and, on the other hand, we include eight years beyond 1998), are less univocal, but tend to support the opposite view.

In favour of a “Maastricht effect” there is the strengthening of the stabilizing reaction to imbalances (both in terms of primary balance and of debt) in the 1993-97 period, compared to the previous period. However, the tightening is only temporary and there is no clear evidence of a structural break.<sup>17</sup> We also find that the behaviour of the countries in excessive deficit throughout the period 1993-2006 is more accurately captured by a specifically constructed regressor (the Maastricht variable), defined on the basis of the European rules. However, the exclusion of the Maastricht variable leaves the explanatory power of the model and the estimates of the reactions to cyclical conditions broadly unchanged, as indicated by the results of the Eq. [1a] model. Overall, we conclude that the European fiscal rules only reaffirmed preferences that can already be detected in the years

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<sup>17</sup> As mentioned in Section 4, the tests for parameter constancy do not identify any structural breaks over the three different sub-periods, for both the base and the Eq. [1a] models.

immediately preceding the Treaty of Maastricht. It is possible, however, that those preferences would have not remained stable in the absence of the Treaty.

The use of the primary balance and the debt to account for the initial conditions of public finances is relatively standard in the literature, but it is also plausible that fiscal policy would react, instead, to the overall balance. To assess this alternative fiscal rule, we have estimated a model substituting the primary balance and the debt with the overall balance, once more following the procedure from general to specific outlined in Section 2. The estimate obtained for the parameter of the overall balance (0.40) is not significantly different from that of the base model for the primary balance (0.43), while that for all the other parameters is virtually identical.<sup>18</sup> However, the explanatory power of the model with the overall balance is slightly worse than that of our base model, suggesting a greater role for both primary deficit and debt in influencing policy decisions. Formal tests point in the same direction.<sup>19</sup> Moreover, focusing on the debt (as in the base model) instead of its cost (implied by a fiscal rule based on the overall balance) is, in principle, a better rule, as it avoids unnecessary reactions to temporary fluctuations in the level of interest rates. This implies, for example, that the role currently assigned to the debt is proportionally larger than its actual cost, as in recent years interest rates have been particularly low.

We also tried to further understand the impact of the 3% rule on the behaviour of fiscal policies in countries violating the threshold. In order to do so, we added the Maastricht variable to the Eq. [1a] model. In this context, the estimate of the parameter of the Maastricht variable falls to 0.5 but it remains highly significant (with a  $t$ -statistics of 3.2), suggesting that this variable contributes to better understanding the behaviour of countries in excessive deficit. Finally, we explored the possibility that the policies of the countries in excessive deficit changed between the period 1993-97 ( $\phi_m$  1993-97) and the following years ( $\phi_m > 1997$ ), in view of the widespread idea that after 1998 the impact of the fiscal rules

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<sup>18</sup> This result is not surprising, as we found that the parameter for the debt, on the base of the average cost of the debt in the sample, was broadly consistent with a value of a parameter on interest payments equal to that estimated for the primary balance (see Section 4).

<sup>19</sup> In a model which includes all three variables, the  $p$ -value of the null hypothesis that the overall balance has no additional impact on the dependent variable is 94.8%, while the  $p$ -value of the null hypothesis that the primary balance and the debt have no additional impact is only 11.9%.

weakened significantly. The point estimates, as well as more formal tests, do not suggest any differences in behaviour.<sup>20</sup>

## 6. The reactions to cyclical conditions

As seen in Table 1 of Section 4, controlling for other factors we find a sizable stabilizing reaction of fiscal policies of the euro-area countries to cyclical conditions, as assessed at the time budgetary decisions were taken. A 1 per cent negative output gap in year  $t$  induces a budgetary loosening in year  $t+1$  amounting to 0.4 per cent of GDP. We get a similar reaction if, in our model, we substitute the estimate of the output gap of year  $t$  with that of year  $t+1$  (see Table A2).

These results are in line with those of Forni and Momigliano (2004) and, partly, with those of Buti and van den Noord (2004),<sup>21</sup> while differ from the findings of various studies that, on the base of *ex post* data and generally referring to periods starting in the early seventies and ending in the late nineties, indicate that discretionary policies (in the euro area or in the EU) have been either a-cyclical or pro-cyclical (e.g. Buti and Sapir, 1998; Wyplosz, 1999; European Commission, 2001; Buti, 2002; Brunila and Martinez-Mongay, 2002; Melitz, 2002; Galí and Perotti, 2003 and the studies referred to in European Commission, 2006). The results of these studies have been generally taken as relevant for assessing the *behaviour* of fiscal authorities *facing* cyclical imbalances. However, as it is shown in Section 8, the use of *ex post* data may largely explain these findings, at least for the last two decades.

The sign of the reaction does not change across sub-periods (Table 2). The reaction is less strong in the 1993-97 period, but it is also not precisely estimated. As in Galí and Perotti (2003), we find evidence neither of the pro-cyclical bias that could stem from the Stability Pact being “all sticks and no carrots” (Bean, 1998) nor of the “overall improvement in cyclical stabilization” with respect to the pre-Maastricht era, detected by Buti and Pench (2004).

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<sup>20</sup> If, starting from the base model, we split the Maastricht variable into two regressors, referring to the two sub-periods, their point estimates are, respectively,  $-0.6$  and  $-0.82$ , with  $t$ -statistics equal to 5.7 and 2.2. When we impose the same value to the two parameters, the restriction is not rejected, with a  $p$ -value of 57.2%.

<sup>21</sup> Buti and van den Noord (2004), examining the years 2000-03 and controlling for errors in forecasting, find that fiscal policy is counter-cyclical in the absence of elections.

A number of recent papers have found an asymmetrical reaction of fiscal policy to cyclical conditions, depending on whether the latter are favourable or unfavourable (OECD, 2003; Forni and Momigliano, 2004 and Balassone and Francese, 2004). These analyses have generally been based on models including two parameters (respectively, for the positive and negative output gaps) for the reaction of fiscal policy to cyclical conditions. Here we consider a more general asymmetric behaviour, as we allow all parameters of our model to have two values, depending on whether the output gap is positive or negative.<sup>22</sup>

In Appendix 1 we show that the restrictions imposing symmetry (with respect to the sign of the gap) in country and time fixed-effects are largely not rejected by data and that country effects can be altogether excluded by the model. Therefore, in this section we focus on two intermediate specifications (IM-BASE and IM-Eq. [1a]) which differ from our base and Eq. [1a] models, respectively, only because they allow the values of the other parameters (which measure the reactions to, respectively, the primary balance, the debt, the cyclical conditions and the elections) to be different, depending on the sign of the output gaps.

