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ABSTRACT

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

We explore how fiscal policies in the OECD have responded to unexpected information about the economy during the period 1995-2006. In particular, 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. In this second step, we use various approaches in dealing with potential endogeneity and changes in data construction methodology after the ex ante data were released. All variants lead to similar results. There are marked differences between ex-ante behaviour and responses to new information, as well as between fiscal policy of the EU countries and the other OECD countries. In particular, the EU countries react in a pro-cyclical way to unexpected changes in the output gap, while the responses of the other OECD countries are a-cyclical. However, ex ante fiscal policy is a-cyclical for the EU countries and counter-cyclical for the other countries.

JEL Classification: E62 and H60
Keywords: cyclicality, EU, first-release data, fiscal policy, OECD and real-time data

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1. Introduction

In this paper we explore for European Union (EU) and other OECD countries how fiscal policymakers set their plans 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. The split between plans and deviations from plans has hardly been investigated, but it is important, because discretionary corrections to the original plans in response to new information about the business cycle are potentially very costly. The private sector may have taken decisions on the basis of these plans and these decisions may be costly to change.

We analyze fiscal policy in two steps. First, we regress ex ante (i.e., forecast or projected) fiscal stances on the output gap forecast and a number of plausible control variables. In our second step, we explore how fiscal policy deviates from the original plan to respond to new information about the output gap and other variables. At this stage, we face two complications. One is that changes in the output gap may be endogenous to the changes in the fiscal stance. The second complication is that the way the data are constructed may change over time. This would introduce measurement error into our regression model. We address these complications in a variety of ways. All alternative strategies yield similar outcomes.

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. Ex ante 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 ex ante fiscal stance.

Our paper connects in several ways to the existing literature. 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 quite a few articles find that fiscal policy has behaved pro-cyclically during at least part of their sample period. 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 articles all use ex-post (i.e. revised) data. However, an increasing number of authors argue that ex ante or "real-time" data may be more informative about the behavior of the fiscal authorities, because these data are available at the moment a fiscal plan is constructed. By using revised data, which often differ substantially from the ex-ante data, one violates the requirement of explaining fiscal behavior by using only variables that are known when the plan is constructed.

Several papers now use ex ante data for assessing the behavior of fiscal authorities, including Forni and Momigliano (2004), Golinelli and Momigliano (2007), Cimadomo (2007) and Giuliodori and Beetsma (2007). However, closest in spirit to this paper are Brück and Stephan (2006) and Pina and Venes (2007). Both papers are interested in the political determinants of forecast errors in fiscal policy, thereby controlling for economic variables. In contrast to this paper, they do not cyclically adjust fiscal policy. Moreover, they do not split between EU and non-EU countries. Finally, their direct data sources and sample periods are different. 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 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).

Estimation based on ex ante data is not free from criticism either. The eventual sources of the ex ante data are usually the national statistical offices. To the extent that these offices are under the influence of politicians, they may be put under pressure to modify their data. The Treasury uses these data to construct economic and budgetary projections and possible political biases (such as an overly optimistic forecast of potential output) may sneak in.[1] To limit the potential influence of such bias on the results, we use data from the OECD, as do many other contributions, including Galí and Perotti (2003) and Lane (2003). The OECD applies its own judgment to the plausibility of the projections it receives and it includes only those fiscal measures that have been legally enacted by the time the forecast is made. Nevertheless, the eventual OECD projections become available only after consultation rounds with the national authorities. Hence, there would still be room for

[1] 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.

possible political bias. However, OECD data are the best we can use in this regard.[2] Even so, we check that there are no systematic biases in our data.[3]

The use of OECD data 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 EDP notifications), which makes it more likely that the OECD forecasts are indeed based on the definitive plans approved by the national parliaments.[4] Finally, OECD data allow for the use of a larger country sample and include directly the cyclically adjusted fiscal measures.

The remainder of the paper is structured as follows. Section 2 discusses the data and notation. Section 3 analyses ex-ante 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 data and notation

All data (except those on population composition and election years, which are from the Comparative Political Dataset, 2007) are from the various December issues of the OECD Economic Outlook (EO). Each issue of the EO contains "forecasts" of variables for 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". These 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 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

[2] Using OECD data is preferable to using IMF or European Commission data, because the OECD is not involved in formal policy decisions, unlike the other two institutions.
[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).
[4] This is not the case for all countries, because for some of them the fiscal year differs from the calendar year.

