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Comments on Chinn (2013)

(Preliminary)

I. Introduction

The paper deals with a crucially important issue of the effects of the unconventional monetary policy (UMP) of the major central banks, especially the FED. Its main focus is on the global spillovers of UMP, particularly the impact on the exchange rates and other asset prices in the emerging economies.

The paper:

1. Discusses the possible effects of UMP with the reference to existing theories (models) - sect. 1, 2, 3, 4.
2. Surveys some empirical literature on this topic – 5.1, 5.2.
3. As the author's own contribution to the empirical research, it uses a series of simple VAR's to assess emerging markets' response to UMP by FED.
4. Discusses the overall impact of the UMP on the economies of both advanced countries, which are applying these policies and other countries which are subject to spillovers from these policies – sect. 6 and 7.

We will comment on Chinn's paper in this order.

II. The impact of non-conventional monetary policies on rates of exchange in the light of existing theories (models)

This section raises several questions or objections:

- The paper claims, that " *Before 2007, it would be fair to say that most macroeconomists (who believed in the relevance of monetary policy) held the belief that once the zero lower bound was encountered, monetary policy would be almost completely hamstrung*". [1] However, the consensus before the crisis seems to have been rather the opposite (more on this see, e.g. Walsh, 2009). There were plenty of papers which used new Keynesian

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[1] This claim appears already in the introduction.

analytical framework so as to prove that zero lower bound should not be a serious problem for credible and sufficiently determined central bank (see, e.g. Eggertsson and Woodford, 2003). Against this background, it is worth contrasting the aforementioned claim with the paper's remark from the section 3. that "*forward guidance typically exhibits extremely powerful results in New Keynesian dynamic stochastic general equilibrium (DSGE) models*."

- The paper states that: "*even as a consensus has developed that unconventional measures can have an impact on asset prices and economic activity, a formal tracing out the channels by which these effects occur has not yet been undertaken*". The first part of this statement overstates the consensus, especially with regard to the impact of unconventional measures on economic activity. Many economists believe (like Chinn) that these measures boost economic activity. However, there are also many economists (and it seems that their number is rather growing than declining) who are concerned about risks created by these measures in the situation when global economy badly needs more confidence (for more on that see the last part of these comments). The second part of these statement seems to be too pessimistic (see, e.g. Cúrdia i Woodford, 2011 and other applications of their framework).
- According to the paper "*given the increase in the money base due to quantitative easing has not been manifested in corresponding increase in money supply, this interpretation* [i.e. monetary interpretation of unconventional monetary policy measures' effects on exchange rate] *does not make sense*". However, later this claim is to some extend weakened and the section ends up with the statement: "*The monetary interpretation of the exchange rate effects can be resurrected if these measures – expanding the money base – are taken to signal future policy outcomes*". However, what, according to the paper, can resurrect the monetary interpretation seems to be quite standard view of monetary interpretation (see, e.g. Woodford, 2012).
- The paper states that "*there seems to be a dearth of results*" with regard to effects of forward guidance on exchange rate, results obtained using new Keynesian analytical framework (DSGE models). That statement seems to be exaggerated (see, e.g. Coenen and Wieland, 2004).
- The paper comes to the conclusion that: "*In order to explain exchange rate movements arising from credit easing, one has to apply to models that treat different bonds (of identical default risk) differently*". This conclusion is a direct consequence of the criticism, presented

earlier in the paper, of standard monetary approach to explain exchange rate changes. It seems to be too strong since this criticism is, as we stress above, debatable.

Finally we think that the Author should refer to other reviews and spell out what is his contribution.

III. The survey of empirical literature

The Author should also refer to the previous surveys of the empirical literature in order to specify what is his contribution to this subject. It appears to us that at least some of them are more comprehensive and detailed, especially regarding the impact of UMP on asset prices (see, e.g. Cecioni, Ferrero and Secchi, 2011, Habermeier et al., 2013 or Stone, Fujita and Ishi, 2011). The paper deals with this issue rather perfunctorily, although the issue is announced in the paper's title. Besides, other surveys do not only discuss more studies on unconventional monetary policy effects than the paper does but put also more emphasis on weaknesses of these studies. True, the paper recognizes, e.g. that these studies largely disregard the fact that the propagation mechanisms that operated before the crisis may have changed after its outburst. However, this is not the only weakness of these studies (more on this see, e.g. Cecioni, Ferrero and Secchi, 2011).