The parameter estimates of these intermediate models and their level of significance are shown in Table 3. The reaction to cyclical conditions  $\phi_x$  is always stabilizing, independently of the model or of the sign of the gap. In the base model, the size of the reaction is almost identical in the two cyclical contexts (0.39 when gaps are positive and 0.42 when they are negative) and the null hypothesis of symmetry cannot be rejected (with a  $p$ -value of 91.4%). In the Eq. [1a] model, the difference in the point estimates is sizeable but it is also not significant (the  $p$ -value for the hypothesis of symmetry is 34.3%). Furthermore, the counter-cyclical reaction is stronger in good times, while previous studies found the opposite result, indicating that in favourable economic conditions policies tended to be either pro-cyclical or a-cyclical.<sup>23</sup>

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<sup>22</sup> For a non-parametric approach to this issue, see Manasse (2006).

<sup>23</sup> As an additional check on this issue we also examined two alternative approaches. The first involves estimating the base model (and the Eq. [1a] model) over two sub-samples, which include, respectively, only positive and only negative output gaps. Then a Chow test for parameter constancy is performed. The second approach, which is in line with previous analyses, involves a model with two parameters  $\phi_x(p)$  and  $\phi_x(n)$  (respectively, for the positive and negative output gaps) estimated over the full sample and a coefficient-equality test. In both cases, the null hypothesis of symmetry is largely not rejected.



Table 3

**TESTING THE SIMMETRY, WITH RESPECT TO THE SIGN OF THE OUTPUT GAP, OF THE POLICY PARAMETERS**

	IM BASE MODEL			IM Eq. [1a] MODEL		
	estimated parameters <sup>(1)</sup>		symmetry tests <sup>(2)</sup>	estimated parameters <sup>(1)</sup>		symmetry tests <sup>(2)</sup>
	if gap > 0	if gap < 0		if gap > 0	if gap < 0	
$\phi_{pb}$	-0.187 ***	-0.215 **	77.8%	-0.196 ***	-0.261 ***	38.6%
$\phi_d$	0.011 ***	0.009 *	65.7%	0.014 ***	0.013 ***	86.6%
$\phi_x$	0.393 **	0.422 **	91.4%	0.427 **	0.224 *	34.3%
$\phi_{e1}$	-1.404 ***	0.128	1.7%	-1.381 ***	-0.045	0.6%
$\phi_{e2}$	-0.592 *	0.078	19.1%	-0.555 *	-0.089	30.3%
$\phi_{e1}$ and $\phi_{e2}$			4.1%			1.9%
$\phi_m$	-0.619 *			-		
- No. of parameters	30			29		
- No. of observations	209			209		
- RMSE	1.132			1.147		
- $R^2$	0.383			0.362		
- $R^2$ adjusted	0.280			0.260		

(1) The estimates of 19 time-dummies are not reported.

The notations \*, \*\*, and \*\*\* indicate that parameters are, respectively, 10%, 5% and 1% significant.

(2)  $p$ -values of the tests of parameter equality.

The difference in our results with respect to those of Forni and Momigliano (2004), which are directly comparable as they are also based on real-time information on cyclical conditions but point to a significant asymmetry in the reaction of fiscal policy, depends on three factors: (i) the different data used for the estimates of the output gap,<sup>24</sup> (ii) the inclusion among regressors, in our model, of the (asymmetric) effects of elections, and (iii) the use of

<sup>24</sup> This result is not surprising. Orphanides and van Norden (2002) show that different methods to compute the output gap lead to significant differences in the results, especially when cyclical conditions are assessed in real time. Forni and Momigliano (2004) use, for the years 1995-2003, the estimates of output gaps published in the OECD EOs and, for the years 1993 and 1994, the estimates of the output gaps implicit in the EOs, approximately computed on the basis of the estimates of the cyclical component of the budget. In the period 1993-2003 the number of positive output gaps in these estimates is limited (27 observations out of 121), which suggests caution in interpreting empirical inferences.

real time information, again in our fiscal rule, on budget balances. In fact, if we estimate our base model over their sample period (1994-2004), we still tend to largely accept the hypothesis of symmetry in the reactions to cyclical conditions, with a  $p$ -value of the test of 93.4%. If we substitute our real time estimates of the output gap and of the budget balances with those used by the authors and exclude elections from the regressors, symmetry is rejected, with a  $p$ -value of 3.3%.

## 7. The role of the political budget cycle

We find that regular elections (*i.e.*, those held at the end of a full term) have a large impact on fiscal policies, provided that budgetary decisions are taken in a cyclical context assessed as favourable (*i.e.* the output gap is positive). Estimates based on our base model (Table 1, Section 4) indicate that, in this case, regular elections lead to a loosening of the fiscal stance of 1.4 per cent of GDP in the year in which they are held and of 0.6 per cent in the year before. These effects are relatively large, clearly on the high side of the empirical evidence (for a survey of the literature on political budget cycles see Drazen, 2001). The two election parameters are jointly highly significant, with a  $p$ -value of 0.2% for the null hypothesis.

In contrast, regular elections have no significant effects on fiscal policies if the budgetary decisions are taken when the output gap is negative (see Table 3, Section 6). In particular, in the test of joint significance, the  $p$ -value of the hypothesis of no effects exceeds 95% (row IM4, Table A1.2).

Other studies have provided evidence of electoral manipulation of fiscal policy in EU countries (Hallerberg and Strauch, 2002; Buti, 2002; von Hagen, 2002 and Buti and van den Noord, 2003). Evidence that the cyclical context has an impact on the extent of these manipulations for the euro-area can already be found in Buti and van den Noord (2004). This previous evidence, however, refers only to four years (2000-03) and to pre- or early election years. Here we broadly confirm and substantially extend those results, as we find a preminent role of the cyclical context over almost two decades in determining fiscal policies both in pre- and in election years.

The importance of the cyclical conditions has, in our opinion, a plausible explanation, in line with the models of political budget cycles which emphasize temporary information

asymmetries (e.g. Rogoff and Siebert, 1988). In good times, policymakers can provide additional public goods to the electorate while signalling, with a relatively low (unadjusted) deficit, that they are good administrators. This behaviour is not possible in adverse economic conditions, as the automatic stabilizers and the counter-cyclical action already raise the deficit and leave no room for providing additional public goods. If correct, this explanation implies that, at least in the euro-area, improving information on cyclical conditions and on their impact on budget balances would help to reduce electoral manipulations of fiscal policy.

As the euro-area countries are essentially established democracies, our results contrast with those of Brender and Drazen (2004), who find that electoral budget cycles are confined to new democracies.<sup>25</sup>

When we split the sample into sub-periods (Table 2), a general pattern emerges. The estimates of the two parameters for the sub-periods 1988-92 and 1998-2006 are always negative (*i.e.*, the effects are deficit-increasing), relatively stable across periods and, in the case of  $\phi_{e1}^P$ , always highly significant. In the period 1993-97 there are so few elections that the results cannot be considered reliable.