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 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.

We adopt the following notational conventions. For a generic variable, X_{it}^{τ} denotes the December of year τ OECD Economic Outlook release of the value 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*.

3. Ex-ante behavior

The baseline specification for ex-ante fiscal behavior is:

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

where c_i is a country-specific constant, θ_t is a year-specific constant (a time fixed effect), *DEF* and *Y*, respectively, denote the cyclically-adjusted primary deficit as a share of GDP and 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" dummy for whether the 3% deficit limit in the period before EMU is exceeded; and $SGP_{i,t-1}^{t-1}$, a Stability and Growth Pact (SGP) dummy for whether the 3% deficit limit is exceeded during EMU membership.[5] Variables $ELECT_{it}$ and

[5] 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, $\tau < 1999$)

$NONACTIVE_{it}$ are denoted without superscript, because they are not from the EO. Moreover, they are realizations rather than forecasts,[6] 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 this 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 mutual peer pressures of countries on each other's fiscal policies – see Giuliodori and Beetsma, 2007) and common changes in the methodology of data construction.[7]

We estimate equation (1) 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 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 ex ante fiscal stance in the sense that most of

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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.

[6] Realizations of $NONACTIVE_{it}$ for 2006 are based on interpolation from preceding observations.

[7] 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.

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.[8] 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 realised 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 ex ante behavior, the fiscal authorities react with a fiscal tightening in the case of such a violation.[9]

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, possibly because of the smaller number of observations. Probably for the same reason the election dummy, while still entering with a coefficient of the "right" sign, is no longer significant.

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.

[8] In the sequel we will refer to a variable as "significant", if it is significant at the 10% confidence level.
[9] 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.

A potential objection to our approach of estimating ex ante 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, the effect of potential changes in methodology should 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.

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 forecasts errors of the output gap show some negative persistence and the first release forecast errors of the cyclically-adjusted primary deficit show 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 thought 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} increases, then it is likely that $Y_{i,t-1}^{f}$ also increases, thereby explaining the positive co-movement between $D\_Y_{it}^{f,t-1}$ and $D\_Y_{i,t-1}^{f,t-2}$.

4. Fiscal responses to new information

Having analysed ex ante fiscal policy we shall now turn to the real-time responses of fiscal policy to economic developments that take place after ex ante policy has been formed.

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

$$D\_DEF_{it}^{t,t-1} = c_i + \theta_t + \gamma D\_Y_{it}^{t,t-1} + \beta' z_{it} + u_{it}. \qquad (2)$$

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 $D\_SGP_{i,t-1}^{t,t-1} \equiv SGP_{i,t-1}^{t} - SGP_{i,t-1}^{t-1}$.[10] 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. Variable DEF_{it}^{t-1} is included on the basis of earlier experimentation with the specification and controls for the potential inefficiency of the forecasts made in period *t*-1. Its significance indicates that these forecasts have not exploited all available information at the time they were made.

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.

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. 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 ex ante 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

[10] 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.

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 each of the variables $D\_DEF_{it}^{t,t-1}$ and $D\_Y_{it}^{t,t-1}$ is constructed from data of two different vintages of the EO. If 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.1 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.[11] 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 the possibility of 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:

$$D\_Y_{it}^{t,t-1} = c_i + \theta_t + \gamma D\_Y_{i,t-1}^{t,t-1} + \varepsilon_{it}, \qquad (3)$$

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 (3) 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:

[11] 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.

$$D\_DEF_{it}^{t,t-1} = c_i + \theta_t + \gamma RESY_{it} + \beta' z_{it} + u_{it}. \quad (4)$$

Clearly, as discussed, with the specification in (4) 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 methodology changes on our estimates.

Table 8 shows the results of the estimation of (4). The residual of (3), $RESY_{it}$, 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 (2) and (4):

$$D\_DEF_{it}^{t,t-1} = c_i + \theta_t + \gamma D\_Y_{it}^{t-\frac{1}{2},t-1} + \beta' z_{it} + u_{it}, \quad (5)$$

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 (2).[12]

As in the case of ex ante 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. 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.

