

Response to the Co-Editor in charge for MS 794

Thank you for kindly reviewing our paper, 'Globalisation Effect on Inflation in the Great Moderation Era: New Evidence from G10 Countries'. The following is our response to the five points you raised.

1) Could the authors please check, and reference, the following paper by Gaiotti: Gaiotti, Eugenio, 2008. "Has globalisation changed the Phillips curve? Firm-level evidence on the effect of activity on prices," MPRA Paper 8389, University Library of Munich, Germany. (if I am not mistaken, the paper has been also published in a journal.)

Thanks for the recommendation. We have read this paper carefully but cannot see how it is directly related to our paper. Although the title seems to be related but Gaiotti's paper uses micro firm-level data from one country only while we use aggregate time-series data from several countries. We do not want to offend you by not following this advice. If your journal has a specific rule for any forthcoming papers to cite a minimum number of papers previously published in your journal, please let us know and we would not mind to comply with it. But we feel that it should be the authors' right to decide what papers to cite.

2) page 4, the expression "the device becomes awkward" is itself awkward, also the term "device" referred to an economic model

Thanks for this point! We have rephrased the sentence.

3) Why do the authors use year-on-year, rather than quarterly inflation? Does this not induce spurious autocorrelation in the data?

There are mainly three reasons: (a) it suits the convention of how inflation is used in the media and by governments. When inflation statistics are published, most countries refer to the y-o-y rates. (b) The dynamic frequencies of y-o-y inflation fit closer to unemployment rates. Sadly, many applied modellers fail to realise this fact. (c) Most of the previous studies that our paper refers to use y-o-y rates. We do not understand why y-o-y inflation would induce 'spurious' autocorrelation. The term 'spurious correlation' as originally from Yule over a century ago does not apply here at all.

4) It is not clear to me why the authors want to estimate the model country by country. If coefficients are different across countries, why not use the Mean Group (and/or possibly the Pooled Mean Group) of Pesaran et al?

We think that we have explained it clearly in section 2 (see the last paragraph of that section). It is not only a matter of different coefficient estimates, but also different dynamic responses, i.e. n in equation (2) is different for different countries, as well as different the long-run static relationships. The latter makes it impossible to aggregate. We were planning to be able to aggregate some countries, but the modelling result rejects the plan.

5) Even if the authors want to stick with the country-by-country estimation, the tables need to be much clearer and more self-

contained, so that the reader does not need to go back to the text to understand them. This comment refers in particular to Table 4, where it is not clear what coefficients represent, there are font and spelling errors, etc.; and Table 5, where it is not reported what the scenarios are.

Thanks for this point and we have made some format correction to Table 4 but we fail to find any spelling errors. We actually tried hard to design the tables while writing the paper. We took the pattern of Table 4 from Pain *et al* (2008) OECD paper because their work is closest to our approach and their table design is also the best way of presenting dynamic model results we can find from the literature. The title of Table 4 gives the model numbers where the coefficients are defined.

As for the scenarios reported in Table 5, these are defined in the penultimate paragraph of Section 3, where Table 5 is first mentioned in the paper. We have now added this information in the footnote of Table 5 as well.