COMMENTS ON SESSION 2: DISCRETIONARY POLICY AND FISCAL IMPACT

Cláudia Rodrigues Braz[*]

Introduction

I would like to start by thanking the organisation for the opportunity to participate as a discussant in this workshop. I found all the papers in this session very interesting but due to time constraints I will only comment on the papers by Golinelli and Momigliano, Claus *et al*. and Ernesto Rezk. I would also like to refer that this discussion is based on the draft versions of the papers presented at the workshop and some of the points raised may no longer be valid for the published versions.

1. Some comments on the paper by Golinelli and Momigliano

The authors constructed a model to analyse the characteristics of fiscal policies in the euro area in the 1988-2006 period, using as explanatory variables the initial state of public finances, the European fiscal rules, cyclical conditions and the political budget cycle. A Maastricht variable is defined to take into account, in the European context, the requirement to correct an excessive deficit with respect to the 3 per cent of GDP threshold. If this variable is binding, the discretionary fiscal action, measured by the change in the cyclically adjusted primary balance, is only a function of the Maastricht variable. Otherwise, it depends on the previous period output-gap, the initial state of public finances (measured by the previous period primary balance and debt level) and some dummy variables to capture the electoral cycle.

Concerning the data used, the main problem is related with the construction of the output-gap series, since real time data it is only available from 1994 onwards. A comparison between the two data sets, for the years for which it is possible, shows quite similar standard deviations (1.4 and 1.8 in the constructed and the published data series, respectively) but an average value still quite different (–0.4 in the authors estimates to be compared with –1.0 in the published data) and a low coefficient of correlation (0.7). Nevertheless, it is worth mentioning that the authors estimated the model on the basis of both sets of data, obtaining similar results.

The authors use real time data instead of *ex post* data for all explanatory variables with the exception of the debt level. Regarding this issue, two observations could be made. Firstly, I fully agree that governments base their fiscal policy decisions on the information actually available at that time as far as the cyclical

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situation is concerned. However, concerning the initial state of public finances, the use of the same argument is not so obvious. Indeed, in most of the countries where significant statistical revisions occurred the authorities were probably aware of the actual situation of public finances. Secondly, the authors base the real time data on the December OECD Economic Outlook and argue that even though budget documents are the most direct source of real time information, the estimates required are not reported sometimes and might be distorted by political reasons, reflecting differences in risk aversion. The following table shows for the 2001-06 period the real time general government balance, on a National Accounts basis, in Portugal using the OECD December Economic Outlook and the Report of the State Budget. As it is shown, the estimates using the two sources are quite similar, with the exception of the years 2001 and 2002. Indeed, *ex post*, the data included in the Budget proved to be closer to the outturn than the OECD estimate since the fiscal authorities anticipated better a statistical revision of the budgetary data in 2001 and the implementation of temporary measures at the end of 2002.

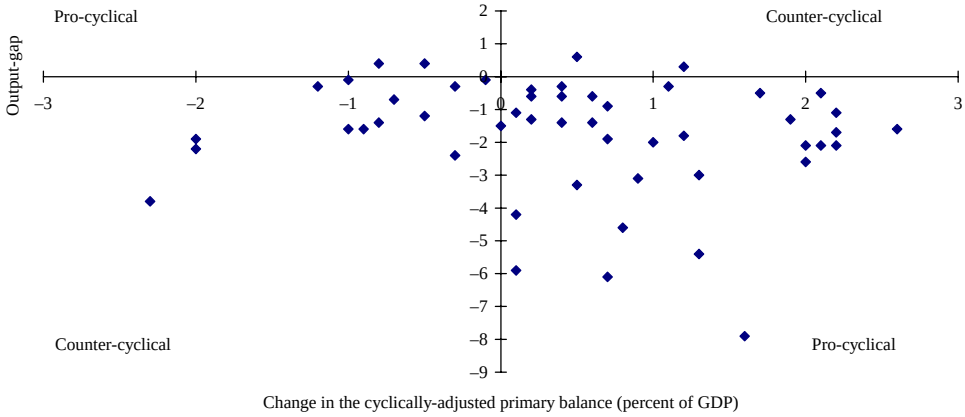
Regarding the specification of the model, I would like to raise three questions to the authors. Firstly, in the definition of the fiscal rule when the Maastricht variable is not binding, could it be used the output-gap of the current year instead of the one of the previous year? The authors argue that the output-gaps are highly persistent but since real time data is used, perhaps it could be a feasible option.

