COMMENT TO
"STRENGTHENING POST-CRISIS FISCAL CREDIBILITY:
FISCAL COUNCILS ON THE RISE. A NEW DATASET"
BY XAVIER DEBRUN AND TIDIANE KINDA

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Thanks to the organisers for putting together such an impressive programme and having me again. It is always a pleasure to be here. I want to start my discussion of Debrun and Kinda by reiterating the paper's key contributions and main messages.

First, the authors have created a new and very rich dataset on existing fiscal councils and their characteristics. Second, they present some empirical evidence on the beneficial impact of those councils. On the one hand, they seem to coincide with better fiscal outcomes, in particular higher primary surpluses, on the basis of a standard fiscal reaction function approach *à la* Bohn (1998). On the other hand, countries with fiscal councils seem to produce better one-year ahead fiscal and, to a lesser extent, macroeconomic projections. Third, to be more precise, this beneficial impact is not so much linked to the mere existence but rather to certain specific characteristics of fiscal councils that pertain to their mandate, mechanisms to ensure non-partisanship, as well as their media impact. Finally, the empirical results also lend support to the mainstream view that fiscal rules and fiscal councils are complements, rather than substitutes.

Before I go into the specifics of the paper, let me remind you of the general context. Fiscal councils are indeed on the rise, as the paper's title suggests. In the European Union, recent legislation has made them an integral part of the fiscal governance framework. It is by now widely accepted that they can provide some protection against the government's deficit basis and can help the general public to assess rule compliance. They come in two forms – parliamentary budget offices or independent fiscal institutions but the paper does not go into that distinction (the authors' dataset comprises both types) so I won't either. There is also a broad agreement on the desirable features of independent fiscal institutions. Many of them (non-partisanship, specific mandate, operational capacity, media impact, etc…) are picked up in Debrun and Kinda's paper. So we have a rather precise view, a blueprint of which kind of fiscal council we want to be part of an effective budget framework.

What we don't have, however, is very strong empirical evidence on the actual impact of these councils. Most of the evidence is anecdotal and drawn from individual country studies. Broader econometric approaches typically have difficulties to establish real causality. This paper tries to address this gap and generally comforts us in the belief that these councils do work even if, in my view, more direct tests of regime changes are still necessary: I will not name names as I want to keep good relations with all colleagues around the table but can countries with a weak fiscal governance track record really change their ways by establishing a fiscal council? Given that the more modern fiscal councils – the ones that comply with the aforementioned principles – are relatively recent phenomena, it may be too early to tell.

At this point, I have a confession to make: I am not the world's best discussant for this paper, not because I don't like it but because I like it too much. I genuinely believe and want to believe in the conclusions of the paper! It is like reading a paper that provides econometric evidence of the fact that my favourite football team, the mighty *Biancoceleste*, is not just the best team in Rome or

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This discussion reflects the views of the author and not necessarily those of the National Bank of Belgium.

in Italy but, indeed, the best team in the world. Would I feel inclined to waste my time and have a critical look at this econometric evidence? Of course not, because I simply know it's true!

However, if I don't do my job as a discussant, I may not be invited anymore by the Banca d'Italia colleagues and I don't want to run that risk. So I try to put my positive bias aside and will offer some comments. I start with the authors' dataset, the companion, if you will, of the existing dataset on fiscal rules. Actually, this is not the first dataset of this kind: as mentioned in the paper, the EC maintains a database on fiscal institutions for EU countries. Apart from the country coverage, the key difference is that the authors' database relies more on expert assessment rather than on self-reporting by the authorities via questionnaires. Even if I know that there is some peer review and in-depth cross-checking of the EU countries' fiscal frameworks, I have to admit that I feel more at ease with the Debrun and Kinda approach. More generally, I have always considered it a tremendous challenge to capture a wide variety of characteristics in a number of simple binary or quantitative indicators. You inevitably lose some detailed information once you try to compare institutions across countries. In the end, some expert judgment will always be required.

