COMMENTS ON SESSION 4
FISCAL TOOLS TO CONTROL MACROECONOMIC RISKS AND IMBALANCES: EXPERIENCES AND PRESCRIPTIONS

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Obviously, I would first like to thank the Banca d'Italia colleagues for organising this excellent workshop and having me again. My comments will focus on two very different papers. The first one, by Lendvai *et al.*, tackles the question how the measurement of the structural (or cyclically-adjusted) budget balance can be improved. The second one, by Herrero García *et al.*, focuses on Spain and analyses the complex relationships between fiscal and macroeconomic imbalances. Both papers provide a lot of food for thought and are well written and I congratulate the authors.

Let me start with Lendvai *et al.* Actually, this paper is a trip down memory lane for me. I vaguely remember from the mists of time that the first Banca d'Italia fiscal workshop in 1999 was primarily devoted to cyclical adjustment of budget balances. This provided the impetus to develop our own method with a number of ESCB colleagues. On the one hand, it is nice to see that this issue is still relevant today, fourteen years later. On the other hand, pessimists may argue that fiscal experts are not working very quickly or, at least, it takes quite long for them to agree on certain issues and cyclical adjustment appears to be a case in point.

Cyclical adjustment methods essentially *simplify* reality by presupposing stable links between broad tax categories and (easily available) macroeconomic variables. In my view the limitations of this approach are most evident for corporate (profit) taxes. In this particular case one typically tries to capture a relationship that is essentially non-linear (as there are no negative taxes, or subsidies, for losses in year *t*) with a constant elasticity. In addition, tax rules are usually so arcane that the real-world concept of "taxable corporate profits" is far removed from anything that can be found in the national accounts. Finally, corporate tax systems typically include rules to carry forward losses, which implies an unknown lag structure for the relationship between macroeconomic aggregates, such as GDP, and corporate tax proceeds.

Most cyclical adjustment methods (e.g., the ones used by the EC, the OECD and the IMF) follow a two-step approach. First, a broad tax category (or unemployment spending) is linked to a macroeconomic aggregate, which is thought to determine its growth, e.g., VAT and other indirect taxes are related to private consumption. Second, this macroeconomic "tax base" is linked to GDP. In both steps, standard elasticities are econometrically estimated or obtained from tax rules. The aggregate budget sensitivity to the cycle can then simply be derived from the set of elasticities for the budget items considered. The Lendvai *et al.* paper is essentially addressing the issue of large "volatility" in this second type of elasticity, between the tax base and GDP. The authors propose to do so by linking part of the budget to another macro variable, *i.e.*, domestic absorption instead of GDP. Please note that the ESCB method is more radical in this respect: (almost) nothing is linked to GDP.

I want to start my specific comments on the paper by focusing on a country example, Belgium. As you can see from table 1, the correction proposed by the authors significantly modifies the levels of the cyclically adjusted balance: for the 2005-07 period the new CAAB – based upon domestic absorption – is about 0.5 per cent of GDP better than the estimate on the basis of the "official" EC method. At the same time, it is clear from the table that the ESCB method

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These comments reflect the views of the author and not necessarily those of the National Bank of Belgium.

Table 1

An Example: Cyclically-adjusted Balances for Belgium
(percent of GDP)

	2005	2006	2007	2008	2009
CAB – EC	–2.8	–0.4	–1.5	–2.1	–4.4
CAAB – Lendvai *et al.*	–2.4	0.0	–1.0	–2.0	–4.4
ESCB – Bouthevillain *et al.* (2001)	–2.1	0.4	–0.5	–1.5	–5.3

(Bouthevillain *et al.*, 2001) that is essentially aimed at a better matching between budget categories and macro variables leads to a larger correction: cyclically-adjusted balances are about 0.7 to 1 per cent of GDP higher than estimated by the EC. So, I agree with the authors: the correction implied by the CAAB is "not just peanuts". It is definitely a step in the right direction but does it go far enough?

Let me now zoom in on 2009. Here I was struck by the fact that the change in the CAB is roughly equal to the change in the CAAB. Yet, 2009 is a textbook case of "unbalanced" growth in my view. The impact of the Great Recession on the major tax bases was much more benign than on GDP. While the latter dropped by close to 3 per cent in real terms, real private consumption declined by less than 0.5 per cent while earned income, due to the specific features of the notorious Belgian indexation system, even increased somewhat in real terms. So, wouldn't you want a (significantly) smaller cyclical impact – and, hence, a more important cyclically-adjusted worsening – than that based on a standard output gap method such as the EC's cyclical adjustment method? In my view, the new CAAB does not seem to fully address the issue of unbalanced growth episodes.