Real-time General Government Balance in Portugal (National Accounts)

	2001	**2002**	**2003**	**2004**	**2005**	**2006**
OECD December Economic Outlook						
2001	**–1.7**	–1.5				
2002		**–3.4**	–3.0			
2003			–2.9	–3.0		
2004				–2.9	–3.0	
2005					–6.0	–4.9
State Budget (October *t*–1)						
2002	**–2.2**	–1.8				
2003		**–2.8***	–2.4			
2004			–2.9	–2.8		
2005				–2.9	–2.8	
2006					–6.0	–4.6

* On a Public Accounts basis.

Output-gap and the Change in the Cyclically-adjusted Primary Balance (Euro Area, 1993-97)



Source: Ameco database, Autumn 2005.

Secondly, could the Maastricht variable, besides interest payments, also take into account the expected contribution of the cyclical component to the fiscal outcome? Finally, could the previous year cyclically adjusted primary balance be used as a measure of the initial state of public finances, instead of the primary balance itself?

Finally, as far as the results are concerned, I would like to comment only on the reaction of fiscal policy to cyclical conditions in the euro area. The findings in the literature for this issue are not homogeneous: fiscal policy appears pro-cyclical or counter-cyclical, asymmetric or symmetric. The authors found a sizeable counter-cyclical reaction of fiscal policy for the euro area countries, relatively stable across sub-periods in terms of point estimates, even in the period prior to the participation in the third phase of EMU (1993-97). This outcome is quite unexpected since we know that in most EU member-states at that time cyclical conditions were not very favourable and many countries had to implement a fiscal effort in order to ensure the participation in the euro area. Indeed, when we look into a simple chart with the change in the cyclically adjusted primary balance and the output-gap in the euro area in that period, using *ex post* data and not controlling for other variables, we find that most fiscal policies appear pro-cyclical (see below). As such, the result obtained by the authors might be biased and is probably only capturing a small sub-sample of the total set of observations. Indeed, the only thing that we can conclude is that for the 'well behaved' countries, *i.e.*, those with a deficit below the 3 per cent of GDP limit, for which the Maastricht variable was not binding, fiscal policies were counter-cyclical. I would admit that the number of observations in this sub-sample might be quite small, explaining the low significance of the results.

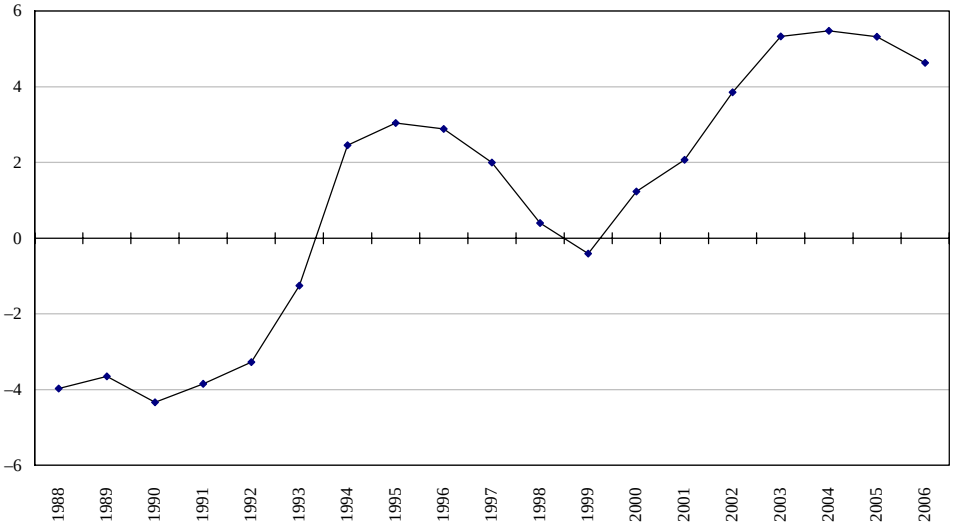
2. Some comments on the paper by Claus *et al*.