On the issue of measuring the electoral variables, other authors have proposed more complex alternatives to the yearly dummies we use. In Table 4 we compare our results with those obtained with two of these alternatives. As benchmark, for comparability with other studies, we show the estimates of a slight variant of our base model (BASE-early), which also includes a parameter for early elections ( $\phi_{e3}^P$ ).

Franzese (2000) defines an electoral variable equal, in the year  $t$  (that of the election), to the number of the month in which the election is held divided by 12 and, in the year before elections, to its complement to 1. In the column “MONTH” of Table 4 we present the estimates of a specification which, compared to our base model, excludes our regular elections dummies (with the corresponding parameters  $\phi_{e1}^P$  and  $\phi_{e2}^P$ ) and includes the corresponding variables proposed by the author (parameters  $\phi_{e1}^P$  with *month* and  $\phi_{e2}^P$  with *month*). As with our model, we set to zero the variable if budgetary decisions are taken in

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<sup>25</sup> In our sample only the elections in 1989 in Spain refer, in Brender and Drazen terminology, to “new democracies”. Excluding that episode (an early election), does not significantly modify our results.

Table 4

**RESULTS OF ALTERNATIVE SPECIFICATIONS  
OF THE ELECTORAL VARIABLES<sup>(1)</sup>**

PARAMETER	BASE-early	MONTH	QUARTER
$\phi_{pb}$	-0.190 -4.33	-0.191 -4.35	-0.218 -4.84
$\phi_d$	0.010 2.95	0.011 3.01	0.011 3.10
$\phi_m$	-0.619 -6.09	-0.619 -6.08	-0.619 -6.14
$\phi_x$	0.422 3.78	0.392 3.55	0.428 3.79
$\phi^{(p)}_{e1}$	-1.413 -4.27		
$\phi^{(p)}_{e2}$	-0.594 -1.90		-0.577 -1.86
$\phi^{(p)}_{e1 \text{ month}}$		-2.536 -4.05	
$\phi^{(p)}_{e2 \text{ month}}$		-0.954 -1.98	
$\phi^{(p)}_{e1 \text{ for } Q1}$			-1.464 -2.67
$\phi^{(p)}_{e1 \text{ for } Q2}$			-0.963 -2.00
$\phi^{(p)}_{e1 \text{ for } Q3}$			-4.142 -3.43
$\phi^{(p)}_{e1 \text{ for } Q4}$			-1.219 -1.73
$\phi^{(p)}_{e3}$	-0.423 -1.18	-0.402 -1.12	-0.455 -1.28
No. of observations	209	209	209
Root Mean Squared Error	1.117	1.120	1.108
$R^2$	0.385	0.382	0.405
$R^2$ adjusted	0.298	0.294	0.310

<sup>(1)</sup>  $T$ -statistics are reported below the estimates. The estimates for the time-dummies are not reported.

bad times (the results of the comparison are not modified if we allow the effects of elections to be symmetric in all the models). The estimates for the parameters of the latter variables (for both sides of the table) are in line with our results, taking into account that the mean and the median of the ratio between the election month and 12 in our sample is slightly above 0.5, but do not seem to add relevant information. More formally, there is no evidence of any of the two models being superior to the other, as both are valid reductions from a general model in which they are nested.<sup>26</sup>

Mink and de Haan (2005) split the electoral variable for the year  $t$  into four variables dependent on the quarter in which the election is held, to capture a non-monotonic relationship. In the column “QUARTER” we present the parameter estimates using this specification.<sup>27</sup> The evidence of statistical differences between their values and that of the yearly parameter  $\phi^{(p)}_{et}$  is mixed, as the  $p$ -value of the joint hypothesis of no differences is 11.4%. Examining individual quarters, only the effects of elections held in the third quarter are significantly different (larger) than those in the other quarters. This time pattern is broadly consistent with that found by Mink and de Haan (2005), who detect a peak in the effect for the elections held in the middle of the year.

Overall, we tend to conclude that, in the euro-area context, there is not any mechanical correlation between the magnitude of the budgetary effects and the month in which the election is held, but there is some evidence that elections held in the third quarter do exert a larger expansionary impact on the deficit than those carried out in the other quarters.<sup>28</sup>

Finally, the choice of electoral variables does not affect the estimates of the other parameters.

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<sup>26</sup> In a model which includes all four variables, the  $p$ -value of the null hypothesis that our two dummies have no additional impact on the dependent variable is 56.2%, while the  $p$ -value of the null hypothesis that Franzese variable has no additional impact is 58.2%.

<sup>27</sup> Here, to facilitate the interpretation of the values of the coefficients and comparability with our results, we present estimates where the yearly dummy is split into four quarterly parameters, while Mink and de Haan (2005) start, in fact, from Franzese electoral variable. Results for the model based on their original specification for the quarterly variables are close to those presented here. In particular, the overall explanatory power is similar and the estimates of the four parameters are, approximately, proportional to the product of those shown in column “QUARTER” and the ratio between 12 and the middle month of each quarter. The  $t$ -statistics of the parameter of the electoral variable for the year before the elections increases slightly and becomes 5% significant.

<sup>28</sup> This result seems to require a different explanation from that proposed by Mink and de Haan (2005), based on information lags concerning the public sector borrowing, as that is inconsistent with the presence of a not-irrelevant impact of elections on fiscal policies in the year before the one in which they are held.

## 8. The effects on estimates of using *ex post* data

As mentioned in the Introduction, most empirical estimates of fiscal rules have used *ex post* (latest available) data. If the estimated parameters are interpreted as identifying the behaviour of policy-makers, the use of data which could not possibly have been used by the latter entails the risk of a biased assessment.

It should be noted that even if only one explanatory variable is measured with error (depending on the use of *ex post* – revised – data in models where real-time information matters), *all* parameter estimates are biased. If there are more variables measured with error (as in our case, where all explanatory variables would have to be measured on the basis of real-time data) the expressions of the biases get very complicated. The direction of the bias on the coefficients is determined by: (i) the model parameters, (ii) the correlations between the variables (measured without error, *i.e.* real-time) and (iii) the ratios of the revisions' variances to the respective variances of the true (*i.e.* real time) variables, see e.g. Levi (1973).

The risks of biased estimates could be limited if the revisions were small. However, it is well known that the initial assessment of the cyclical conditions is subject to large revisions over time. This is, in part, due to the error in assessing growth in the current year but, more importantly, depends on the fact that the estimate of the output gap for a given year is crucially tied to the growth of GDP in the following periods, which is usually forecasted with large errors. In the case of fiscal data, the initial assessment for some countries has been also significantly modified in recent years, as the application of some methodological criteria has been clarified by Eurostat and/or corrected by National Statistical Institutions and new information has become available.<sup>29</sup>

In order to quantitatively assess the extent to which the use of *ex post* data can modify the estimates, at least in our sample, Table 5 shows the comparison between our results and those obtained using the latest available information (from the OECD December 2005 Economic Outlook). Overall, it indicates that the type of information set used has a large impact on estimates.