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 ex ante ("planned") fiscal policy and an analysis of deviations from ex ante policy in response to new information becoming available since ex ante policies were determined. We employed various alternative strategies to deal with the potential

[12] 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, 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 unadjusted 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.

endogeneity of the explanatory variables as well as potential methodology changes between different vintages of the OECD data.

We find that ex ante 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 ex ante 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 ex ante policy, for the EU we find that fiscal policy reacts in a pro-cyclical way to new information about the business cycle. No such 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 ex ante to deviations from ex ante, the election dummy and the Maastricht and SGP variables all lose their significance.

We believe that our findings point to a general lesson. It is important that fiscal plans are set in such a way as to avoid discretionary corrections later when the economy performs differently from what was originally expected. Individuals take decisions on the basis of planned policies and such decisions may be costly to change or to reverse. However, our estimates suggest that cyclical corrections are found in the policy updates and, most unfortunate of all, they indicate pro-cyclical contractions in response to an unforeseen fall in economic activity. These findings make clear that it is important to better scrutinize policies at both the planning and implementation stage.

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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*** (0.032)	0.73*** (0.042)	0.92*** (0.056)
Y_{it}^{t-1}	-0.096** (0.047)	0.047 (0.053)	-0.20* (0.11)
$DEBT_{i,t-1}^{t-1}$	0.0074 (0.0046)	0.0065 (0.0060)	-0.0016 (0.011)
$NONACTIVE_{it}$	-0.075 (0.078)	-0.0006 (0.087)	0.051 (0.24)
$ELECT_{it}$	0.23** (0.090)	0.20** (0.10)	0.22 (0.17)
$M_{i,t-1}^{t-1}$	-0.40*** (0.11)	-0.39*** (0.11)	-
$SGP_{i,t-1}^{t-1}$	-1.32*** (0.26)	-1.04*** (0.27)	-
Estimation method	IV	IV	IV
Fixed Effects	country, time	country, time	country, time
Adjusted-R^2	0.96	0.93	0.98
Durbin Watson	1.91	2.04	2.06
Sargan test (p-value)	0.12 (0.99)	0.51 (0.92)	0.47 (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***	0.90***
	(0.033)	(0.043)	(0.061)
YN_{it}^{t-1}	-0.0097	0.11	-0.061
	(0.062)	(0.068)	(0.14)
YP_{it}^{t-1}	-0.30***	-0.092	-0.52
	(0.11)	(0.11)	(0.32)
$DEBT_{i,t-1}^{t-1}$	0.0056	0.0053	-0.0018
	(0.0048)	(0.0060)	(0.011)
$NONACTIVE_{it}$	-0.060	0.0088	0.016
	(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
Adjusted-R^2	0.96	0.93	0.98
Durbin Watson	1.97	2.10	2.07
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)
Adjusted-R^2	0.19	0.18	0.54	0.55	0.16	0.14	0.49	0.53
Durbin-Watson	2.08	2.17	1.95	1.94	2.06	2.09	1.72	1.77
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
Adjusted-R^2	0.66	0.65	0.64	0.62	0.74	0.75
Durbin-Watson	1.90	1.90	2.04	2.01	1.71	1.71
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
Adjusted-R^2	0.66	0.64	0.70
Sargan test (p-value)	0.14 (0.98)	0.003 (1.00)	0.36 (0.95)
Durbin-Watson	1.93	2.05	1.98
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
Adjusted-R^2	0.48	0.60	0.38
Durbin-Watson	2.35	2.37	2.51
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
Adjusted-R^2	0.66	0.63	0.74
Durbin-Watson	1.91	2.12	1.70
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
Adjusted-R^2	0.66	0.65	0.74
Durbin-Watson	1.91	2.06	1.71
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
Adjusted-R^2	0.65	0.66	0.61	0.64	0.72	0.73
Sargan test (p-value)	0.31 (0.96)	-	0.11 (0.99)	-	0.19 (0.98)	-
Durbin-Watson	1.96	1.91	2.22	2.04	1.99	1.72
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.