Fiscal Adjustment to Cyclical Developments in the OECD: An Empirical Analysis Based on Real-Time Data[*]

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ABSTRACT

Estimation of fiscal policy reaction functions usually links fiscal outcomes to macroeconomic and possibly other factors. This approach ignores that those outcomes are the result of both a planning and an implementation stage. In this paper, we explore the determinants of both stages of the fiscal process for OECD countries during the period 1995-2006. We first estimate standard fiscal rules using ex-ante data (i.e. forecasts). We then estimate how fiscal policy reacts to new information, especially on the business cycle. There are marked differences between planned behaviour and responses to new information, as well as between the fiscal policy of the EU countries and the other OECD countries. Planned fiscal policy is a-cyclical for the EU countries and counter-cyclical for the other countries. However, in the implementation stage, the EU countries react pro-cyclically to unexpected changes in the output gap, while the responses of the other OECD countries are a-cyclical. These findings show that the empirical distinction between the two fiscal stages is crucial and that the relatively strong emphasis on ex-ante compliance with fiscal rules (such as Europe's Stability and Convergence Programs) may be misleading. Our results also show that it is important to analyse the two stages *jointly*, because more ambitious plans lead to larger implementation errors. Finally, our approach has the advantage that we use real-time data. Hence, we can better approximate the information set of the policymakers when they took their decisions than with commonly-used revised data that often become available after long delay and substantial revision in response to information that was not available at the time.

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1. Introduction

A substantial number of recent contributions have estimated fiscal rules for the EU or the OECD. The results are sometimes conflicting (see Golinelli and Momigliano, 2008, for a recent overview), but it is frequently found that fiscal policy has behaved pro-cyclically during at least part of the sample periods under consideration. For example, Galí and Perotti (2003), Wyplosz (2006) and Annett (2006) find for the Euro-zone that fiscal policy has behaved pro-cyclically before the Maastricht Treaty was signed, and a-cyclically after that. Lane (2003), Balassone and Francese (2004) and Debrun and Kumar (2007) find for a set of OECD countries that fiscal policies have (on average) been pro-cyclical.

These contributions all try to explain the "overall" fiscal outcomes, while fiscal policy involves a planning and an implementation stage that together determine the eventual fiscal outcomes. The empirical distinction between the two fiscal stages is important, for example because it may reveal at what stage enforcement of fiscal rules should be directed to be most effective. Also, discretionary corrections to the original fiscal plans can be costly if the private sector has taken decisions on the basis of these plans.

In this paper we explore empirically for European Union (EU) and other OECD countries how fiscal policymakers set their planned cyclically-adjusted primary deficits in relation to macroeconomic projections and how they deviate from these plans in response to new information about the economy. We pay particular attention to the cyclicality of such fiscal policy corrections. We find substantial differences between planned fiscal behavior and responses to new information about the economy, both between the EU countries and the other OECD countries. Planned fiscal policy is a-cyclical for the EU countries and counter-cyclical for the non-EU countries. However, the response of this latter group to new business cycle information is a-cyclical. By contrast, the EU countries react in a procyclical way to new information about the output gap. Further investigation suggests that this finding should be attributed to discretionary fiscal contractions in response to an unexpected deterioration of the business cycle. Additionally, for the EU countries the planned fiscal stance is more relaxed for election years, while a projected violation of the EU deficit restrictions imposed on these countries induces a tightening in the planned fiscal stance.

The differences in fiscal behaviour between the planning and implementation stages confirm the relevance of the empirical distinction between the two fiscal stages. However, it is also important that the two stages are analyzed jointly. In particular, even though on average implementation errors

do not differ significantly from zero, a more tightly planned ("overambitious") budget tends to lead to larger implementation errors.

Our paper not only contributes to the literature by distinguishing between the planning and implementation stages of fiscal policy, it also contributes by using real-time rather than ex-post (i.e., revised) data as employed in the contributions mentioned earlier. That is, we model plans on the basis of the (economic) information that was available when the plan was drawn up, and we model implementation on the basis of information as it becomes available between the planning moment and the moment the next data release is brought out. This way we approximate as closely as possible the information sets of the authorities at the moments they took their decisions.

We use data from the OECD. This has several additional advantages. First, the definitions and methodologies used to construct the variables are comparable across countries. Second, OECD data are published in December. This is later in the year than the forecasts in alternative datasets (such as the notifications from the Excessive Deficit Procedure (EDP)), which makes it more likely that the OECD forecasts are indeed based on the definitive plans approved by the national parliaments.[1] Finally, OECD data allow for the use of a larger country sample and include directly the cyclically-adjusted fiscal measures.

The OECD applies its own judgment to the plausibility of the projections it receives from the national governments and the eventual OECD projections become available only after consultation rounds with the national authorities. This limits the scope for the fiscal plans as published by the OECD to be politically biased.[2,3] Even so, we check that there are no systematic biases (such as an overoptimism bias resulting from too rosy forecasts of potential output) in our data.

Closest in spirit to this paper are Brück and Stephan (2006) and Pina and Venes (2007). In contrast to this paper, these papers are interested in the political determinants of forecast errors in fiscal policy, while controlling for economic variables. Further, they do not cyclically adjust fiscal policy, they do not split between EU and non-EU countries, and their direct data sources and sample periods are different. However, most importantly, they do not jointly consider the two stages of fiscal policy and the way these stages relate to each other. As far as their data sources are concerned, Brück and Stephan (2006) use fiscal forecasts published by the European Commission. Pina and Venes (2007) base their analysis on the budgetary notifications in the context of the

[1] This is not the case for all countries, because for some of them the fiscal year differs from the calendar year.
[2] Here, "political bias" refers to the incentive of the government to publish a plan that differs from the true plan that it has in its mind. This true, unreported plan and the eventual policy outcomes may of course also be driven by political considerations.
[3] A number of recent papers assess the potential presence of biases in economic and budgetary forecasts (see, for example, Strauch et al., 2004, Jonung and Larch, 2006, and Pina and Venes, 2007, for the European countries).

Excessive Deficit Procedure (EDP). Other authors (Strauch, *et al*, 2004, Moulin and Wierts, 2006, and Annett, 2006) use the projections in the EU Stability and Convergence Programmes (SCPs).

The remainder of the paper is structured as follows. Section 2 presents our decomposition of the eventual fiscal outcomes into planned outcomes and implementation errors. This section also discusses the data and the notation. Section 3 analyses planned fiscal policy. Similarly, Section 4 investigates fiscal policy responses to new information about the economy. Section 5 concludes this paper and draws some policy implications.

2. The general framework, notation and data

We start with the decomposition of the fiscal process. Our data (except those on population composition and election years, which are from the Comparative Political Dataset, 2007) come from the various December issues of the OECD Economic Outlook (EO). Each issue of the EO contains "forecasts" of variables for the next year, "estimates" of variables for the current year and revised values of variables over the preceding years ("revised data"). We shall refer to the estimates for the current year as "first-release data".

We adopt the following notational convention that remains valid for the remainder of the paper. For a generic variable, X_{it}^{τ} denotes the December of year τ release of *X* for country *i* in year *t*. Hence, X_{it}^{t-1} denotes the December year *t*-1 *forecast* for year *t*, while X_{it}^{t} denotes the *first release* for year *t*, that is the *estimate* for year *t* released in December of year *t*. The current estimate of the fiscal stance DEF_t^t can be decomposed into:

$$DEF_t^t = DEF_t^{t-1} + \left(DEF_t^t - DEF_t^{t-1} \right), \tag{1}$$

where *DEF* denotes the cyclically-adjusted primary deficit as a share of GDP. The first term DEF_t^{t-1} on the right-hand side of (1) is the fiscal plan composed towards the end of year *t*-1 for year *t*, while the second term is the deviation from plan (also the implementation error). The first stage of our analysis consists of linking the fiscal plan to its real-time determinants. In the second stage we model the deviations from plan. Here, we pay particular attention to the response of the implementation error to new information on the business cycle. However, we also link the implementation stage to the planning stage. In particular, as we shall see below, a more ambitious fiscal plan (a lower DEF_t^{t-1}) may be associated with a larger implementation error.

