Long Run Macroeconomic Relations in the Global Economy
by Stephane Dees, Sean Holly, M. Hashem Pesaran, L. Vanessa Smith

Referee Report

In short, I think the paper is very interesting, and contributes to a literature that has remained surprisingly underdeveloped. However, it suffers of two main shortcomings. First, it is too much of a derivative of Dees, di Mauro, Pesaran and Smith (2007). Second, it needs extensive revisions in order to be made more digestable and readable. In primis, it should be trimmed significantly.

More detailed comments

1) The result that I found most interesting is the apparent support for the UIP condition in a number of countries. This is in stark contrast with the conventional wisdom that usually finds an overwhelming failure of the UIP condition empirically. The authors should discuss at more length the reasons for their different results. Does that simply mean that the main problem with the usual practice of testing the UIP was to test it always on a bilateral basis? What would be the results then of simply testing the UIP for country "i" by including the trade-weighted nominal exchange rate and the trade-weighted foreign nominal interest rate? Would that suffice? Or is it rather the enlarged VAR structure imposed in the model that really makes the difference? I would like to know much more on this point.

2) Is a trade-weighted measure of the "foreign" nominal interest rate the right measure? Shouldn't some measure of "financial" trade weights in this case be more appropriate?

3) There is one equilibrium relationship that is critical in baseline open economy DSGE models with complete markets, and that the authors choose surprisingly to disregard; namely, the risk-sharing condition (RSH). This implies a direct link between relative consumption across countries and the real exchange rate, and is a result of the agents pooling risk internationally. Like the UIP condition, the RSH condition is usually subject to fierce criticism in the literature, and similarly seems to be overwhelmingly rejected by the data (the so-called Backus and Smith puzzle). I would like the authors to discuss why they chose not to analyze such condition, and would probably like to see an analysis of the RSH condition included in a revised version of the paper.

3) In the introduction, the authors discuss the connection with the open economy DSGE literature. Yet the system of long-run relationships (2.3)-(2.7) features long-run conditions which are either a somehow ad-hoc modification of the strict theory-based relationships (e.g, the PPP condition modified for Balassa Samuelson), or completely disconnected from the DSGE model (like the Solow Swann condition 2.4). Since the authors discuss the consistency of those relations with the DSGE model of Gali and Monacelli (2005), they should show exactly what relationships are drawing from that model (or from a baseline model in general), and which they are not. (For instance, there is no Solow-Swan condition in the GM model).

Although I understand that coherence with the data requires sometimes a bit of stretching of the theory, I think we should be more careful before claiming such a tight connection between the empirical reduced form model and the allegedly structural DSGE model. This is in another point the authors should discuss at more length.