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<sup>29</sup> From 1998 to 2005, public deficits of the euro-area countries were above the Maastricht Treaty limit (3% of GDP) 12 times with real-time data and 24 times on the basis of the latest available information.

The first column of the table shows the OLS estimates based on real-time data for a slight variant of our base model, in which we allow elections to exert different effects in the positive and in the negative cyclical phases.<sup>30</sup> In addition, since *ex post* data embody revisions that may not average to zero within countries over time, we start with a model with both country and time effects. As in our base model, individual effects are not significant (and are therefore excluded by the model on which the reported estimates are based), while time effects are, and election effects are significant only during the positive cyclical phases. Point estimates and significance levels of all other parameters are almost identical to those of the base model.

In the second column we report the OLS estimates of the same model but using *ex post* data for the output gaps. Results are generally broadly similar to those of the first column but without the asymmetry in the effects of elections dependent on the sign of the output gap. Furthermore, the counter-cyclical reaction is significantly smaller and is only 10% significant. Finally, the overall explanatory power of the model is reduced.

In the third and fourth columns of Table 5 we report OLS and GMM estimates of the same model using *ex post* data not only for the output gaps but also for the budget balances. We also use GMM (following the proposal of Arellano and Bond, 1991) as, in this case, country effects are statistically significant and, therefore, they are included in the specification on which the reported estimates are based. Not being significant, time effects were excluded. While the results in the third and fourth column are broadly similar (except for the parameter of the Maastricht variable), almost all estimates are significantly different from those based on real-time data and the explanatory power of the models drops further.

## 9. Conclusions

This paper examines the impact of four major factors on the fiscal policies of the euro-area countries over the last two decades. We rely on information actually available to policy-makers at the time of budgeting in constructing our explanatory variables. A parsimonious model, which does not include fixed effects for individual countries, is able to

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<sup>30</sup> Given that measuring the output gap with *ex post* data may alter the identification of positive and negative cyclical phases, it is not granted *a priori* that elections play an asymmetric role with *ex post* data too. For this reason, we prefer to start from a more general framework.

Table 5

**COMPARING RESULTS WITH REAL-TIME AND EX POST DATA<sup>(1) (2)</sup>**

	Real-time data	<i>Ex post</i> data for cyclical conditions	<i>Ex post</i> data for cyclical conditions and primary balance	
	<i>OLS</i> <sup>(3)</sup>	<i>OLS</i> <sup>(3)</sup>	<i>OLS</i> <sup>(4)</sup>	<i>GMM</i> <sup>(4), (5)</sup>
$\phi_{bb}$	-0.193 -4.36	-0.181 -3.98	-0.348 -6.81	-0.318 -7.62
$\phi_d$	0.011 3.02	0.011 2.96	0.023 3.55	0.012 3.92
$\phi_m$	-0.619 -6.05	-0.619 -5.89	-0.634 -5.55	-1.154 -6.61
$\phi_x$	0.426 3.69	0.197 1.83	0.098 1.41	0.114 1.87
$\phi^{(p)}_{e1}$	-1.367 -4.13	-0.976 -2.62	-0.563 -1.56	-0.615 -1.80
$\phi^{(n)}_{e1}$	0.030 0.06	-0.790 -1.84	-0.289 -0.73	-0.408 -1.21
$\phi^{(p)}_{e2}$	-0.551 -1.76	-0.181 -0.57	-0.125 -0.36	-0.253 -0.78
$\phi^{(n)}_{e1}$	-0.024 -0.07	-0.651 -1.62	-0.190 -0.52	-0.195 -0.58
<i>Joint significance (p-values)</i>				
Individual effects	41.4%	43.4%	0.7%	-
Time effects	2.2%	6.2%	76.5%	26.6%
Elections effects	0.2%	2.3%	56.0%	30.2%
<i>Main diagnostics:</i>				
N. of observations	209	209	209	209
RMSE <sup>(6)</sup>	1.124	1.156	1.150	1.215
$R^2$	0.381	0.345	0.323 <sup>(7)</sup>	0.241
$R^2$ adjusted	0.289	0.248	0.256 <sup>(7)</sup>	0.214
Hansen $J^{(8)}$	-	-	-	15.1%
Autocorrelation <sup>(9)</sup>	12.4%	7.9%	96.2%	62.3%

(1) *T*-statistics are reported below the estimates. The estimates of the individual and time-dummies are not reported.

(2) Base model plus generalised (*i.e.* positive and negative) election parameters. Deterministic components are estimated when significant (see notes below).

(3) As a result of parameter tests, the model allows for time effects and does not include country fixed effects.

(4) As a result of parameter tests, the model allows for country fixed effects and does not include time effects.

(5) Arellano and Bond (1991) GMM-diff estimator.

(6) Root Mean Squared Error.

(7) Based on the squared coefficient of correlation between actual and fitted values.

(8) Hansen (1982) overidentification test.

(9) First-order residual autocorrelation for OLS, second order for GMM.



explain almost 40 per cent of the variability of budgetary actions between countries and over time. The tests for parameter constancy over the three sub-periods 1988-92, 1993-97, 1998-2006 do not identify any structural breaks. Our estimates indicate that:

- Fiscal policies reacted to the levels of the primary balance and of the debt in a stabilizing manner: the coefficient of the primary balance indicates that, *ceteris paribus*, one fifth of the imbalance is corrected in the following year, while the reaction to the debt is equal to 1 per cent of the outstanding stock.
  - European fiscal rules play a somewhat limited role in our model. The point estimates of the reactions to primary balance and debt levels are higher for the sub-period 1993-97, but this increase is not statistically significant and it is temporary: the estimates for the following period (1998-2006) are in line with those for the pre-Maastricht years (1988-92). We also find that the behaviour of the countries with excessive deficits is more accurately captured throughout the period 1993-2006 by a specifically constructed regressor, defined on the basis of the European rules. However, the exclusion of this variable leaves the overall explanatory power of the model and the parameter estimates broadly unchanged. Overall, we conclude that the European fiscal rules only reaffirmed preferences that can already be detected in the years immediately preceding the Treaty of Maastricht. It is possible, however, that those preferences would have not remained stable without the Treaty.
- The reaction of the fiscal authorities to cyclical conditions has generally been stabilizing and not negligible: a 1 per cent negative output gap leads to a budgetary loosening of 0.4 per cent of GDP. This result differs from the findings of most empirical analyses, which find on the basis of *ex post* data and referring to periods including earlier years that the normal response of euro-area fiscal policies to cyclical developments has been in general either a-cyclical or pro-cyclical. The type of information set used seems a crucial element in explaining the different results. If we replicate our analysis using the latest available (*ex post*) information for our regressors, the estimated reaction becomes smaller and not significant.
- The results for the response to cyclical conditions have some implications for the current debate on fiscal rules and policies. First, as well as Galí and Perotti (2003), we do not observe the pro-cyclical bias that could stem from the Stability Pact (Bean, 1998). Second, taking into account that the counter-cyclical reaction comes on top of the

