Referee report on the paper

"Uncover Latent PPP by Dynamic Factor Error Correction Model (DF-ECM) Approach: Evidence from five OECD Countries"

Content of the paper

This paper develops a two-stage multivariate econometric approach to test for purchasing power parity (PPP) among OECD countries. Standard exchange rate theory and also the majority of conventional econometric analyses take a bilateral perspective. They generally refer to only two countries, where the domestic country faces just one foreign country despite the multilateral relationships of exchange rates in reality. The approach proposed in this paper extracts in the first stage latent disequilibrium factors from a larger set of bilateral deviations from PPP. In the second stage, these factors are taken as error correction terms in the explanation of subsequent exchange rate developments (and inflation). The paper finds empirical support for PPP and a much better performance in its exchange rate equations than "standard" econometric approaches.

Significance of the contribution

Overall, the results are potentially significant. To my knowledge, this is the first paper that proposes this mode of analysis (used in other fields) in the empirical testing of purchasing power parity. For this reason, it is also interesting to assess the performance of this econometric approach with standard econometric approaches in the field. However, given some shortcomings in the actual implementation it is difficult assess the contribution on the basis of the current paper (see comments below)

Quality of the analysis

While the paper offers an interesting and somewhat fresh perspective on exchange rate modelling, the actual version has a number of important weaknesses and shortcomings:

- in the introduction the authors mention in their review of the literature just two (!) main strategies to rescue the explanatory and predictive performance of the PPP hypothesis with respect to exchange rates. No reference was made to the multivariate approaches using panel data which is surprising given the per se multivariate focus of this paper. It would be good to put this paper into perspective with regard to that string of the literature.

- the formal presentation of the method of investigation in section 2 starts in equation (1) in a somewhat unprofessional fashion. Why do you need in the bilateral case a subscript "d" which seems redundant at a first glance? The following explanation in the text is rather imprecise: What means "the exchange rate between the two economies denominated in the domestic currency". Does this mean the units of domestic currency per unit of foreign currency or vice versa? The US dollar is the reference currency in the empirical analysis such that the exchange rates are units of domestic currency per US dollar. But then equation (1) does not make sense. Why not use superscripts for countries

and subscripts for time. This would clearly simplify the notation of (3) and (3a) and so on.

- in the verbal presentation, the paper remains in many instances clearly too vague and too general in its descriptions. For example, on p. 6 it is plainly stated "Once the specific model is obtained, it is further simplified mainly through reparametrization. Here, special attention is paid to the constancy of coefficient estimates, especially the feedback coefficients". In an econometric paper with this methodological orientation one would definitely like to get clearer guidance concerning the mode of analysis. Later, on p. 9, the authors turn a bit more specific, but still the description remains unsatisfactory.

- on the conceptual side, the authors remain completely silent on the implications of generated regressors in their multi-stage setting. Their approach with generated regressors ought to affect standard errors and test statistics at subsequent stages which definitely needs further methodological discussion.

- in the presentation and discussion of results, I am *not* convinced that it is necessary to compare the results for *all* countries on the basis of monthly *and* on the basis of quarterly changes. This produces a wealth of additional information in the tables, graphs and so on without adding much substance to the qualitative conclusions of the paper (see appendix). My recommendation is to stick to the monthly changes as this avoids problems of overlapping observations and conforms better the ECM interpretation even at the price that the explanatory power is usually higher in the models using quarterly changes.

- in my understanding and in comparison with standard approaches the specification of the ECMs in the paper beginning in table 6 is of somewhat "hybrid" form. In a standard ECM conventionally testing for bilateral PPP, the change in a *bilateral* exchange rate would be regressed on an error correction term based on the *bilateral* PPP relationship. An analogous investigation could be performed on the basis of the effective exchange rate. However, in the ECM in the paper such as in table 6ff, the bilateral exchange rate with respect to the dollar is regressed on a multilateral (effective) exchange rate. The assessment of the power of the current methodology needs as a standard of reference a conventionally specified ECM (and not a "hybrid" one.

Apart from these more substantial points the authors ought to consider in their revision the following "minor" points:

- concerning the verbal presentation the paper definitely needs further polishing, as it sometimes contains incomplete sentences or somewhat casual statements. Just two of many examples: From page 6: "Some are from the ECM procedure and the others from the DFMs". Or on p.8: "One is month rate and the other quarterly rate". Single words are missing in a decent number of sentences.

- the paper has a considerable number of printing errors; definitely too large to be enumerated in this report.

- the paper ought to adhere to conventional standards in the citation of papers.