I now turn to a number of technical and other specific issues. First, I wonder if the authors could elaborate on the rationale for using deviations with respect to norms rather than deviations from trends for the absorption component. The norms in themselves are period- and country-specific but they are determined in a panel regression and, hence, time-varying and country-specific impacts of explanatory variables are ruled out. I am not sure if such an approach really captures actual *cycles* in absorption. If the correction does not have a zero mean in the longer term, it is a somewhat different type of animal than the cyclical adjustment methods that we are used to and that we use to evaluate fiscal policy in a medium-term perspective. More generally, it is worthwhile to place the paper in the context of the literature on CAB measurement errors and revenue windfalls and shortfalls. One strand of this literature stresses the role of asset prices. An operational method to augment CABs with a correction for asset price cycles is proposed, for instance, by Price and Dang (2011). The results that they present are not fully in line (and sometimes at odds) with the CAAB correction and I would argue that both corrections are not complementary as asset price cycles should, in principle, be related to absorption cycles. Another strand of the literature tries to explain actual revenue windfalls and shortfalls. In this connection, Morris *et al.* (2009) show that unexplained residuals can be large – even if one uses the ESCB approach that is supposed to provide a better matching! – and are found primarily in profit taxes and, to a much lesser extent, in consumption taxes. It seems to me that the CAAB correction mainly targets the latter and would not necessarily deal with unexplained cyclicality in corporate taxes.

Table 2

Trend Uncertainty when Measuring CABs
(percent of GDP)

	CAAB	CAB	CAB – Spring 2008
Spain (2007)	0.2	0.9	2.4

This brings me to the more general issue: what should be the role of cyclical adjustment in fiscal governance systems, and, more specifically, how should CAB measurement uncertainty be reconciled with the need to consider the business cycle when assessing fiscal outcomes? Let me first reiterate the point made by previous speakers: in my view the biggest problem, by far, is the trend uncertainty, as shown for instance by different vintages for the 2007 CAB for Spain (see Table 2). I was also intrigued by the recent Borio *et al.* (2013) paper that manages to proxy "final" output gaps in real time by just looking at a number of simple financial indicators and the work shown by Christophe Kamps earlier in the conference goes in the same direction even though the methodology is different. In addition, the method that is now used by the EC should in principle be less prone to trend revision. However, I am not sure whether trend uncertainty can and should be completely sanitised away. In the end, our view on the cycle is and should be determined by our expectations for the future. As we do not have perfect foresight, trend revisions will remain part of life and CAB estimates will continue to come with some degree of uncertainty. So, how should we use these uncertain CABs in fiscal rules? My take on this consists of three elements. First, be prudent. Make sure that there are sufficiently large safety margins. This implies, for the EU countries, that one should err on the side of caution, e.g. when setting medium-term objectives or defining the appropriate size of fiscal consolidation. Second, one should always try to not just estimate but also *explain* developments in CABs, in particular by reconciling them with a bottom-up approach starting off from actual fiscal measures. An operational method is proposed, for instance, by Kremer *et al.* (2006). Finally, fiscal rules based on CABs should be complemented by other types of rules: limits on nominal deficits, public debt ceilings and, as other people have already argued in this workshop, expenditure rules.

If you allow me one final comment on the Lendvai *et al.* paper: it comes at a time when the new EU fiscal governance framework will be put to the test and e.g. the European Commission, the authors' employer, is currently considering imposing fines for certain fiscal sinners on the basis of a CAB methodology, which, according to the authors, is not fully sound from a methodological point of view. This makes the paper not only very interesting but also very courageous. I obviously assume here that the upper echelons of the European Commission still intend to take the new governance framework seriously.

I turn to Spain now. Herrero *et al.* provide a very rich paper combining descriptive analysis ("What went wrong?") with policy-relevant empirical research on the links between fiscal and other imbalances. Let me skip the summary. I guess you've all been awake during the presentation. I go to the comments immediately. The paper uses different models to analyse different questions so discussants have to do some cherry-picking and I will focus on two specific issues. However, let me first say that I really enjoyed also the descriptive part of the paper. We all know something about what went wrong in Spain but it is very nice to have all elements together in one paper. I take from the paper that all the failures of our monitoring and governance system somehow came together in Spain. If only the paper could have been written six years ago!