First-release data capture most accurately the information set available to the fiscal policymakers at the moment they take decisions to revise their policy in deviation from their

original plan. Hence, "first-release data" are most suited for our purposes. Revised data, as are commonly used in empirical fiscal studies, often contain information that was not available at the moment when policy decisions were taken.

Another disadvantage of using revised data is that the methodology for constructing the data changes over time. This reduces the comparability of the revised and originally planned figures. For example, at some point the national statistical offices or the OECD may decide to include (or exclude) certain components in the calculation of the deficit or they may update previous estimates of tax elasticities, etcera. One could view these methodological updates as random shocks independent of the shocks that hit the economy. The larger the time difference between the original plan for a year and the revised figure for that year, the larger is the variance of the accumulated methodological shock.

Our dataset consists of a panel. In the time dimension it runs from 1995 to 2006. The year 1995 is when the OECD started publishing cyclically adjusted fiscal positions and output gaps. The cross-section dimension consists of the EU-14 (i.e., all countries that were part of the EU before 2004 minus Luxemburg), plus the U.S., Canada, Japan, Norway and Australia.

3. The fiscal planning stage

The baseline specification for planned fiscal behavior is:

$$DEF_{it}^{t-1} = c_i + \theta_t + \rho DEF_{i,t-1}^{t-1} + \alpha Y_{it}^{t-1} + \beta' x_{it} + u_{it}, \qquad (2)$$

where c_i is a country-specific constant, θ_t is a year-specific constant (a time-fixed effect) and Y denotes the output gap (defined as the deviation of actual from potential GDP, as a share of the latter). Finally, x_{it} is a set of control variables. For the baseline specification, x_{it} contains $DEBT_{i,t-1}^{t-1}$, the debt/GDP ratio, because more indebtedness may lead to more concern about fiscal sustainability and induce governments to contract fiscal policy (as found by Ballabriga and Martinez-Mongay, 2003, and Favero, 2003, for realised EU data – though for a different sample period); $ELECT_{it}$, an election dummy that takes a value of one (zero) if year t is (not) an election year; $NONACTIVE_{it}$, the share of the population that is not of working age (the number of 15 year or younger plus the number of 65 or older as a share of the total population); $M_{i,t-1}^{t-1}$, a "Maastricht" variable for whether the 3% deficit limit in the period before EMU is exceeded (taking a zero value otherwise); and

$SGP_{i,t-1}^{t-1}$, a Stability and Growth Pact (SGP) variable for whether the 3% deficit limit is exceeded during EMU membership (taking a zero value otherwise).[4] Variables $ELECT_{it}$ and $NONACTIVE_{it}$ are denoted without superscript, because they are not from the EO. Moreover, they are realizations rather than forecasts,[5] so strictly speaking they will not be in the information set of the fiscal policymaker when it sets up its plan. However, many, if not most, elections are scheduled quite long in advance, while $NONACTIVE_{it}$ is a slow-moving demographic variable. If anything, if we already detect an effect of prospective elections using the variable $ELECT_{it}$, then we would expect the effect to become only stronger if we were to use a proxy that actually is in the information set of the policymaker at the moment he draws up his plan.

The baseline regression includes country-specific constants to capture country-specific effects as well as systematic misrepresentation in the forecasts to the extent that such misrepresentation is constant over time (for example, there may be specific time-invariant political incentives to exaggerate potential output). The baseline regression also includes time-fixed effects. These are intended to capture common third factors driving all fiscal stances, interdependence in fiscal stances that is unlinked to common economic circumstances (for example, due to peer pressures of countries on each other's fiscal policies – see Giuliodori and Beetsma, 2007) and common changes in the methodology of data construction.[6]

We estimate equation (2) using instrumental variables, because one might expect potential feedback effects from the planned fiscal stance DEF_{it}^{t-1} onto the forecast of the output gap Y_{it}^{t-1}. As instruments for Y_{it}^{t-1} we use the estimate of the output gap for the previous period, $Y_{i,t-1}^{t-1}$, and the GDP-weighted averages of the output gap, short-term interest rate and long-term interest rate forecasts of the *other* countries in the (sub) samples (YW_{it}^{t-1}, $IRSW_{it}^{t-1}$ and $IRLW_{it}^{t-1}$, respectively). These variables can reasonably be expected to be exogenous. By far the most important instrument

[4] More precisely, these variables are defined as follows (see also Forni and Momigliano, 2004, who introduce these variables into their analysis). $M_{i\tau}^{\tau} \equiv \left(DE_{i\tau}^{\tau} - 3\%\right)/\left(1997 - \tau\right)$, if $DE_{i\tau}^{\tau} > 3\%$, $\tau < 1997$ (for Greece, $\tau < 1999$) and i is currently in the Euro-area; and $M_{i\tau}^{\tau} \equiv 0$, otherwise. Here, "*DE*" stands for the actual (i.e., not cyclically adjusted) deficit. Further, $SGP_{i\tau}^{\tau} \equiv \left(DE_{i\tau}^{\tau} - 3\%\right)/2$, if $DE_{i\tau}^{\tau} > 3\%$, $\tau \geq 1997$ (for Greece, $\tau \geq 1999$) and i is currently in the Euro-area; and $SGP_{i\tau}^{\tau} \equiv 0$, otherwise. Beetsma and Debrun (2007) provide a recent discussion of the SGP.

[5] We do not have forecasts for $ELECT_{it}$ and $NONACTIVE_{it}$ Realizations of $NONACTIVE_{it}$ for 2006 are based on interpolation from preceding observations.

[6] Obviously, while we focus on the response of the ex ante fiscal stance to the ex ante output gap, with the inclusion of time dummies we will fail to capture that part of the response that is due to a common reaction to global ex ante output gap movements. However, econometric testing strongly favours the inclusion of time dummies as a prerequisite for an appropriate econometric specification. In fact, some further investigation suggests that the common response to global ex ante output gap movements is only of minor importance.

is $Y_{i,t-1}^{t-1}$ and, in fact, dropping the other instruments has no qualitative and even very little quantitative effect on the estimates.

Table 1, column 1, presents the estimates for a sample in which we include all available countries. Consistent with what is generally found in empirical work on fiscal policy rules, the estimates suggest a large degree of persistence in the planned fiscal stance in the sense that most of the explanatory power is provided by the estimated stance for the preceding year, $DEF_{i,t-1}^{t-1}$. The output gap enters with a significantly negative coefficient suggesting that as a group the OECD countries have been following counter-cyclical fiscal plans.[7] Public debt enters with an unexpected positive coefficient, but it is insignificant. The share of non-active people does not seem to play any role. If anything, the sign of its coefficient is the opposite of what one would expect. The election dummy is highly significant with the expected sign: the expectation of a future election induces the policymaker to plan a more relaxed fiscal stance. This is in line with the findings by Buti and Van den Noord (2004) for ex-post data. Finally, the variables capturing the violation of the Maastricht and SGP 3% limit on the deficit are highly significant, indicating that, at least in their planned behavior, the fiscal authorities react with a fiscal tightening in the case of such a violation.[8]

In order to assess the robustness of our results, we have rerun our baseline regression excluding one country at the time. However, the main results are not sensitive to the exclusion of individual countries. In another robustness check we have normalized the cyclically-adjusted primary deficit with potential GDP instead of actual GDP. This produces almost identical results, which excludes the possibility of potential spurious correlation via the denominator of the dependent variable. These results are reported in Table A.1 of the Additional Appendix (not for publication).