So I know how hard this exercise is and I am not going to bother you with any criticism on the specific judgement made in the paper. Let me just raise a few general issues. Take a crucial feature like independence for instance: this is really hard to measure even if the paper already goes beyond the basics and provides data on who appoints who in fiscal councils. In the end, what matters is, how the council behaves, so, in principle, you also need to score their actions in a way. Ideally, one should look at the (time) consistency and coherence of their reports, whether they do not simply reflect a pre-existing government consensus, etc.

Media impact is another crucial issue: how is this measured? I remembered that, in earlier papers, Xavier Debrun looked at the number of times a council is mentioned to in the media. However, there is also a qualitative dimension, right? One short paragraph on p. 27 of the newspaper referring to an arcane descriptive report that was published by a government-friendly fiscal council should not have the same weight as, say, Kevin Page or George Kopits stating publicly and clearly that their respective governments got it wrong.

Finally, taking stock of all these features is just one step. In further work, you may also want to think about constructing a synthetic Fiscal Council Index taking into account your empirical findings of which characteristics are important. I now turn to the econometrics and will first make some general comments on the approach before going to the results.

First, the authors use a dynamic panel approach but two different country samples and periods. I was wondering about the second sample, which is smaller and shorter (1998-2010), that is used for forecast analysis. Not that many changes took place in the fiscal council landscape in that particular period as far as I know. According to Table 1 only 7 fiscal councils were created then; the "new-generation" fiscal councils are typically more recent. Actually, that number includes some specific cases such as the Austrian public debt committee, for which the numbers in the dataset may overstate the actual impact, and the first Hungarian fiscal council that was so effective and independent that it got closed down two years later. So, I am wondering whether the authors have sufficient within-country variation in the dataset to get robust conclusions.

Second, the dataset includes both EU and other countries. Maybe one needs to acknowledge the existence of the European Commission by including an EU dummy for the former? Otherwise any positive impact from this supranational watchdog could be wrongly attributed to the national institutions.

Third, due consideration should be given to the use of interaction terms that seek to capture the joint impact of different independent variables. On the one hand, I am not fully convinced by the need to study the interaction between fiscal rules and fiscal councils, which the authors have tried. They have shown that both matter: this is in my view sufficient to establish complementarity.

On the other hand, interaction terms may be necessary for different dimensions of the same characteristic: independence comes to mind, in particular if one thinks about the fate of the aforementioned previous Hungarian fiscal council. An institution can only be truly independent if guarantees exist with respect to all dimensions of this independence, which to me suggests interacting at least some of the dummies for independence (legal/budget) that the authors consider.

I now turn to the specific results of on fiscal performance. My first question is why the observed primary balance rather than the cyclically-adjusted or structural one is used. Distinguishing between good policies and good luck is one of the core tasks of a fiscal council so you may want to analyse more precisely how the council contributes to good policies and correct the estimates for any impact of good luck. The output gap appears among the explanatory variables but it is lagged so that should not raise a particular problem. Second, I was simply wondering why the number of fiscal council staff only appears in this section of the econometric part and not in the other section on forecast performance. At the same time, I am curious why the authors do not consider a broader spectrum of characteristics in the dataset: the dummy describing whether the council's remit includes normative analysis or making specific recommendations would seem to be an ideal candidate to be included in these regressions. Third, apart from the aforementioned need for an EU country dummy, one may include, in these equations in particular, a dummy for the incidence of specific adjustment periods (the Excessive Deficit Procedure or, especially, the recent troika-managed programmes for specific euro area countries). Under such programmes, budget targets will be more binding and not taking that into account may bias the coefficients, e.g., for fiscal council characteristics, to some extent. Finally, I want to come back to the issue of correlation vs. causality that the authors duly mention in the paper. If the omitted variable bias is linked to a country's social preferences regarding fiscal discipline, it can obviously be more of an issue when one specifically tries to find out if good fiscal institutions contribute to good fiscal performance.