working of the automatic stabilizers, there is probably little need to modify fiscal rules in order to induce governments to seek greater stabilization as suggested, for example, in Bruck and Zwiener (2006). Finally, the results based on *ex post* information suggest that actual stabilization carried out by the governments (which is particularly important for the euro area, not only because of the centralization of monetary and exchange policies, but also owing to the limited geographical mobility of labour and to wage flexibility; cfr. Feldstein, 2005) would be enhanced by improving the real-time assessment of cyclical conditions.

- When we distinguish between favourable and adverse cyclical conditions there is no evidence of the asymmetry in the policy response that some recent studies found.
- We find strong evidence for the existence of a political budget cycle, but the fiscal loosening associated with elections (1.4 per cent of GDP in the year in which they are held and 0.6 per cent in the year before) is present only if cyclical conditions are assessed as favourable when the relevant budgetary decisions are taken. The tentative explanation we offer for this pattern suggests that improving information on cyclical conditions and on their impact on budget balances would help to reduce electoral manipulations of fiscal policy. It is noteworthy that the evidence of a political budget cycle tends to disappear when we use the latest available (*ex post*) information for our regressors.
- The results are robust to alternative measures of the dependent variable and of the regressors, and to the exclusion of any country, in turn, from the sample. In particular, the estimate of the response of fiscal policies to cyclical developments is almost unaffected by the imposition that the effects of elections be constant across good and bad times.
- Many of our results do not carry over when we apply the same model to a group of 8 OECD countries outside the area.

**ELECTION DATABASE (FROM 1987 TO 2007):  
YEAR (MONTH) R=REGULAR, E=EARLY**

United States of America: 1988(11)R, 1992(11)R, 1996(11)R, 2000(11)R, 2004(11)R

Japan: 1990(2)R, 1993(7)E, 1996(10)E, 2000(6)R, 2003(11)E, 2005(9)E

Germany: 1987(1)R, 1990(12)E, 1994(10)R, 1998(9)R, 2002(9)R, 2005(9)E

France: 1988(6)E, 1993(3)R, 1997(5)E, 2002(4)R, 2007(4)R

Italy: 1987(4)E, 1992(4)R, 1994(3)E, 1996(4)E, 2001(5)R, 2006(4)R

United Kingdom: 1987(6)R, 1992(4)R, 1997(5)R, 2001(6)R, 2005(5)R

Canada: 1988(10)E, 1993(10)R, 1997(6)E, 2000(11)E, 2004(6)E

Australia: 1987(7)E, 1990(3)E, 1993(3)R, 1996(3)R, 1998(10)E, 2001(11)R, 2004(10)R, 2007(11)R

Austria: 1990(1)R, 1994(1)R, 1995(12)E, 1999(10)E, 2002(11)E, 2006(11)E

Belgium: 1987(12)E, 1991(11)R, 1995(5)R, 1999(6)R, 2003(5)R, 2007(5)R

Denmark: 1987(9)R, 1988(5)E, 1990(12)E, 1994(9)R, 1998(3)R, 2001(11)R, 2005(2)R

Finland: 1987(3)R, 1991(3)R, 1995(3)R, 1999(3)R, 2003(3)R, 2007(3)R

Greece: 1989(9)R, 1993(10)R, 1996(9)E, 2000(4)R, 2004(3)R

Ireland: 1987(2)R, 1989(6)E, 1992(11)E, 1997(6)R, 2002(5)R, 2007(6)R

Netherlands: 1989(9)E, 1994(5)R, 1998(5)R, 2002(5)R, 2003(1)E, 2007(5)R

New Zealand: 1987(7)R, 1990(7)R, 1993(11)R, 1996(10)R, 1999(11)R, 2002(7)R, 2005(7)R

Portugal: 1987(7)E, 1991(10)R, 1995(10)R, 1999(10)R, 2002(3)E, 2005(2)E

Spain: 1989(10)E, 1993(6)R, 1996(3)E, 2000(3)R, 2004(3)R

Sweden: 1988(9)E, 1991(9)E, 1994(9)E, 1998(9)R, 2002(9)R, 2006(9)R

## APPENDIX 1

### FROM THE GENERAL TO THE BASE MODEL

In this Appendix we provide a detailed description of the process of reduction from a general unrestricted model (GUM) to our base model and from a general model which does not include the Maastricht variable (GUM-Eq. [1a]) to the Eq. [1a] model.

Preliminarily, we perform on the GUMs (GUM and GUM-Eq. [1a]) a number of specification tests. Results are shown in Table A1.1. In detail, the upper half of the table shows the results of a few specification tests to the GMM estimates of the GUMs and of the comparison between the latter and the estimates based on OLS. In the lower part we analyse the statistical properties of the residuals obtained with OLS.

Then we assess the restrictions which enable us to move from the general unrestricted model to, respectively, the base and the Eq. [1a] models. This analysis performed for all restrictions at once and, for greater transparency, also for homogeneous groups of restrictions (Table A1.2). In particular, we assess the sets of restrictions which enable us to move to two intermediate models, IM-BASE and IM-Eq. [1a], which differ from the final ones (base and Eq. [1a] model) only for the fact that they allow the values of policy parameters to vary depending on the sign of the output gaps. The estimates for these intermediate models are shown in the main text in Table 3.

#### Validation of the GUMs

In order to decrease the impact on parameter estimates of biases due to possible model specification errors in the GUMs, we allow for country and time effects. The country effects should account for the influence of almost time-invariant omitted variables, and the time effects should allow for a degree of dependency across individuals due to common factors (individual-invariant omitted variables). It is widely acknowledged that the presence of individual effects in dynamic panel models implies that the lagged dependent variable is correlated to the equation error. In this context, the approach proposed by Arellano and Bond (1991), involving the GMM applied to differenced data, delivers consistent parameter estimates. Nevertheless, we prefer to use OLS estimators, especially for the restricted models, for a number of reasons.

