Comments on Qianying Chen, Andrew Filardo, Dong He and Feng Zhu's paper "International spillovers of central bank balance sheet policies"

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First of all, I would like to thank the BIS Representative Office for Asia and the Pacific for inviting me to join this thought-provoking conference and, in particular, to be a discussant for the paper entitled "Central Bank Balance Sheet Spillovers to Asia and the Pacific" by Qianying Chen, Andy Filardo, Dong He and Feng Zhu, which I have found very interesting.

My comments are based on the October version of the paper. Since then, the authors have made some extensions to their work, but I think my comments are still very much relevant. So I will try to first highlight the strengths of this paper, elaborate a bit on some of the parts that the authors didn't touch upon so much in their presentation today, and then spend more time on suggestions that might make the paper even stronger.

So let me start with the strengths. To me, the paper extends the study on the impact of major economies' balance sheet policies in the right direction – that is, toward considering the impact of these policies on real activity, as opposed to only the impact on the asset markets, and at the same time toward considering the cross-border spillover effects, especially on emerging markets in Asia and Latin America. These two questions are important indeed to understanding the international implications of quantitative easing and the policy issues that follow. And given that previous research works on unconventional measures have focused largely on their impact on specific financial market indicators, the aim of this paper is commendable.

Another strong point is that the paper tries to capture the effects in a systematic manner, starting with an event study to gauge the importance of the asset price channel. Then comes the interesting section on estimating the impulse responses to a US quantitative easing shock, and this is done for many countries using the vector autoregressive (VAR) technique. In the October version of the paper, the authors used changes in the US term spread between 10-year and 3-month Treasury yields as an indicator of the Fed's balance sheet policies and estimated the VAR model based on data from February 1995 to December 2010, separating the full sample into **the pre-crisis period** up to December 2006 and **the crisis period** from January 2007 onward. Due to limited data availability, the crisis-sample impulse responses are *derived* from impulse responses estimated from the pre-crisis and the full samples, assuming that the full-sample estimates are a weighted average of the pre-crisis sample and crisis-sample estimates. The paper concludes with counterfactual simulations based on the VAR results.

The key findings are: (1) from the event study, the global asset price channel appears to be important, which is consistent with the conclusion reached by previous works based on event studies; (2) international spillovers of US balance sheet policies are large and, rather intriguingly, larger than their domestic impact; (3) the spillover effects, however, vary greatly from one emerging economy to another, both in terms of size and direction. The authors attribute this variation to the many channels of transmission in play.

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Since the VAR results are central to the paper's conclusions, I think it is important to take a good look at these VAR estimates. So my comments are in one way or another related to the estimated VAR model, and there are four points I would like to make here:

1. The first thing that strikes me is the imprecision of the VAR estimates, as indicated by the wide standard error bands. Understandably, there could be many reasons for this, even for the case of the US, such as impaired transmission channels during stressful times and different market reactions to policy shocks in different circumstances. That is to say that much of the effect of unconventional measures probably worked through the confidence channel, and this is difficult to measure. For emerging markets, the problem could be compounded by varying importance of transmission channels and different policy contexts in each country. So my first point is that the crisis sample estimates are, even at first glance, likely to suffer from poor accuracy, and I am not yet comfortable drawing strong conclusions from them. Perhaps we should look at what may contribute to the precision problem.

2. Given that the data used cover the years 1995 to 2010, it should be pointed out that most of this period is considered normal times, and the monetary policy instrument of choice for the US was the interest rate. The fact that in normal times central banks use the interest rate rather than balance sheet measures means that it is also likely to be a more potent instrument, and hence we cannot ignore the fact that prior to the adoption of balance sheet policies, the Fed did reduce the policy rate very aggressively [Figure 1]. With the long and variable lag of monetary policy transmission, the effects of such interest rate reduction would still take place in the economy within the crisis sample, and we definitely cannot attribute everything to the balance sheet measures. The paper did not seem to take this into account, so the results are not clean enough. To avoid mixing up the effects of interest rate and balance sheet measures, the paper would need to look at the impact of an innovation in the term spread that is *not* correlated with changes in the policy interest rate, and also other factors which could influence the term spread. Therefore, the paper should at least consider controlling for the policy interest rate in the VAR estimation.


Figure 1: Fed Balance Sheet & Policy Rate
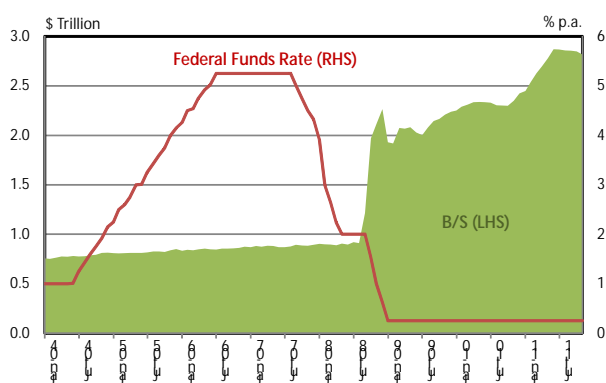
Source: Bloomberg

3. Another problem with using term spreads as a policy indicator as the paper does is that we are mixing two regimes in the full sample estimation – one regime under normal times when monetary expansion is associated with **an increase** in term spreads and another regime during the crisis period where unconventional

expansionary monetary policy leads to ***a reduction*** in term spreads [Figure 2]. Mixing the two regimes with opposite reactions of the term spread to the monetary policy direction in the same VAR estimation is troubling and could be a major problem for the accuracy of the results.

Figure 2: UST Spread (10Y – 1Y)
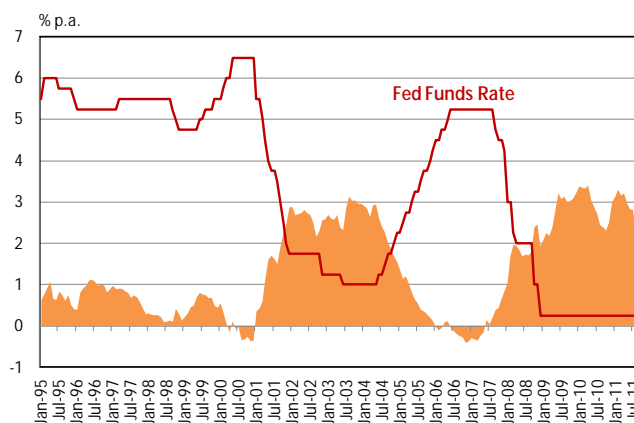
Source: CEIC

4.   Now I come to my fourth and final comment. In my opinion, a VAR-type model may not be the most suitable approach in a situation like the one we are dealing with. It seems that the key shortcomings of the "no theory" or "let the data speak" philosophy of VAR become more serious in this situation. VAR requires a lot of data, but we have very limited data regarding the balance sheet policies. Even in normal times, VAR results can be hard to interpret due to the entangling of many monetary policy transmission channels. It is definitely harder when there are changes in the policy regime or changes in the relative significance of the transmission channels, and here we surely have those problems. Thus, I was thinking that a structured model that imposes some theoretical underpinnings on the transmission channels would be more appropriate in this context.

I leave the authors to consider whether they would like to use another type of model to confirm the results presented today. Thank you.