We estimate the baseline specification also for subgroups of countries. In particular, we split the country sample into EU countries and non-EU countries. If we restrict ourselves to the EU-14, the regression outcomes remain qualitatively unaltered, except that the output gap is no longer significant. Hence, the counter-cyclicality that was found for the full sample must be due to the remaining OECD countries. Indeed, for the non-EU sub-sample the output gap is significantly negative and more than twice as large than for the full sample. Significance is weaker though than for the full sample, probably because of the smaller number of observations. Likely for the same reason the election dummy, while still entering with a coefficient of the "right" sign, is no longer significant.

[7] In the sequel we will refer to a variable as "significant", if it is significant at the 10% confidence level.
[8] In Giuliodori and Beetsma (2007), for a very similar specification, we do extensive robustness checks, such as excluding fixed effects and including additional economic and political variables, but without any effect on the results.

We are also interested in seeing whether the fiscal authorities react differently to forecasts of negative versus positive output gaps. Therefore, we define two new variables $YN_{it}^{t-1} = Y_{it}^{t-1}$, if $Y_{it}^{t-1} < 0$, and $YN_{it}^{t-1} = 0$, otherwise, and $YP_{it}^{t-1} = Y_{it}^{t-1}$, if $Y_{it}^{t-1} > 0$, and $YP_{it}^{t-1} = 0$, otherwise. The results for this split are reported in Table 2. Estimation for the full sample suggests countercyclical planning when the output gap forecast is positive and a-cyclical planning when it is negative. This is consistent with the findings of Cimadomo (2007), who uses the same sample, but a slightly different specification. If we split the country-sample into EU and non-EU countries, none of the estimates of the coefficients on the output gap forecasts remain significant. The coefficient of YP_{it}^{t-1} is negative and quite close to significance for the non-EU subsample, suggesting that the number of degrees of freedom has become too low to formally test for counter-cyclicality at conventional confidence levels.

A potential objection to our approach of estimating planned fiscal policy could be that the forecasts of the cyclically-adjusted primary deficit or the output gap are systematically biased for some (maybe political) reason (e.g., Brück and Stephan, 2006, and Pina and Venes, 2007). If such a bias were constant at the country level, because governments actually plan a cyclically-adjusted primary deficit that exceeds its *reported* forecast by a fixed amount, we would expect it to feed into the estimate of the country-fixed effect. Because the OECD processes the data, this might reduce the bias. After all, the OECD applies its own judgment to the plausibility of the forecasts. Nevertheless, we can check whether there is indeed such a bias by testing statistically whether the reported forecasts deviate from the corresponding first release observations. We also check whether the data from the last available release deviate in a systematic way from the reported forecasts. However, as argued above, first-release observations are best able to capture the real-time fiscal response to real-time changes in the information set. A potential complication is that changes in the data construction methodology between the moments of publication of a forecast and its realization may undermine the validity of the test. Given that for first release data these moments are just one year apart, we might expect the effect of potential changes in methodology to be limited.

In order to address the importance of potential systematic projection biases and methodological changes, we first define $D\_DEF_{it}^{t,t-1} \equiv DEF_{it}^{t} - DEF_{it}^{t-1}$ and $D\_Y_{it}^{t,t-1} \equiv Y_{it}^{t} - Y_{it}^{t-1}$. Hence, $D\_DEF_{it}^{t,t-1}$ ($D\_Y_{it}^{t,t-1}$) is the deviation of the *first-release* primary cyclically-adjusted deficit (output gap) from its original forecast. The corresponding forecast errors based on the *final* release are constructed as $D\_DEF_{it}^{f,t-1} \equiv DEF_{it}^{f} - DEF_{it}^{t-1}$ and $D\_Y_{it}^{f,t-1} \equiv Y_{it}^{f} - Y_{it}^{t-1}$, where DEF_{it}^{f} (Y_{it}^{f}) is the primary cyclically-adjusted deficit (output gap) for year *t* published in the *latest* vintage of the OECD Economic Outlook. Table 3 reports for each country the means over time of the

forecast errors (and their statistical significance). It is interesting to notice that the means of the errors based on the first-release data are only statistically significant in one instance, indicating that there does not seem to be evidence of a systematic bias into one direction. This suggests that systematic biases of the type discussed above play little or no role. However, the means of $D\_DEF_{it}^{f,t-1}$ and $D\_Y_{it}^{f,t-1}$ are in many instances significant. This suggests that ex-post fiscal data are unsuitable for inferring the intentions of fiscal policymakers. Indeed, Cimadomo (2007) shows that these large forecast errors in the fiscal stance and the output gap are the source of the difference in cyclicality of fiscal policy found by the literature using real-time rather than ex-post data.

One (indirect) way to address the potential problem of methodological changes is to regress forecast errors on their "lags". In particular, for the first-release data we regress $D\_Y_{it}^{t,t-1}$ on $D\_Y_{i,t-1}^{t-1,t-2} \equiv Y_{i,t-1}^{t-1} - Y_{i,t-1}^{t-2}$, and for the final release data we regress $D\_Y_{it}^{f,t-1}$ on $D\_Y_{i,t-1}^{f,t-2} = Y_{i,t-1}^{f} - Y_{i,t-1}^{t-2}$. We perform similar regressions for the cyclically-adjusted primary deficit. Table 4 reports the results, which show that the final release forecasts errors of both the output gap and the cyclically-adjusted primary deficit show very high (positive) persistence, while the first-release forecast errors of the output gap exhibit some negative persistence and the first-release forecast errors of the cyclically-adjusted primary deficit reveal no persistence.

What explains the significance in the persistence patterns? As far as the first-release forecast errors are concerned, we notice that Y_{it}^{t-1} and $Y_{i,t-1}^{t-1}$ are from the same EO vintage. Variables Y_{it}^{t-1} and $Y_{i,t-1}^{t-1}$ move together, presumably because of common methodologies or because new information becoming available in year *t*-1 is judged to be relevant not only for year *t*-1, but also for year *t*. Hence, given $Y_{i,t-1}^{t-2}$ and Y_{it}^{t}, if $Y_{i,t-1}^{t-1}$ rises (thereby raising $D\_Y_{i,t-1}^{t-1,t-2}$), it is likely that Y_{it}^{t-1} rises as well (thereby lowering $D\_Y_{it}^{t,t-1}$). This explains the negative persistence in the first-release forecast errors for the output gap. The significance in the persistence parameter drops to only 10% if we leave out the country-fixed effects (as is econometrically favoured – see Table 4, column 2) and it disappears entirely when we split the country sample into EU and non-EU countries (not reported here). As regards to the final release data, we observe that Y_{it}^{f} and $Y_{i,t-1}^{f}$ are from the same EO vintage and highly correlated. Hence, for given Y_{it}^{t-1} and $Y_{i,t-1}^{t-2}$, if Y_{it}^{f} rises, then it is likely that $Y_{i,t-1}^{f}$ also rises, thereby explaining the positive co-movement between $D\_Y_{it}^{f,t-1}$ and $D\_Y_{i,t-1}^{f,t-2}$.