First, in our analysis two factors should limit the risk of biases of the OLS estimator. The different nature of data – cyclically-adjusted *ex post* data for the dependent variable  $\Delta capb_{it}$  and unadjusted real-time data for the explanatory primary balance  $pb_{it-1}$  – should weaken the endogeneity problem of the regressor. Moreover, the size of the bias should be limited, as it is inversely proportional to the time dimension of the sample, which in our case is relatively large (19 years). In this context, the OLS bias may be more than offset by its greater precision compared to GMM estimator (see Nickell, 1981; Judson and Owen, 1999; and Attanasio *et al.*, 2000).

Second, estimates of OLS over GMM can be formally compared with the Hausman (1978) test. As the test does not reject the null, suggesting OLS and GMM (in differences) estimates are equivalent, OLS estimates are advisable, being more efficient.

Finally, it should be pointed out that the country effects, though on the basis of OLS estimates, can be restricted to zero (see the following section of this Appendix) and in this context the OLS method delivers consistent parameter estimates.

As a check preliminary to performing the Hausman (1978) test, we assess the estimates of the GUMs with the GMM-differences approach proposed by Arellano and Bond (1991). The main diagnostics are laid out in the upper panel of Table A1.1. The absence of second-order residual autocorrelation suggests well behaved residuals, while the presence of first-order autocorrelation is simply due to data transformation in first-differences. Hansen (1982) *J*-test does not reject, at least at the 1% level, the over-identification restrictions (*i.e.* the choice of the instruments).

The lower part of Table A1.1 reports the main tests on residuals with OLS, namely: White (1980) and Breusch and Pagan (1980) tests for heteroskedasticity, Ramsey (1969) specification error test, Bhargava *et al.* (1982) Durbin-Watson-type, Wooldridge (2002, pp. 282-83) and Arellano and Bond (1991) tests for first- and second-order autocorrelation. In addition, Godfrey (1988) LM-type tests for first- and second-order autocorrelation are reported for OLS estimates without fixed individual effects (*i.e.*, only for base and intermediate models). The residual diagnostics are reported not just for the GUMs but also for the intermediate models (IMs) and for the final models. In general, the models performance is in line with the hypothesis of well-behaved residuals; hence, parameter inferences can be drawn on the basis of OLS estimator statistical distributions.

## Validation of the restrictions

Table A1.2 presents the results of the tests on the restrictions which allow to move from the GUMs to our intermediate models (IM-BASE and IM-Eq. [1a]) and to our final specifications (base and Eq. [1a]) discussed in the main text.

Table A1.1

### GUM AND BASE MODELS MISSPECIFICATION TESTS AND DIAGNOSTICS

	Equations [1a]–[1b]			Equation [1a]		
Residual and specification tests of GMM estimates						
GMM AR1 $p$ -values	0.8%			2%		
GMM AR2 $p$ -values	8.8%			24.5%		
Hansen $J$ $p$ -values	1.5%			8.0%		
Hausman statistic, $\chi^2$	48.3			60.3		
- degrees of freedom <sup>(1)</sup>	61			45		
- $p$ -values	88.1%			6.4%		
Analysis of GUM, IM and base model OLS estimates						
Residual tests:	GUM	IM	BASE	GUM[1a]	IM[1a]	Eq. [1a]
- White $p$ -values	46.7%	70.7%	31.0%	44.7%	93.1%	56.6%
- Breusch-Pagan $p$ -values	65.4%	93.6%	97.9%	81.5%	90.6%	99.0%
- Ramsey RESET $p$ -values	0.1%	49.8%	46.7%	7.4%	7.1%	6.6%
- Bhargava et al Durbin-Watson	2.38	2.25	2.24	2.51	2.25	2.25
- Wooldridge, AR1 $p$ -values	5.6%	5.1%	10.9%	0.8%	2.7%	2.9%
- Arellano-Bond, AR2 $p$ -values	35.5%	13.9%	17.5%	47.1%	13.1%	12.3%
- Godfrey LM, AR1 $p$ -values		5.4%	6.4%		5.7%	5.7%
- Godfrey LM, AR2 $p$ -values		14.9%	16.6%		16.4%	16.3%
Other diagnostics:						
- Number of parameters	91	30	25	66	29	24
- Number of observations	209	209	209	209	209	209
- RMSE	1.118	1.132	1.118	1.162	1.147	1.141
- $R^2$	0.603	0.383	0.381	0.480	0.362	0.352
- $R^2$ adjusted	0.297	0.280	0.297	0.240	0.260	0.268

<sup>(1)</sup> Number of parameters in GUMs (see below), excluding the individual effects.

The upper part of Table A1.2 (rows 1-10) is devoted to test restrictions that imply a switch from equation [1a] to equation [1b] when the Maastricht variable  $m_{it-1}$  is negative (see Section 2). This dichotomic representation requires that all parameter estimates of equation [1a] be not significantly different from zero when  $m_{it-1}$  is negative. The tests of these 25 restrictions are shown in row 8 of Table A1.2. Since the GUM allows for different  $\phi_m$  parameters for the run-up to Maastricht (1993-97) and for the post-1998 period, we also test the restriction that the two parameters are equal (row 9).

On the basis of the large  $p$ -value reported in row (10), the 26 restrictions that allow to simplify the GUM into the dichotomic representation [1a]–[1b] (*i.e.*  $m_{it-1} < 0$ ), cannot be rejected.

In row 11, we present the results of the test on whether it is admissible to restrict to zero the effects of the German unification, a captured by dummies for the years 1990-94. The GUMs allow the possibility of asymmetry, depending on the sign of the output gap, both in country and time fixed-effects and in explicit policy parameters (which measure the reactions to, respectively, the output gap, the initial conditions of public finances and the coming elections). Rows 12 and 13 of Table A1.2 show the results of tests examining the restrictions which impose symmetry in the country and time effects, respectively. The overall test of row 14, for both the GUM and the GUM-Eq. [1a], suggests that the null hypothesis of no German unification effects and of symmetry in country and time effects cannot be rejected. Therefore, the German unification dummies can be excluded from the specifications and the two sets of, respectively, country and time effects can be unified.

In lines 15 and 16 we test the relevance of (symmetric) country and time effects. Individual effects are largely not significant, while the overall relevance of time dummies is relatively less clear. To assess the latter, we prefer referring to the  $p$ -values in row (IM1), where only 19 restrictions are tested (against the intermediate specifications IM-BASE and IM-Eq. [1a]), and to reject the null hypothesis of zero time effects.

Therefore, on the basis of the finding just mentioned, we are able to simplify the starting GUMs by imposing the 63 and 37 non-rejected restrictions in row (17) to, respectively, the GUM and the GUM-Eq. [1a]. On the basis of the resulting intermediate models (IM and IM-Eq. [1a]) in Section 6 (Table 3) we examine the issue of asymmetry/symmetry in policy actions, depending on whether the output gaps are favourable or adverse.

