

Interactive comment on “Stratospheric Variability at a glance – Analysis of the intra decadal timescale and the QBO” by Duy Cai et al.

Anonymous Referee #2

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The authors present several good quality plots that would be of interest as a kick-off to a model intercomparison activity. However, the basic approach of using Hyashi spectrum / Wheeler and Kiladis style analysis has been widely applied to different fields from surface precipitation to top-of-atmosphere OLR as well as vertically varying levels. I myself considered it a sufficiently well-trodden route to suggest that a 2010 summer placement student follow published work of Ern et al. to investigate the propagation of Kelvin waves in two different resolution versions of our GCM and start with spectral plots such as these at varying heights in the model stratosphere, which suggests to me that such use is fairly commonplace.

Refs: M. Ern et al., Atmos. Chem. Phys., 8, 845–869, 2008 M. Ern and P. Preusse, Atmos. Chem. Phys., 9, 3957–3986, 2009

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Discussion paper



I think the authors would have to concede that either of these papers is considerably more substantial than the manuscript submitted here.

If the aim is to keep the paper short (and as a reviewer I have no objection to that) it really does need to have more impactful (well-supported and focused) conclusions than the authors presently offer. Starting an introduction with the statement that the QBO is a dominant source of internal variability in the tropical lower stratosphere would be fair enough: presenting it as a conclusion appears less than insightful.

Likewise, the very basic statements that QBOs depend on waves on a range of scales and that vertical resolution will impact the representation in models were made around the time of the 2001 Baldwin et al. review and can hardly be seen as 'novel' today.

The precise statement that your manuscript supports would be that "Given the horizontal resolution of our EMAC configuration a vertical resolution longer than about 2km is not sufficient to sustain a QBO at 30hPa whereas a resolution about 1km appears to be." This is a much more qualified and model specific statement than you make on several occasions but actually it could be a good starting point for extending the work in one of a number of directions, such as investigating other levels in your model to distinguish issues of wave generation versus propagation, mean flow structure, looking at interactions between resolved waves and any subgrid parametrizations (you say nothing about these), further variation of resolution to refine the transition and even making comparisons against other models.

In conclusion, I agree with the authors that there is interest in the general topic of modelling the QBO and understanding the processes by which models arrive at their diverse representations but I would encourage them to ask what is the key message with which they would like to really catch a reader's attention in this manuscript, whether they choose to tighten or expand it, and what information about or from the experiment really adds to that core message that they want people to buy in to.

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