4. The fiscal implementation stage

Having analysed planned fiscal policy we shall now turn to the real-time responses of fiscal policy to economic developments that take place after planned policy has been formed. While in principle governments are constrained by their plans as incorporated in the annual budget law, based on the results from an OECD questionnaire on budgeting practices and procedures (OECD, 2008) we find that in virtually all countries in our sample the government may deviate (sometimes subject to approval requirements) from the original plan by increasing public spending. Most prevalent is the possibility to raise mandatory spending once the budget has been approved by the legislature. Moreover, the vast majority of the countries in our sample are also allowed to raise discretionary spending after budget approval.

We start our analysis of deviations (or "updates") from plans with the following regression:

$$D\_DEF_{it}^{t,t-1} = \hat{c}_i + \hat{\theta}_t + \hat{\alpha} D\_Y_{it}^{t,t-1} + \hat{\beta}' \mathbf{z}_{it} + \hat{u}_{it}, \qquad (3)$$

where $\hat{c}_i$ and $\hat{\theta}_t$ are the country-, respectively time-fixed, effects. Hence, the left-hand side is the "update" of the fiscal stance, while the right-hand side contains the "update" $D\_Y_{it}^{t,t-1}$ of the output gap. The set of controls z_{it} now includes $D\_DEF_{i,t-1}^{t,t-1}$, DEF_{it}^{t-1}, $NONACTIVE_{it}$, $ELECT_{it}$, $D\_M_{i,t-1}^{t,t-1}$ and $D\_SGP_{i,t-1}^{t,t-1}$. Here, $D\_DEF_{i,t-1}^{t,t-1} \equiv DEF_{i,t-1}^{t} - DEF_{i,t-1}^{t-1}$, $D\_M_{i,t-1}^{t,t-1} \equiv M_{i,t-1}^{t} - M_{i,t-1}^{t-1}$ and $D\_SGP_{i,t-1}^{t,t-1} \equiv SGP_{i,t-1}^{t} - SGP_{i,t-1}^{t-1}$.[9] Variable $D\_DEF_{i,t-1}^{t,t-1}$ is included to account for new information on the fiscal stance in period *t*-1 becoming available only during period *t*. It is also included to account for potential methodological changes during period *t* that affect the calculation of the cyclically adjusted deficit in *t*-1. While those methodological changes would affect the figure for $D\_DEF_{it}^{t,t-1}$, they are unlinked to new economic information becoming available in *t* and should therefore not affect the coefficients of the other variables in the regression. We include DEF_{it}^{t-1}, the dependent variable in the planning stage, to explore the relationship between the two stages of the budgetary process. In particular, it is conceivable that more ambitious fiscal plans (i.e. lower DEF_{it}^{t-1}) tend to produce implementation errors. In addition, the term helps to control for the potential inefficiency of the forecasts made in period *t*-1. Its significance may indicate that these forecasts have not exploited all information that was available at the time they were made.

[9] We define $M_{i\tau}^{\tau+1} \equiv \left(DE_{i\tau}^{\tau+1} - 3\%\right)/\left(1997 - \tau\right)$, if $DE_{i\tau}^{\tau+1} > 3\%$, $\tau < 1997$ (for Greece, $\tau < 1999$) and *i* is currently in the Euro-area; and $M_{i\tau}^{\tau+1} \equiv 0$, otherwise. Here, "*DE*" stands for the actual (i.e., not cyclically adjusted) deficit. Further, $SGP_{i\tau}^{\tau+1} \equiv \left(DE_{i\tau}^{\tau+1} - 3\%\right)/2$, if $DE_{i\tau}^{\tau+1} > 3\%$, $\tau \geq 1997$ (for Greece, $\tau \geq 1999$) and *i* is currently in the Euro-area; and $SGP_{i\tau}^{\tau+1} \equiv 0$, otherwise.

Again, we present the results for the full set of countries and for the two sub-samples of EU and non-EU countries. Table 5 reports the estimates obtained by OLS. First, we consider columns 1, 3 and 5 where both country-fixed and time-effects are included. For the complete country-sample the estimates suggest on average a pro-cyclical reaction of the update in the fiscal stance to the output gap update. A split into EU and non-EU countries indicates that the EU countries are responsible for this finding. For the EU sample, the coefficient on the output gap update increases in size, while for the other group it becomes insignificant. This suggests that the EU have followed pro-cyclical policy changes in response to unexpected developments in the business cycle, while the other countries have on average not responded to such unexpected developments.

The difference in responses between the two stages, moreover, indicates that it is important to empirically distinguish the two stages, for example if the objective is to find institutional solutions that limit the pro-cyclicality of fiscal policy. Efforts in this direction seem to be better targeted at the implementation stage than at the planning stage.

As regards the control variables, we see that DEF_{it}^{t-1} enters significantly for the full sample and for the EU subsample, but not for the non-EU subsample. The estimates suggest that a more ambitious fiscal plan is on average met with a larger (adverse) implementation errors. Apparently, it is easier (probably for political reasons) to present an ambitious plan than to stick to it. For all three (sub) samples $D\_DEF_{i,t-1}^{t,t-1}$ enters with a large, highly significant coefficient. This is consistent with the fact that, being from the same vintage (and, hence, subject to the same information or methodological updating), both DEF_{it}^{t} and $DEF_{i,t-1}^{t}$, and DEF_{it}^{t-1} and $DEF_{i,t-1}^{t-1}$ are highly correlated and, hence, also the differences $D\_DEF_{it}^{t,t-1}$ and $D\_DEF_{i,t-1}^{t,t-1}$ are highly correlated. The electoral dummies are no longer significant, suggesting that they play a greater role in planned fiscal policy than in the policy update. Finally, also the updates of the Maastricht and the SGP variables are insignificant.

We have re-estimated all three variants excluding the country-fixed effects. In a dynamic panel, including those fixed effects could bias the results. Although strictly speaking we do not have a dynamic panel, because $D\_DEF_{i,t-1}^{t,t-1}$ is formally not a lag of $D\_DEF_{it}^{t,t-1}$, we nevertheless want to check the implications of leaving out the fixed effects. We report the results in columns 2, 4 and 6 of Table 5. Qualitatively, the results are largely unaffected with $D\_DEF_{i,t-1}^{t,t-1}$ still entering with a high coefficient and, most importantly, $D\_Y_{it}^{t,t-1}$ still entering with unchanged, highly significant, coefficients in the cases of the full sample and the EU sample. The main difference is

that DEF_{it}^{t-1} is no longer significant in any of the cases. In view of these results, we continue with the specification with both the time- and country-fixed effects.

The OLS regression suffers from two potential shortcomings. First, economic reasoning would suggest a potential feedback effect from the update of the fiscal stance, $D\_DEF_{it}^{t,t-1}$, to the update of the output gap, $D\_Y_{it}^{t,t-1}$. This would require the use of instrumental variables. A second potential complication is that the components of the variables $D\_DEF_{it}^{t,t-1}$ and $D\_Y_{it}^{t,t-1}$ are obtained from different vintages of the EO. If data construction methodologies change from vintage to vintage, then this will introduce measurement error into the regression. Also here the solution is to use instrumental variables. The only variable that we need to instrument is $D\_Y_{it}^{t,t-1}$. The instruments correspond to those that we have employed for the estimation of the fiscal plans. Specifically, we take as instruments $D\_Y_{i,t-1}^{t,t-1} \equiv Y_{i,t-1}^{t} - Y_{i,t-1}^{t-1}$, $D\_YW_{it}^{t,t-1} \equiv YW_{it}^{t} - YW_{it}^{t-1}$, $D\_IRSW_{it}^{t,t-1} \equiv IRSW_{it}^{t} - IRSW_{it}^{t-1}$ and $D\_IRLW_{it}^{t,t-1} \equiv IRLW_{it}^{t} - IRLW_{it}^{t-1}$. Thus, the last three variables are the GDP-weighted averages of the updates of, respectively, the output gap, the short-run interest rate and the long-run interest rate for all countries other than *i*. Because $D\_IRSW_{it}^{t,t-1}$ and $D\_IRLW_{it}^{t,t-1}$ are financial variables, they should be relatively free from methodological complications. Table 6 reports the results of the IV regressions. The results basically confirm those for our OLS estimates. The coefficient of the output gap update ($D\_Y_{it}^{t,t-1}$) remains significantly positive for the EU countries (column 2), indicating pro-cyclical fiscal responses to new information on the business cycle, and insignificant for the other group (column 3). Now, however, it is no longer significant for the full country sample (column 1). We have also redone the IV estimations dropping the country-fixed effects. For the sake of space, we only report these estimates in Table A.2 of the Additional Appendix (not for publication). The output gap update remains insignificant for the full country sample, while it is again significantly positive (with roughly the same coefficient as before) for the EU sub-sample. Quite interestingly, its negative coefficient for the non-EU sub-sample now becomes significant, suggesting that for these countries fiscal stances have responded counter-cyclically to the output gap updates.