In this paper, the authors use a three variable VAR model (GDP, net tax and government spending) to assess the effects of fiscal policy in New Zealand. One of the main challenges in the implementation of this type of methodology is the assumption of identification restrictions to estimate the structural residuals. As the model is based on Blanchard and Perotti (2002), the same identification procedure is adopted.

The authors assume two alternative trend specifications in their fiscal VAR: a deterministic specification, where the variables are defined in logarithms, and a stochastic specification, which is estimated using the first differences of the logarithms of the variables. Then, they compare the contemporaneous and dynamic effects of government spending and net tax temporary shocks using the two specifications. However, the dynamic results are not comparable. With the deterministic specification, as the variables are defined in levels, the shock is indeed temporary. But, in the stochastic specification, the temporary shock in the variables' first difference is very similar to a permanent shock in levels (not completely in this case because the authors in the stochastic specification subtract a changing mean). This type of reasoning may explain why the impulse response functions in the deterministic specification tend to converge to zero but the same does not occur in the stochastic specification. The same results appear in the alternative model specification tested by the authors, which compares the stochastic specification with a 'Hodrick Prescott specification' (variables defined in levels but deducted by trends obtained using the Hodrick Prescott filter).

To assess the individual effects of tax revenue and transfer payments, the authors re-estimate the model splitting net taxes in two variables. In the case of a four variable VAR, six identification restrictions are needed (three more than in the previous specification). As mentioned above, the identification procedure is crucial for the interpretation of the results. However, this one is not specified in the text and can only be guessed through the analysis of the impulse response functions (like the no reaction of transfers to tax or spending shocks).According to Blanchard and Perotti, 2002, "the imposition of a cointegrating relation between G and T yields very similar results to our benchmark case". Indeed, the existence of cointegrating relations between the VAR variables changes the way the identification restrictions are imposed (for the econometric procedure proposed in this situation, see, for example, King, Plosser, Stock and Watson, 1991, AER). In the paper on New Zealand, the authors did not check this hypothesis. However, the analysis of the general government balance in this country between 1988 and 2005, suggests that net taxes and government spending are probably not cointegrated as the budget balance fluctuates a lot (see chart opposite). In any case, a reference to the results in the text could be added.

New Zealand – General Government Overall Balance
(percent of GDP)



Source: OECD Economic Outlook.

3. Some comments on the paper by Rezk

The author estimates a five variable VAR model (tax revenue, public expenditure, GDP, unemployment and inflation) to assess the effects of fiscal policy in Argentina for the period 1980-2005. Ten identification restrictions were imposed, including the assumption that public expenditure does not contemporaneously depend on unemployment. For this to be correct, public expenditure in Argentina should not include any type of unemployment benefits.

Concerning the cointegration relations between the VAR variables, the author refers that "the possibility of effects of cointegration are more important when long-run relations are being analysed". Nevertheless, as mentioned in the comments to the previous paper, the existence of cointegration in a VAR model may change the identification procedure. In this context, perhaps it could be checked by the author and a reference added to the text.

Finally, as mentioned in some cases by the author in the paper, some results still require further analysis. As an example, I would like to highlight three results whose interpretation is not very intuitive:

i) the negative response of tax revenues to a public expenditure shock, in spite of an increase in GDP;

(ii) the positive reaction of GDP to a positive tax revenue shock;

(iii) the "cyclical" reaction of inflation to a GDP shock.