Table A1.2

**FROM GENERAL TO RESTRICTED MODELS,  
TESTS ON COEFFICIENT RESTRICTIONS**  
(*p*-values of the tests)

	<i>From GUM</i>	
Tests of irrelevance of other factors if $m_{it} < 0$		
(1) no country effects	44.2%	
(2) no time effects	85.1%	
(3) no country and time effects = (1+2)	73.2%	
(4) no output gap effects	74.6%	
(5) no primary balance and debt effects	98.6%	
(6) no election effects	57.0%	
(7) no (4 + 5 + 6) effects	91.7%	
(8) no country, time and policy effects = (3+7)	83.0%	
(9) $\phi_m$ constancy	70.1%	
(10) All restrictions above	85.0%	
		<i>From</i>
Test of irrelevance of German unification dummies <sup>(1)</sup>		
(11) no effects of German unification dummies <sup>(1)</sup>	84.4%	52.7%
Tests of symmetry w.r.t. the sign of the output gap <sup>(1)</sup>		
(12) symmetry in country effects <sup>(1)</sup>	26.3%	40.4%
(13) symmetry in time effects <sup>(1)</sup>	85.0%	69.7%
(14) All the restrictions above <sup>(1)</sup>	37.1%	56.1%
(15) no (symmetric) country effects <sup>(1)</sup>	31.8%	54.1%
(16) no (symmetric) time effects <sup>(1)</sup>	46.1%	13.2%
(17) Test of restrictions from GUMs to the Intermediate models: restrictions (14+15) <sup>(2)</sup>	37.7%	67.3%
(18) Restrictions (14) and no time effects (16)	11.8%	12.4%
(19) Test of restrictions imposed on Final models: restrictions (17+IM2) <sup>(3)</sup>	46.3%	66.0%
	From IM-BASE	From IM-Eq. [1a]
Further tests, starting from Intermediate Models		
(IM1) no time effects	1.7%	2.2%
(IM2) symmetry (w.r.t. the sign of the output gap) in output gap, primary balance and debt effects	89.9%	45.5%
(IM3) symmetry (w.r.t. the sign of the output gap) in election effects	4.1%	1.9%
(IM4) no effects of elections if the output gap <0	96.0%	95.9%

<sup>(1)</sup> The following tests include, for the first column, all the restrictions in row 10.

<sup>(2)</sup> The null hypothesis of these restrictions identifies in the first and in the second column, respectively, the IM-BASE and the IM-Eq. [1a] model; their parameter estimates are reported in Table 3.

<sup>(3)</sup> The null hypothesis of these restrictions identifies in the first and in the second column, respectively, the BASE and the Eq. [1a] models; their parameter estimates are reported in Table 1.



Here, summarizing the results of Section 6, we show that the joint test on the symmetry of policy reactions to output gaps, primary balance and debt effects (row IM2) are largely not rejected, while that on the symmetry of the policy reactions to elections is clearly rejected. Moreover, in the case of elections, their effects when the gaps are negative are not significant and can be excluded (row IM4). Overall, the reduction from GUMs to the corresponding specifications, base and Eq. [1a] model, respectively implies 67 and 44 largely non-rejected restrictions, with  $p$ -values equal to 46.3% and 66.0%.

## APPENDIX 2

### OTHER ROBUSTNESS EXERCISES

In this section we test the robustness of our estimates to the timing and measurement of the output gap and to the exclusion of any single country of our sample. In the main text, additional evidence of robustness has been provided: in Table 1 for alternative samples of countries (outside the euro area), in Table 2 for different periods of time, and in Table 4 using alternative elections' indicators.

#### **Robustness to the timing and measurement of the output gap**

Given the relevance of the role played by the output gap in our modelling strategy (it is both a regressor of the base model and the variable governing the cyclical phases), it is important to assess the robustness of our results with respect to alternatives involving this variable. As far as timing is concerned, in our base model we assume that policymakers react to the *current* cyclical conditions ( $x_{t-1}$ ), *i.e.* existing at time the policy is set, but they may plausibly react to the conditions *expected* for the following year ( $x_t$ ). In this case, because of the simultaneity of the explanatory output gap, the base model parameters must be estimated with instrumental variables (IV) rather than using OLS. As for alternative output gap measures, we use: that obtained by filtering GDP real time data with the traditional Holdrick-Prescott approach instead of Mohr's, and that reported in the OECD *EO*. A drawback with the *EO* measure is the reduced number of observations available (only since 1993, see Section 3).

Estimation results of all the robustness exercises about the output gap described above are reported in Table A2. In particular, in the first column (BASE( $x_{t-1}$ )) we report the benchmark estimates of the base model over the period 1988-2006. In the second column (IV) the instrumental-variable estimate of the base model is reported. The (simultaneous) output gap at time  $t$  is instrumented with its lagged values.

It is well known that the performance of estimators exploiting instrumental information crucially depends on the relevance of the instruments in question, that is, on the correlation between the instruments and the endogenous explanatory variables. In finite samples, low instrument relevance ("weak instruments") can lead both to biased estimators

and to the departure of their distribution from the asymptotic normal. In order to check for instrument relevance, we performed the Stock, Wright and Yogo (2002)  $F$ -statistic to test the null hypothesis that the lagged output gap is weak. The first-stage  $F$ -statistic in our case (27.9) is well above the 5% critical value (8.96, see Stock, Wright and Yogo, 2002, Table 1, p. 522), and leads to the rejection of the null.

In the third column (HP), the output gap is measured by the traditional Holdrick-Prescott-filtered GDP. In the case presented here, we set to 100 the smoothing parameter, but the use of alternative values of the parameter would not significantly alter our results.

Table A2

**ROBUSTNESS TO ALTERNATIVE ESTIMATES OF CYCLICAL CONDITIONS<sup>(1)</sup>**

Model Parameter	1988-2006			1994-2006	
	BASE ( $x_{t-1}$ )	IV ( $x_t$ )	HP ( $x_{t-1}$ )	BASE( $x_{t-1}$ )	OECD( $x_{t-1}$ )
$\phi_{pb}$	-0.192	-0.180	-0.223	-0.189	-0.223
	-4.39	-4.11	-4.66	-2.59	-2.76
$\phi_d$	0.011	0.009	0.014	0.008	0.009
	3.03	2.46	3.57	1.50	1.34
$\phi_m$	-0.619	-0.619	-0.619	-0.653	-0.653
	-6.09	-6.05	-5.97	-6.21	-6.12
$\phi_x$	0.427	0.553	0.489	0.421	0.203
	3.82	3.29	3.61	3.16	2.16
$\phi^{(p)}_{e1}$	-1.366	-1.258	-1.153	-1.153	-1.094
	-4.16	-3.42	-3.14	-3.09	-2.50
$\phi^{(p)}_{e2}$	-0.551	-0.728	-0.139	-0.791	-1.339
	-1.77	-2.21	-0.47	-1.94	-2.41
N. of observ.	209	209	209	143	143
RMSE <sup>(2)</sup>	1.118	1.126	1.140	1.021	1.037
$R^2$	0.381	0.355 <sup>(3)</sup>	0.356	0.410	0.391
$R^2$ adjusted	0.297	0.267 <sup>(3)</sup>	0.269	0.319	0.298

<sup>(1)</sup> The  $t$ -statistics are reported below the estimates. The estimates of the time-dummies are not reported.