A limitation of using instrumental variables is that one needs to find suitable instruments. Some experimenting showed that by far the most important instrument for $D\_Y_{it}^{t,t-1}$ is $D\_Y_{i,t-1}^{t,t-1}$.

That is, dropping the other instruments yields virtually the same estimates.[10] This, in turn, implies that what we estimate is basically the response of the fiscal stance update $D\_DEF_{it}^{t,t-1}$ to new information on the output gap in period *t*-1 that becomes only available in period *t*. We now employ an alternative two-stage method that in effect enables us to estimate the response of $D\_DEF_{it}^{t,t-1}$ to new information on the output gap in period *t*, while also allowing us to take account of potential methodological changes. We use this mainly as an indirect check of our results, because this approach cannot completely solve the potential endogeneity problem of the regressors, while, moreover, it ignores the effects of new information on the output gap in period *t*-1 that becomes available only in period *t*.

The first stage of our alternative approach consists of running the regression:

$$D\_Y_{it}^{t,t-1} = \overline{c}_i + \overline{\theta}_t + \overline{\gamma} D\_Y_{i,t-1}^{t,t-1} + \overline{u}_{it}, \qquad (4)$$

where $D\_Y_{i,t-1}^{t,t-1} \equiv Y_{i,t-1}^{t} - Y_{i,t-1}^{t-1}$. We retain the fitted residuals $RESY_{it}$ from this regression for the next stage. The idea behind this approach is that potential methodological changes from year *t*-1 to year *t* that plague $D\_Y_{it}^{t,t-1}$ would also be present in the variable $D\_Y_{i,t-1}^{t,t-1}$, so that the residuals $RESY_{it}$ would be filtered, at least for a substantial part, from such methodological changes. Importantly, however, if $D\_Y_{i,t-1}^{t,t-1}$ differs from zero, then this may not only capture methodological updating, but it could also be the result of new information on the business cycle in period *t*-1 becoming available only with a lag.

Table 7 reports the estimates of equation (4) for the three country (sub)samples. For the complete sample, the term $D\_Y_{i,t-1}^{t,t-1}$ is highly significant with a coefficient of 0.67. If we split the country sample into EU and non-EU countries, the outcomes for the two groups are very different. For the EU countries the coefficient increases to 0.82 and remains highly significant, while for the non-EU group it drops to 0.19 and it loses its significance.

For the second stage we estimate:

$$D\_DEF_{it}^{t,t-1} = \tilde{c}_i + \tilde{\theta}_t + \tilde{\alpha} RESY_{it} + \tilde{\beta}' z_{it} + \tilde{u}_{it}. \qquad (5)$$

[10] By including time dummies we implicitly account for all country-invariant common factors that drive the instrumented variables. Therefore, it is not surprising that the role of the instruments $D\_YW_{it}^{t,t-1}$, $D\_IRSW_{it}^{t,t-1}$ and $D\_IRLW_{it}^{t,t-1}$ is very limited.

Clearly, as discussed, with the specification in (5) the potential reaction in period *t* to new information on the economy *before* period *t* will be ignored, because this information is contained in the variable $D\_Y_{i,t-1}^{t,t-1}$, which is orthogonal to $RESY_{it}$. Obviously, we would not want to miss out on the fiscal reaction to this piece of new information. However, this is the price we have to pay for eliminating, or at least reducing, the impact of potential methodology changes on our estimates.

Table 8 shows the results of the estimation of (5). The residual of (4), $RESY_{it}$, enters with a significantly positive coefficient in the full country sample and, after splitting the sample, it also enters with a significantly positive coefficient for the EU countries, but it is insignificant for the non-EU countries. These findings are in line with our earlier result that fiscal policy updates in the former group behave pro-cyclically, while there is no cyclical effect in the fiscal policy updates of the latter group. Now, however, we only measure the pro-cyclical fiscal response to new information about the economy in period *t*.

As a final alternative, we use data from the June versions of the Economic Outlook. We run the following OLS regression analogous to (3) and (5):

$$D\_DEF_{it}^{t,t-1} = \hat{c}_i + \hat{\theta}_t + \hat{\alpha} D\_Y_{it}^{t-\frac{1}{2},t-1} + \hat{\beta}' z_{it} + \hat{u}_{it}, \qquad (6)$$

where superscript "$t-\frac{1}{2}$" denotes June of year *t*. Here, $D\_Y_{it}^{t-\frac{1}{2},t-1} \equiv Y_{it}^{t-\frac{1}{2}} - Y_{it}^{t-1}$, which is defined analogously to $D\_Y_{it}^{t,t-1}$. Hence, $D\_Y_{it}^{t-\frac{1}{2},t-1}$ is the difference between the June period *t* estimate of the output gap in period *t* and the December period *t*-1 forecast for the output gap in period *t*. The advantage of this specification is that we can expect this regression to be less vulnerable to *both* potential endogeneity and methodological updating problems. If the feedback from an update of the fiscal stance onto the output gap takes at least half a year, any potential endogeneity vanishes completely. Table 9 shows the estimates for our three (sub-) samples. Variable $D\_Y_{it}^{t-\frac{1}{2},t-1}$ is insignificant for the full country sample (although it is close to significance at the 10% level), presumably reflecting the fact that the update of the fiscal stance contains only a limited amount of new information (only what becomes available since the December release of the previous year) to react to. However, for the EU sample the output gap update is again significant, with a coefficient estimate that is virtually equal to that of $D\_Y_{it}^{t,t-1}$ in (3).[11]

[11] As a robustness check, we included (one at a time) some additional controls to this regression. One was a dummy to distinguish between "delegation" versus "commitment" forms of fiscal governance (see Hallerberg, 2004, and Annett, 2006). Under the former, the Finance Minister has a leading role in the budgeting process. The latter form of governance is characterised by a fiscal contract between government parties involving strict budget targets. Two further controls, from the Comparative Political Dataset (2007), measure the cabinet composition on a left-to-right scale,