As regards the second part of the empirical analysis, that focuses on the link between fiscal councils and forecast performance, I was struck by the graph showing the optimistic macro forecast bias, also in countries that have effective fiscal councils, even if the over-prediction seems smaller in that case. Probably the Great Recession also came as a surprise for the Wise (Wo)Men in those fiscal councils and the large forecast error in 2008-09 has a significant impact on the average. Staying with the issue of the councils' impact on the macro forecast error, I found the empirical results somewhat difficult to interpret: the authors seem to show that, for real growth, the reduction in the absolute forecast error – or the increase in forecast accuracy – is significant (Table 10 in the Annex), while that in the average or mean forecast error – or the bias – is not (Table 7). Should we then conclude that, on this particular topic, the value added of independent fiscal institutions primarily lies in technical expertise (smaller errors), rather than in non-partisanship (smaller bias)? However, the descriptive statistics depicted in Figure 5 do seem to suggest that some fiscal council characteristics are positively correlated with less (over)optimistic forecasts.

For this empirical analysis it may also be useful to consider a specific hierarchy of fiscal council characteristics. One may expect that the councils' mandate to provide or assess official forecasts would be the most important feature here. Hence, it may be useful to include interaction terms of this remit variable with other characteristics of fiscal councils, e.g., independence. Maybe the joint impact of a truly independent council that has some say over the official macro forecasts could be more significant than the separate impact of both the remit and the independence variable? One other minor quibble relates to the use in pooled regressions of actual forecast errors for different countries and, hence, different business cycles. As the amplitude of the cycles may differ, forecast errors should perhaps ideally be normalised. Finally, as one of my core tasks is to coordinate macro forecasts, I just want to highlight also the quantitative results for the absolute real growth forecast error: coefficients of fiscal council dummies in Table A.3 tend to be in the [0.4;0.6] interval. This seems to suggest that the accuracy gain of giving a fiscal council some responsibility over the official growth forecasts is actually huge!

Turning then to the analysis regarding the budget forecast performance, it should be stressed that the weak contribution of the fiscal rule index in explaining the average primary balance forecast error is somewhat surprising (and disappointing?). At the same time, European colleagues, for instance, will probably recall many episodes where rule circumvention seems to have been an equally important guiding principle than rule compliance. However, the empirical results regarding the impact of fiscal councils are actually more clear-cut than for the macro forecast performance discussed earlier.

In this connection, I was wondering whether there could be a story here. Clearly, for most countries there are quite a lot of growth forecasts on the market. In this sense, a multitude of both private forecasters, as well as international organisations may already offer the necessary checks and balances for the official macro forecasts used in the budgetary process. Governments that want to cheat may face an uphill task if they have to explain why their growth forecasts are much more benign than the average of those of the relevant international organisations such as the IMF, the OECD and the EC and private think-tanks. This may be somewhat different for the actual budget, i.e. government revenue and expenditure, forecasts that are derived from those macro projections. In this area the government typically has an information advantage over other forecasters, e.g., regarding actual tax elasticities, spending risks, the impact of new measures, etc. For this reason, the existence of these other forecasts may not be sufficient to deter the government from presenting overoptimistic estimates in the budget.

If this is the case, then the real value added of independent fiscal institutions could lie in verifying the way in which budget projections are derived from macro forecasts. This is consistent with the authors' empirical findings. This also suggests that the mandate of such independent fiscal institutions should definitely include responsibilities related to the costing of measures and, more generally, government revenue forecasting. As soon as there is sufficient variation, the authors may also want to check the empirical significance of the costing dummy in their dataset. In this connection, a specific case can also be made for the costing of electoral platforms to make sure that political parties do not present unrealistic plans before the elections.

These were my (minor) comments on the Debrun and Kinda paper. Let me just reiterate that, in my view, this is a very interesting and important paper. Both the descriptive analysis based upon the new dataset and the empirical results on the effectiveness of fiscal councils are significant contributions to the literature. I congratulate the authors and encourage them to continue this line of research.