<sup>(2)</sup> Root Mean Squared Error.

<sup>(3)</sup> Generalised  $R^2$ , see Pesaran and Smith (1994).

In order to ease comparisons, columns four and five of Table A2 report alternative estimates of the base model over the common sample 1994-2006, given the limited availability of OECD's output gap data. The fourth column reports estimates based on our data set, while the fifth column shows results based on the estimates of the output gap of the OECD (OECD).

These robustness experiments confirm our base model findings, pointing to the asymmetry of the election effects and to significant, and symmetric, counter-cyclical policies. Across the first three columns, the expansionary effect of elections in the same year they are held is quantitatively similar, while there is some variability for the effect of the elections held in the previous year.

In the last two columns of Table A2, notwithstanding the reduced dimension of the sample, our results generally confirm the estimation results of the base model. However, in the last column the reaction to cyclical conditions is lower (but still significant) and the effect of the elections in the year before that in which they are held is higher.

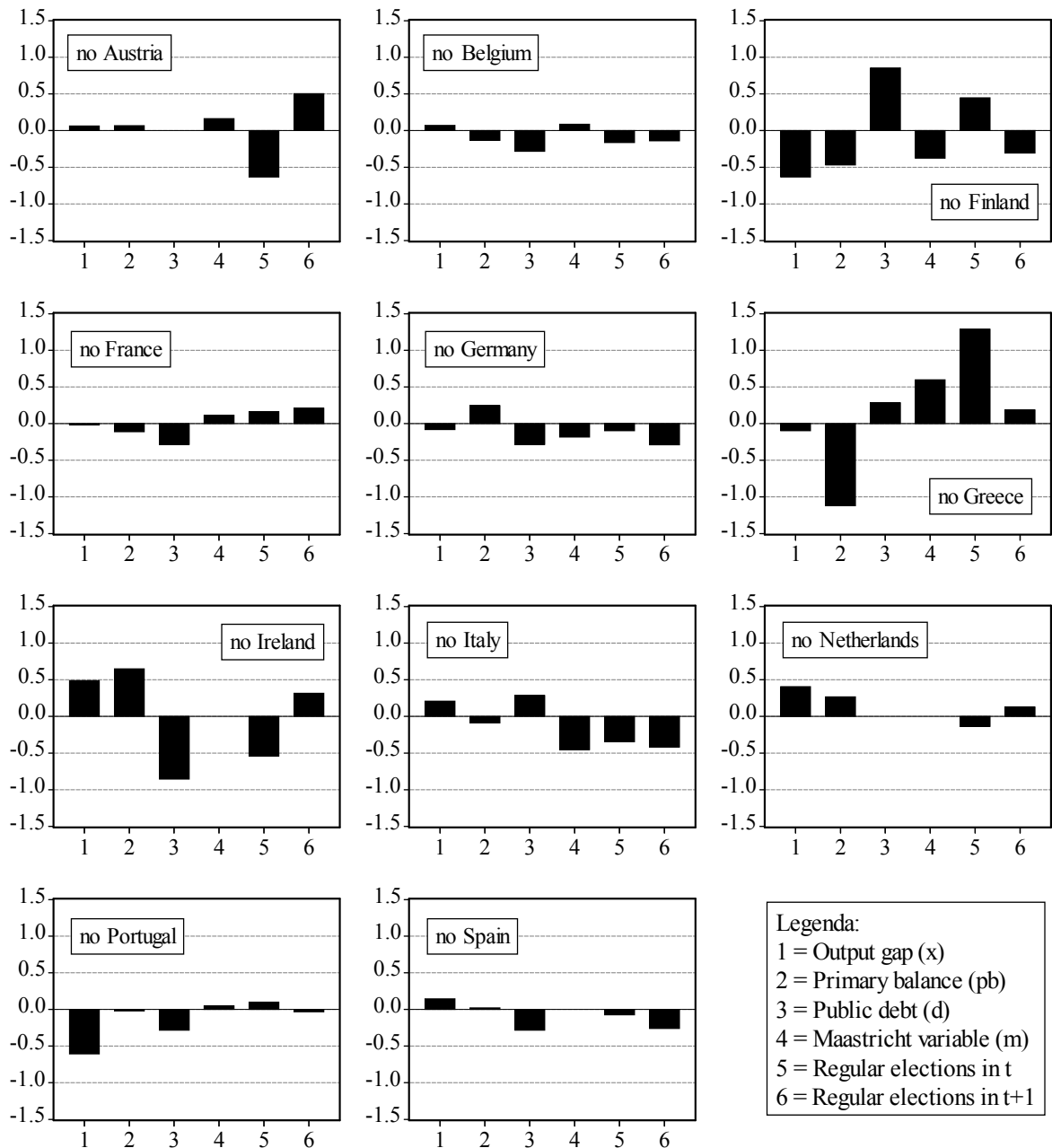
### **Robustness in euro-area subsamples**

We estimated our base model on the basis of eleven alternative samples obtained by excluding one country at a time (the number of observations in each sub-sample is 190 against 209 in the full sample). Results are shown in Figure A2, where each plot represents one particular sample (e.g. the "no Austria" plot reports estimation results for the euro-area sample without Austria).

In order to ease the comparison between the results for each of the 11 sub-samples and for the base model estimates, we report for each parameter (here represented by a histogram bar) the difference of its sub-sample estimate against the corresponding result in the base model, divided by the standard error. The results indicate that sub-sample estimates never fall outside the corresponding 95% confidence intervals (two standard errors) of the base model estimates. In fact, even the larger discrepancies (such as those involved by excluding Greece, Finland or Ireland) rarely fall outside the  $\pm 1$  range.

Figure A2

**NORMALISED DIFFERENCES WITH RESPECT TO THE BASE MODEL  
ESTIMATES OBTAINED BY EXCLUDING, IN TURN,  
ONE COUNTRY FROM THE EURO-AREA PANEL<sup>(1)</sup>**



<sup>(1)</sup> Each parameter estimate (along the horizontal axis) is measured as the difference with respect to the corresponding estimate of the base model in terms of its standard error. In this way, bins bigger than two in absolute value suggest that the corresponding estimates (obtained excluding that country from the sample) lay outside the 95% confidence interval of the base model estimates.

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