As in the case of planned policy, we are interested in seeing whether the fiscal authorities react differently to negative versus positive output gap updates. We explore the effects of this split only for our most preferred regressions, which are the IV regressions that we reported in Table 7 and the June regressions in Table 9. For the IV estimates we define the following two new variables $D\_YN_{it}^{t,t-1} = D\_Y_{it}^{t,t-1}$, if $D\_Y_{it}^{t,t-1} < 0$, and $D\_YN_{it}^{t,t-1} = 0$, otherwise; and $D\_YP_{it}^{t,t-1} = D\_Y_{it}^{t,t-1}$, if $D\_Y_{it}^{t,t-1} > 0$, and $D\_YP_{it}^{t,t-1} = 0$, otherwise. For the June estimates we define the corresponding new variables $D\_YN_{it}^{t-\frac{1}{2},t-1} = D\_Y_{it}^{t-\frac{1}{2},t-1}$, if $D\_Y_{it}^{t-\frac{1}{2},t-1} < 0$, and $D\_YN_{it}^{t-\frac{1}{2},t-1} = 0$, otherwise; and $D\_YP_{it}^{t-\frac{1}{2},t-1} = D\_Y_{it}^{t-\frac{1}{2},t-1}$, if $D\_Y_{it}^{t-\frac{1}{2},t-1} > 0$, and $D\_YP_{it}^{t-\frac{1}{2},t-1} = 0$, otherwise. Table 10 presents the new results, which are qualitatively the same for the IV and June estimates. In both cases the EU countries exhibit is a significant pro-cyclical reaction to negative output gap updates and an a-cyclical reaction to positive output gap updates. This finding corresponds to the rather common perception that the EU countries undertake too little effort to exert discipline in good times, which forces them to contract fiscal policy in bad times.[12] For the non-EU countries, responses to both positive and negative output gap updates are a-cyclical. Of course, these findings may also be an indication of insufficient observations when the output gaps are split into positive and negative values.

5. Concluding remarks and policy implications

In this paper we have used a new approach to analysing fiscal policy behavior. We have split the analysis of fiscal policy into an analysis of planned fiscal policy and an analysis of deviations from planned policy in response to new information becoming available since fiscal plans were made. We find that planned fiscal policy in response to output gap forecasts is acyclical for the EU countries, but counter-cyclical for the non-EU countries. Moreover, for the EU sub-sample the planned fiscal stance is relaxed for election years and tightened in response to violations of the Maastricht and SGP reference deficit levels. As regards to the deviations from plans, for the EU we find that fiscal policy reacts pro-cyclically to new information about the business cycle. No such

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respectively the strength of the government in parliament. None of the three controls came out significantly, nor did their inclusion affect the estimates of the coefficients of the other variables. This contrasts with Brück and Stephan (2006) and Pina and Venes (2007), who find a greater role for political variables, although for revisions in cyclically *un*adjusted fiscal balances. We also included the update of the short-run interest rate to control for unexpected changes in monetary policy and we included an index of national fiscal rules (generously provided by Alessandro Turrini – see Debrun et al., 2008) both alone and interacted with the output gap update. Also, none of these additional controls came out significantly, nor did they affect the estimates of the other variables.

[12] Wierts' (2008) results suggest that this asymmetry is linked to a downward correction of public expenditures when output conditions are worse than expected.

cyclicality is found for the non-EU sub-sample. Further investigation suggests that the cyclicality for the EU sub-sample arises from a pro-cyclical response to a worsening of the business cycle, while an improvement in the business cycle does not lead to any systematic fiscal reaction. This corresponds to the rather common observation that EU governments generally fail to tighten fiscal policy during good times, which forces them into contraction when the business cycle deteriorates. Finally, in going from plans to deviations from plans, the election dummy and the Maastricht and SGP variables all lose their significance.

We believe that our findings point to several general lessons. First, the differences in the responses to the economic environment between the two fiscal policy stages show that it is important to empirically distinguish these stages, as this provides valuable information for the design of institutional solutions that limit the pro-cyclicality of fiscal policy. For example, Europe's SGP emphasizes the avoidance of fiscal pro-cyclicality at the planning stage through its Stability and Convergence Programs. Second, our results indicate that it is crucial to explore both stages in connection with each other. We found that more ambitious plans tend to be followed by larger implementation errors. Hence, properly scrutinizing fiscal policy at the planning stage is important as overambitious plans may produce wrong expectations of private sector agents and, therefore unnecessary volatility in public debt markets. Also, the private sector may take decisions on the basis of planned fiscal policies and such decisions may be costly to change or to reverse. For the same reasons discretionary corrections to unexpected changes in the economy's performance should be avoided. Our estimates suggest that such corrections do occur on a rather substantial scale for the EU countries and that, most unfortunate of all, they take the form of pro-cyclical contractions in response to an unforeseen fall in economic activity.

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TABLES

Table 1: Estimates of planned fiscal behaviour

	Dependent variable:		
	(1)	(2)	(3)
	All countries	EU-14	Non-EU
	DEF_{it}^{t-1}	DEF_{it}^{t-1}	DEF_{it}^{t-1}
$DEF_{i,t-1}^{t-1}$	0.82***	0.73***	0.92***
	(0.032)	(0.042)	(0.056)
Y_{it}^{t-1}	-0.096**	0.047	-0.20*
	(0.047)	(0.053)	(0.11)
$DEBT_{i,t-1}^{t-1}$	0.0074	0.0065	-0.0016
	(0.0046)	(0.0060)	(0.011)
$NONACTIVE_{it}$	-0.075	-0.0006	0.051
	(0.078)	(0.087)	(0.24)
$ELECT_{it}$	0.23**	0.20**	0.22
	(0.090)	(0.10)	(0.17)
$M_{i,t-1}^{t-1}$	-0.40***	-0.39***	-
	(0.11)	(0.11)	
$SGP_{i,t-1}^{t-1}$	-1.32***	-1.04***	-
	(0.26)	(0.27)	
Estimation method	IV	IV	IV
Fixed Effects	country, time	country, time	country, time
R^2 - within	0.90	0.89	0.96
Sargan test	0.12	0.51	0.47
(p-value)	(0.99)	(0.92)	(0.93)
Sample period	1995-2006	1995-2006	1995-2006
No. of observations	228	168	60

Notes: (1) Robust standard errors are in brackets below the point estimates, * = significance at the 10% level, ** = significance at the 5% level, *** = significance at the 1% level. (2) Instruments for Y_{it}^{t-1} are $Y_{i,t-1}^{t-1}$, YW_{it}^{t-1}, $IRSW_{it}^{t-1}$ and $IRLW_{it}^{t-1}$. (3) The Sargan test assesses the validity of the instruments by testing their exogeneity.

Table 2: Planned fiscal behaviour with positive-negative output gap split

	Dependent variable:		
	(1)	(2)	(3)
	All countries	EU-14	Non-EU
	DEF_{it}^{t-1}	DEF_{it}^{t-1}	DEF_{it}^{t-1}
$DEF_{i,t-1}^{t-1}$	0.81***	0.72***	0.91***
	(0.033)	(0.043)	(0.061)
YN_{it}^{t-1}	-0.0085	0.11	-0.058
	(0.062)	(0.068)	(0.14)
YP_{it}^{t-1}	-0.30***	-0.092	-0.51
	(0.11)	(0.11)	(0.32)
$DEBT_{i,t-1}^{t-1}$	0.0056	0.0053	-0.0019
	(0.0048)	(0.0060)	(0.011)
$NONACTIVE_{it}$	-0.060	0.0088	0.014
	(0.078)	(0.083)	(0.25)
$ELECT_{it}$	0.22**	0.20*	0.24
	(0.090)	(0.10)	(0.18)
$M_{i,t-1}^{t-1}$	-0.35***	-0.35***	-
	(0.11)	(0.11)	
$SGP_{i,t-1}^{t-1}$	-1.12***	-0.91***	-
	(0.27)	(0.28)	
Estimation method	IV	IV	IV
Fixed Effects	country, time	country, time	country, time
R^2 - within	0.90	0.90	0.95
Sargan test	0.11	0.41	0.39
(p-value)	(0.99)	(0.94)	(0.94)
Sample period	1995-2006	1995-2006	1995-2006
No. of observations	228	168	60

Notes: (1) Robust standard errors are in brackets below the point estimates, * = significance at the 10% level, ** = significance at the 5% level, *** = significance at the 1% level. (2) Instruments for YN_{it}^{t-1} and YP_{it}^{t-1} are $YN_{i,t-1}^{t-1}$, $YP_{i,t-1}^{t-1}$, YW_{it}^{t-1}, $IRSW_{it}^{t-1}$ and $IRLW_{it}^{t-1}$. (3) The Sargan test assesses the validity of the instruments by testing their exogeneity.