Also, the Author should, in our view, not only report the diverging findings of different empirical studies on the same topic (p. 10-13) but should try to explain the reasons for such differences (e.g. different assumptions, different models, different samples).

IV. The results obtained from estimated simple VAR's

As the Author rightly stresses the results obtained from estimated simple VARs are not robust. The estimated reaction in case of three out of four analyzed countries is either opposite than expected or not statistically significant. The assumption that the sample restricted to the post-crisis period is better suited to the aim of the analysis is questionable. The Author is right that *"one need not to assume that the pre- and post-crisis periods exhibit the same behavior"* when restricting the sample. However far more intuitive solution in this case would be to estimate the model on the whole available sample, but with non-linear structure enabling different reaction of exchange rate to monetary base changes before and during the crisis. This approach is widely used in empirical research focused on differences in economies' response to impulses in normal times compared to periods of economic slack. More importantly the methodology applied (i.e. three variable VAR) seems to be oversimplified and poses risk that the estimated relations, even if statistically significant, may be spurious and subject to omitted variables bias.

Notwithstanding these methodologies remarks, the conclusion drawn from the VAR exercise confirms the expectation that monetary stimulus produced by FED leads to dollar depreciation and to the efforts of some emerging economies to resist the resulting pressure on their currencies.

V. The overall impact of the UMP

This is, by far, the most important problem to which the Author dedicates only 3 pages. He makes a strong claim that the UMP pursued by the advanced economies is likely to benefit both these economies and at least some of the emerging countries. However, without excessive oversimplification one may reduce the whole argumentation of the paper to one relationship and two assumptions. The relationship is that unconventional monetary policy pursued by major central banks puts appreciation pressure on the currencies of emerging economies. This assertion makes sense and is quite well documented (also in the paper.) The first assumption is that there is considerable economic slack in major advanced economies (in spite of current account deficits), while there is near full employment or even excess demand and current account surpluses in emerging economies. This assumption is debatable[2]. It gives rise to the assertion that policymakers in these economies are likely to allow their currency to appreciate instead of attempting to offset the appreciation pressure with FX interventions or capital controls. And if they allow their currency to appreciate, global imbalances will be reduced.

On the top of that, the second assumption appears. The paper additionally assumes that domestic demand in emerging economies could benefit from improvement of economic conditions in major advanced economies. This line of argumentation is hardly new. It was developed, with quantitative analysis, already in the situation, where only Bank of Japan was facing a problem of the zero lower bound (see, e.g. Coenen and Wieland, 2003.). However, the quantitative analyses suggest that domestic demand in emerging economies could indeed increase but mainly as a result of interest rates cuts in these economies, aimed at resisting appreciation pressure put on their currencies by unconventional monetary policy measures undertaken in major advanced economies. Thus, the second assumption implies that central banks in emerging economies would not conduct monetary policy best suited for their countries without major central banks' interventions. Allowing for such an implication is at best patronizing.

[2] Economic slack in advanced economies may be apparent, since large part of capital used in sectors which overgrew before the crisis, cannot be used elsewhere (investments is largely irreversible). In turn, labor employed in these sectors may re-gain productivity only, if they move to other sectors. However, their reallocation is hampered by, *inter alia*, unconventional monetary policy measures (more on this see, e.g. Ciżkowicz and Rzońca, 2013.)

Most importantly, the Author's claim about the benefits of the UMP disregards the long list of risks created by unconventional monetary policy measures. They are analyzed in depth, e.g. by Borio (2012), Hannoun (2012) or White (2012). The paper merely states that "*The (understandable) fear is that such capital inflows* [i.e. caused by unconventional monetary policy pursued by major central banks] *will spark a credit boom-bust cycle. The choices are most stark for small open economies".* Yet directly after this remark it adds: "*However, the benefits of expansionary monetary policy outweighs the costs*". This strong statement is made without any evidence or argument.

The point is that the continued UMP is likely to create increasing risks to a longer-term growth, both in the advanced economies which pursue it and - directly and indirectly – for the other countries. These risks include: weakening the policy-makers' incentive to engage in structural reforms, slowing down the banks' and companies' restructuring, weakening the financial institutions which rely on the debt instruments (pension funds, insurance companies), the emergence of new asset bubbles and the risks related to the exit from the UMP. The benefits from the UMP are short-term but the risks and costs it produces, are likely to grow in time. The static, short-term models, like the ones discussed in the paper, are not capable of even considering these dynamic effects. However, to disregard the underlying reality with the help of hugely oversimplified models, is very dangerous. Wasn't it shown enough by experience of the 'Great Moderation'?

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