OK, issue number 1: non-Keynesian effects. I guess it is fair to say that the debate on fiscal multipliers is raging. It seems to me that the claim about the presence of non-Keynesian effects is somewhat at odds with the current majority view (or, at least, the concern) that we may underestimate the demand-reducing impact of fiscal consolidation. Basically, the authors are up against a Grand Coalition of various fiscal experts, ranging from the IMF, household names such as professors Krugman and De Grauwe to the research staff of the Syriza party in Greece, not to mention the thousands of people that are demonstrating in the streets of Madrid from time to time. So I guess the issue deserves a closer look. Even though I am actually sympathetic to the claim, I am not fully convinced that it can be derived from the empirical results presented in the paper. The argument is essentially based upon responses of private investment (+) and unemployment (–) to a "fiscal balance" shock but the VAR that is used does not contain GDP or private consumption. In addition, it was unclear to me if the fiscal balance shock is orthogonal to the business cycle (the authors do not seem to use cyclically-adjusted budget balances in the paper that I read). Moreover, the alternative VAR identification scheme that is proposed in the paper would seem to be at odds with the conventional wisdom and Section 2 of the paper. Finally and most importantly, the observed positive link between fiscal consolidation and investment may just point to "crowding-in" of private investment but not to non-Keynesian multipliers. In this connection, the Cardoso and Domenech (2010) paper that is referred to is very interesting. These authors use a rational expectations model with liquidity constraints and find a similar crowding-in impact of fiscal contraction on investment. However, their empirical results still point to Keynesian multipliers overall as the beneficial impact on private investment is more than offset by the negative impact on private consumption.

Another point is that, if I learned anything from the literature, it is that circumstances matter for the size and the sign of the multipliers: liquidity constraints, fiscal stress, openness, degree of monetary accommodation, you name it. In this respect, I was wondering if one single model can describe both pre-crisis and post-crisis Spain. I would argue that, today, there is for instance a higher degree of fiscal stress but, presumably, the share of liquidity-constrained consumers has also risen.

Finally, the authors argue that the composition of fiscal policy matters. I couldn't agree more. However, they can not really substantiate it because the empirical approach does not go beyond the budget balance. As the workshop is hosted by the Banca d'Italia, I felt that it was appropriate to refer to a number of papers by Banca d'Italia colleagues – e.g., Giordano *et al.* (2008) and Caprioli and Momigliano (2011) – that use VARs to look at the budget in a more disaggregated manner. I think that the authors will find some support for their claim on the importance of the composition of fiscal adjustment there.

Let me now turn to the second issue: how to rebalance the current account and what is the role for the government budget? Here the authors use an ECM linking the current account to 5 variables. Now, I personally wouldn't know the first thing about how one should model the current account but then I remembered that this is actually done in the other paper that was allocated to me. So why not use the know-how of the European Commission to criticise our Spanish colleagues, so to speak? When looking at the differences with the panel approach used in the Lendvai *et al.* paper, it struck me that in the regressions there is no role for GDP (growth) so one may miss out on differences related to catching-up effects. Also the International Investment Position and the oil balance are not included. In my experience, trying to explain current account movements without taking into account the latter, in particular, is quite an uphill task for many countries.

Then there is the issue of the positive long-run relationship and the negative short-run dynamics for some variables such as the budget balance and the investment variable. I am not fully convinced by the explanation of diminishing returns. In my view it may also point to identification

or multicollinearity problems. The long-run and short-run coefficients of almost equal magnitude for the budget balance are particularly difficult to interpret in my view.

In addition, the paper finds a (clear) positive link between the fiscal balance and the current account, which suggests that fiscal consolidation weighs on domestic absorption. How does that square with authors' view on non-Keynesian effects? Several papers, including for instance Nickel and Vansteenkiste (2008), specifically look at the fiscal-current account relationship to evaluate the presence of Ricardian equivalence.

Let me wrap up by getting back to the broader picture and the policy messages: the authors suggest that the scope for addressing external imbalances with a fiscal contraction alone is likely to be limited. This lends support to the policy line that fiscal consolidation needs to be accompanied by structural reforms, in particular aimed at enhancing competitiveness and increasing the employment rate. Against that background, an even greater focus on cost developments, including but not limited to unit labour costs, in the paper would have been warranted.

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