Table 3: Forecast errors

	$D\_DEF_{it}^{t,t-1}$	$D\_DEF_{it}^{f,t-1}$	$D\_Y_{it}^{t,t-1}$	$D\_Y_{it}^{f,t-1}$
Australia	-0.29	-1.20***	0.08	-0.19
Austria	-0.11	0.20	-0.08	0.40
Belgium	-0.19	-0.27	-0.03	0.43*
Canada	-0.12	0.49	0.11	0.36
Denmark	-0.15	-0.32	0.02	0.25
Finland	-0.12	-1.00***	-0.09	-1.02
France	0.14	0.32	-0.06	0.00
Germany	0.33	0.88***	-0.11	1.02***
Greece	0.37	2.60***	0.02	-0.52**
Ireland	0.18	-0.38	0.69	0.08
Italy	0.24	1.14**	-0.01	1.62***
Japan	0.45	1.03**	0.35	1.87***
Netherlands	0.07	0.33	0.18	0.40**
Norway	-0.10	0.35	0.33	0.92**
Portugal	0.42	1.50***	0.00	1.24***
Spain	0.05	0.05	0.17	-0.08
Sweden	-0.85**	-0.67	-0.10	-0.79*
United Kingdom	0.16	0.18	0.23	0.30
United States	-0.15	-0.45	0.18	-0.45*

Notes: the table reports the mean of the forecast errors for each given country over the sample period (1995-2006). * = significance at the 10% level, ** = significance at the 5% level, *** = significance at the 1% level. The confidence intervals are calculated as $\pm c.v.*\left(\sigma/\sqrt{n}\right)$, where *c.v.* = critical value of the standard normal distribution, σ = sample standard deviation, and *n* = number of observations.

Table 4: Serial correlation in forecast errors

Dependent	$D\_Y_{it}^{t,t-1}$		$D\_Y_{it}^{f,t-1}$		$D\_DEF_{it}^{t,t-1}$		$D\_DEF_{it}^{f,t-1}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$D\_Y_{i,t-1}^{t-1,t-2}$	-0.14* (0.071)	-0.18** (0.073)	-	-	-	-	-	-
$D\_Y_{i,t-1}^{f,t-2}$	-	-	0.60*** (0.052)	0.50*** (0.063)	-	-	-	-
$D\_DEF_{i,t-1}^{t-1,t-2}$	-	-	-	-	0.12 (0.078)	0.068 (0.085)	-	-
$D\_DEF_{i,t-1}^{f,t-2}$	-	-	-	-	-	-	0.52*** (0.063)	0.39*** (0.069)
Fixed Effects	N	Y	N	Y	N	Y	N	Y
Time Effects	Y	Y	Y	Y	Y	Y	Y	Y
H_0: redundant fixed effects (p-value)	-	0.91 (0.58)	-	1.22 (0.24)	-	0.71 (0.79)	-	1.97** (0.02)
H_0: redundant time effects (p-value)	4.72*** (0.00)	4.62*** (0.00)	9.30*** (0.00)	8.80*** (0.00)	4.16*** (0.00)	4.17*** (0.00)	4.72*** (0.00)	7.08*** (0.00)
R^2 - within	0.25	0.26	0.48	0.48	0.20	0.20	0.47	0.48
Sample period	1995-2006	1995-2006	1995-2006	1995-2006	1995-2006	1995-2006	1995-2006	1995-2006
No. of obs.	228	228	228	228	228	228	228	228

Notes: Robust standard errors are in brackets below the point estimates, * = significance at the 10% level, ** = significance at the 5% level, *** = significance at the 1% level.

Table 5: OLS estimates of deviations from plans

	Dependent variable: $D\_DEF_{it}^{t,t-1}$					
	(1)	(2)	(3)	(4)	(5)	(6)
	All countries	All countries	EU-14	EU-14	Non-EU	Non-EU
$D\_Y_{it}^{t,t-1}$	0.14**	0.14**	0.22***	0.21***	-0.012	-0.020
	(0.068)	(0.069)	(0.077)	(0.077)	(0.18)	(0.17)
$D\_DEF_{i,t-1}^{t,t-1}$	0.83***	0.91***	0.64***	0.71***	1.15***	1.22***
	(0.070)	(0.067)	(0.078)	(0.077)	(0.14)	(0.11)
DEF_{it}^{t-1}	-0.15***	-0.018	-0.17***	-0.041	-0.068	0.015
	(0.041)	(0.020)	(0.053)	(0.027)	(0.073)	(0.035)
$NONACTIVE_{it}$	-0.069	-0.043	-0.11	-0.087**	0.052	0.053
	(0.091)	(0.038)	(0.12)	(0.041)	(0.15)	(0.088)
$ELECT_{it}$	0.030	0.0020	0.13	0.12	0.047	0.022
	(0.11)	(0.11)	(0.12)	(0.12)	(0.26)	(0.25)
$D\_M_{i,t-1}^{t}$	0.066	-0.21	0.48	0.16	-	-
	(0.48)	(0.46)	(0.44)	(0.43)		
$D\_SGP_{i,t-1}^{t}$	0.45	-0.25	0.95	0.15	-	-
	(0.72)	(0.66)	(0.67)	(0.61)		
Estimation method	OLS	OLS	OLS	OLS	OLS	OLS
Fixed Effects	Y	N	Y	N	Y	N
Time Effects	Y	Y	Y	Y	Y	Y
R^2 - within	0.70	0.68	0.68	0.66	0.82	0.82
Sample period	1995-2006	1995-2006	1995-2006	1995-2006	1995-2006	1995-2006
No. of observations	228	228	168	168	60	60

Notes: Robust standard errors are in brackets below the point estimates, * = significance at the 10% level, ** = significance at the 5% level, *** = significance at the 1% level.

Table 6: IV estimates of deviations from plans

	Dependent variable: $D\_DEF_{it}^{t,t-1}$		
	(1)	(2)	(3)
	All countries	EU-14	Non-EU
$D\_Y_{it}^{t,t-1}$	0.043 (0.096)	0.21** (0.089)	-0.44 (0.29)
$D\_DEF_{i,t-1}^{t,t-1}$	0.85*** (0.073)	0.64*** (0.083)	1.13*** (0.15)
DEF_{it}^{t-1}	-0.15*** (0.041)	-0.17*** (0.053)	-0.046 (0.080)
$NONACTIVE_{it}$	-0.064 (0.091)	-0.11 (0.12)	0.026 (0.17)
$ELECT_{it}$	0.023 (0.11)	0.13 (0.12)	0.17 (0.28)
$D\_M_{i,t-1}^{t}$	0.019 (0.49)	0.47 (0.44)	-
$D\_SGP_{i,t-1}^{t}$	0.39 (0.74)	0.94 (0.67)	-
Estimation method	IV	IV	IV
Fixed Effects	Y	Y	Y
Time Effects	Y	Y	Y
R^2 - within	0.69	0.68	0.80
Sargan test (p-value)	0.14 (0.98)	0.003 (1.00)	0.36 (0.95)
Sample period	1995-2006	1995-2006	1995-2006
No. of observations	228	168	60

Notes: (1) Robust standard errors are in brackets below the point estimates, * = significance at the 10% level, ** = significance at the 5% level, *** = significance at the 1% level. (2) The instruments for $D\_Y_{it}^{t,t-1}$ are $D\_Y_{i,t-1}^{t,t-1}$, $D\_YW_{it}^{t,t-1}$, $D\_IRSW_{it}^{t,t-1}$ and $D\_IRLW_{it}^{t,t-1}$. (3) The Sargan test assesses the validity of the instruments by testing their exogeneity.

Table 7: Estimates of deviations from plans – first stage

	Dependent variable: $D\_Y_{it}^{t,t-1}$		
	(1)	(2)	(3)
	All countries	EU-14	Non-EU
$D\_Y_{i,t-1}^{t,t-1}$	0.67*** (0.070)	0.82*** (0.071)	0.19 (0.18)
Estimation method	OLS	OLS	OLS
Fixed Effects	Y	Y	Y
Time Effects	Y	Y	Y
R^2 - within	0.53	0.64	0.54
Sample period	1995-2006	1995-2006	1995-2006
No. of observations	228	168	60

Notes: Robust standard errors are in brackets below the point estimates, * = significance at the 10% level, ** = significance at the 5% level, *** = significance at the 1% level.

Table 8: Estimates of deviations from plans – second stage

	Dependent variable: $D\_DEF_{it}^{t,t-1}$		
	(1)	(2)	(3)
	All countries	EU-14	Non-EU
$RESY_{it}$	0.19** (0.083)	0.23** (0.11)	0.025 (0.18)
$D\_DEF_{i,t-1}^{t,t-1}$	0.86*** (0.068)	0.71*** (0.077)	1.15*** (0.14)
DEF_{it}^{t-1}	-0.15*** (0.041)	-0.17*** (0.054)	-0.069 (0.073)
$NONACTIVE_{it}$	-0.091 (0.093)	-0.12 (0.13)	0.052 (0.15)
$ELECT_{it}$	0.020 (0.11)	0.12 (0.12)	0.036 (0.26)
$D\_M_{i,t-1}^{t}$	0.023 (0.48)	0.34 (0.45)	-
$D\_SGP_{i,t-1}^{t}$	0.49 (0.72)	0.93 (0.67)	-
Estimation method	OLS	OLS	OLS
Fixed Effects	Y	Y	Y
Time Effects	Y	Y	Y
R^2 - within	0.70	0.67	0.82
Sample period	1995-2006	1995-2006	1995-2006
No. of observations	228	168	60

Notes: Robust standard errors are in brackets below the point estimates, * = significance at the 10% level, ** = significance at the 5% level, *** = significance at the 1% level.

Table 9: Estimates using June data

	Dependent variable: $D\_DEF_{it}^{t,t-1}$		
	(1)	(2)	(3)
	All countries	EU-14	Non-EU
$D\_Y_{it}^{t-\frac{1}{2},t-1}$	0.14 (0.085)	0.21** (0.10)	0.039 (0.21)
$D\_DEF_{i,t-1}^{t,t-1}$	0.85*** (0.069)	0.69*** (0.078)	1.15*** (0.14)
DEF_{it}^{t-1}	-0.14*** (0.041)	-0.16*** (0.054)	-0.068 (0.072)
$NONACTIVE_{it}$	-0.064 (0.090)	-0.099 (0.12)	0.052 (0.15)
$ELECT_{it}$	0.0096 (0.11)	0.070 (0.12)	0.038 (0.25)
$D\_M_{i,t-1}^{t}$	0.096 (0.49)	0.52 (0.45)	-
$D\_SGP_{i,t-1}^{t}$	0.38 (0.72)	0.76 (0.67)	-
Estimation method	OLS	OLS	OLS
Fixed Effects	Y	Y	Y
Time Effects	Y	Y	Y
R^2 - within	0.70	0.67	0.82
Sample period	1995-2006	1995-2006	1995-2006
No. of observations	228	168	60

Notes: (1) Robust standard errors are in brackets below the point estimates, * = significance at the 10% level, ** = significance at the 5% level, *** = significance at the 1% level.

Table 10: Estimates of deviations from plans with output gap split

	Dependent variable: $D\_DEF_{it}^{t,t-1}$					
	(1)	(2)	(3)	(4)	(5)	(6)
	All countries	All countries	EU-14	EU-14	Non-EU	Non-EU
$D\_YN_{it}^{t,t-1}$	0.27 (0.42)	-	0.70** (0.34)	-	-0.80 (0.75)	-
$D\_YN_{it}^{t-\frac{1}{2},t-1}$	-	0.27* (0.15)	-	0.44*** (0.17)	-	0.12 (0.42)
$D\_YP_{it}^{t,t-1}$	-0.12 (0.31)	-	-0.15 (0.27)	-	-0.034 (0.87)	-
$D\_YP_{it}^{t-\frac{1}{2},t-1}$	-	0.044 (0.14)	-	0.047 (0.16)	-	-0.0033 (0.28)
$D\_DEF_{i,t-1}^{t,t-1}$	0.85*** (0.074)	0.85*** (0.069)	0.64*** (0.087)	0.69*** (0.076)	1.16*** (0.16)	1.15*** (0.14)
DEF_{it}^{t-1}	-0.15*** (0.043)	-0.14*** (0.041)	-0.16*** (0.054)	-0.15*** (0.053)	-0.023 (0.087)	-0.070 (0.073)
$NONACTIVE_{it}$	-0.076 (0.096)	-0.069 (0.088)	-0.11 (0.13)	-0.12 (0.12)	0.10 (0.22)	0.056 (0.15)
$ELECT_{it}$	0.026 (0.11)	-0.0014 (0.11)	0.16 (0.12)	0.055 (0.12)	0.17 (0.27)	0.026 (0.26)
$D\_M_{i,t-1}^{t}$	0.012 (0.50)	0.090 (0.49)	0.42 (0.47)	0.51 (0.45)	-	-
$D\_SGP_{i,t-1}^{t}$	0.40 (0.75)	0.36 (0.73)	0.89 (0.72)	0.70 (0.67)	-	-
Estimation method	IV	OLS	IV	OLS	IV	OLS
Fixed Effects	Y	Y	Y	Y	Y	Y
Time Effects	Y	Y	Y	Y	Y	Y
R^2 - within	0.65	0.70	0.65	0.66	0.82	0.82
Sargan test (p-value)	0.31 (0.96)	-	0.11 (0.99)	-	0.19 (0.98)	-
Sample period	1995-2006	1995-2006	1995-2006	1995-2006	1995-2006	1995-2006
No. of observations	228	228	168	168	60	60

Notes: (1) Robust standard errors are in brackets below the point estimates, * = significance at the 10% level, ** = significance at the 5% level, *** = significance at the 1% level. (2) The instruments for $D\_YN_{it}^{t,t-1}$ and $D\_YP_{it}^{t,t-1}$ are $D\_YN_{i,t-1}^{t,t-1}$, $D\_YP_{i,t-1}^{t,t-1}$, $D\_YW_{it}^{t,t-1}$, $D\_IRSW_{it}^{t,t-1}$, and $D\_IRLW_{it}^{t,t-1}$. Here, $D\_YN_{i,t-1}^{t,t-1} = D\_Y_{i,t-1}^{t,t-1}$ if $D\_Y_{i,t-1}^{t,t-1} < 0$, and zero otherwise; and $D\_YP_{i,t-1}^{t,t-1} = D\_Y_{i,t-1}^{t,t-1}$ if $D\_Y_{i,t-1}^{t,t-1} > 0$, and zero otherwise. (3) The Sargan test assesses the validity of the instruments by testing their exogeneity.