Response to Reviewer #2

We appreciate the reviewer's constructive comments.
The following is our point-to-point reply to these comments.

Twenty-three CMIP5 models are investigated for their match with observations in representing aspects of tropical intraseasonal and interannual variability. Despite the title, which emphasises the relationship between interannual and intraseasonal variability, the majority of the paper is first spent analysing which models are best at simulating individual aspects of the variability, namely the two types of ENSO, the MJO, and Equatorial Rossby and Kelvin waves. The results show a large variety of behavior from the models, with very few models showing variability and relationships like observed. This may be of interest to model developers, but I don't think it adds much new insight into the dynamics of the observed variability. Also, I can't see how these results can help pin-point what aspects of the models need to be changed for improvement. I understand this is a difficult task, but is one that needs to be done to help improve the models.

We agree with the reviewer that our paper does not propose or suggest ways to improve the models. It is an evaluation of the realism of the ITV/ENSO relationship, and as such, the paper can be viewed as a preliminary step towards suggesting improvement in the model physics, considering that it proposes a physically-based metrics to evaluate models and classify them into two broad classes, the less and most realistic ones. Such a "classification" could be the basis for identifying differences in some key dynamical aspects of the ITV, like the energy sources of the ITV (i.e. extra-tropical disturbances, tropical instabilities or non-linear interactions of multiple waves), its coupling with SST, its seasonal phase locking etc.
Despite the limitation of not addressing issues on model development, we believe that our results may still fit with the scope of Geoscientific Model Development since it provides "*new methods for assessment of models, including work on developing new metrics for assessing model performance and novel ways of comparing model results with observational data*", as well as proposes "*novel ways of comparing model results with observational data*"

1. The English grammar needs improving to make it easier to read and understand.
For example, there are many instances where the word "the" is inserted incorrectly or missing.

We have thoroughly revisited the text and improved the English grammar

2. Page 3, line 24: Kim and You (2012) missing from reference list.

Added to the reference list

3. Page 5, line 6: “PI” is not defined.

Pre-Industrial – corrected

4. Section 2.2: It is noteworthy that you are using zonal wind data instead of a proxy for clouds and convective rainfall (e.g. outgoing longwave radiation) as used by Wheeler and Kiladis (1999). This means that the variability highlighted by your wavenumberfrequency analysis (Figure 3) is somewhat different to that highlighted in Wheeler and Kiladis (1999). It also means that the variability you show and isolate is not necessarily ‘convectively-coupled’. For example, Figure 3 indicates the existence of the global Rossby-Haurwitz waves for low westward-propagating wavenumbers and periods around 5 days. It also means that the convectively-coupled equatorial Rossby (ER) and Kelvin waves are much less clear in Figure 3. This means that your filtered fields will also contain a much greater mix of variability compared to Wheeler and Kiladis. Finally, I note that you use rectangles to define your regions of filtering instead of following the dispersion curves for the equatorial waves. Ideally you should change your fields and filtering to better match the characteristics of the waves. However, I support the use of the western Pacific wind indices later in the paper as this is consistent with the findings of Hendon et al. (2007).

In the revised manuscript, we better justify the use of U850 field for deriving the ITV components:
“We use here the U850 field for deriving the various components of the ITV instead of Outgoing Longwave Radiation (OLR) or brightness temperature signals from satellite data noting that the regions in the frequency-wavenumber domains where the spectral energy peaks are similar for OLR and U850, which is also predicted by a simple dynamical model of ITV (Thual et al., 2014). Moreover the use of U850 eases the interpretation of the results since it is the westerly wind anomalies that serve a physical conduit from the ITV to the ENSO dynamics. This approach follows previous relevant studies (McPhaden et al. 2006; Hendon et al. 2007).”

In the paper, the focus is on two component of ITV – MJO and equatorial Rossby (ER) wave which were shown to be associated to El Niño development (McPhaden et al., 2006, Hendon et al., 2007, Gushchina and Dewitte, 2011, 2012). The Figure B1 provides the wavenumber-frequency spectra for both OLR and U850 and it can be seen that, the domains where the MJO and ER spectral energy peaks are comparable for both fields. The MJO spectral maximum in OLR is shifted to the higher zonal wave numbers as compare to U850 in accordance with the results of previous studies (Zhang, 2005).

For example, Figure 3 indicates the existence of the global Rossby-Haurwitz waves for low westward-propagating wavenumbers and periods around 5 days. It also means that the convectively-coupled equatorial Rossby (ER) and Kelvin waves are much less clear in Figure 3. This means that your filtered fields will also contain a much greater mix of variability compared to Wheeler and Kiladis.

The frequency band for MJO filtering is 30-60 days and for ER – 10-50 days, thus we do not include in our filtered fields the waves with 5 days period. Overall the main difference between OLR and U850 spectra is located at the periods shorter than 10 days, which does not impact our results. We have improved the presentation of figure 3 (new figure 2) so as to better visualize the MJO and ER domains. We applied the same color scale as in Hung et al. (2013) for easing the comparison with their results.

Note that we use rectangles to define our regions of filtering only for MJO as in Wheeler in Kiladis (1999). For Rossby wave following (Wheeler and Kiladis, 1999) we follow the dispersion curves for the equatorial waves with equivalent depth ranging from 8 to 90 m. This is now mentioned in the text of the revised manuscript. “For Rossby waves, the frequency-wavenumber bands is also limited by the dispersion curves corresponding to values of the atmosphere equivalent depth ranging from 8 m to 90 m, which follows (Wheeler and Kiladis, 1999)”

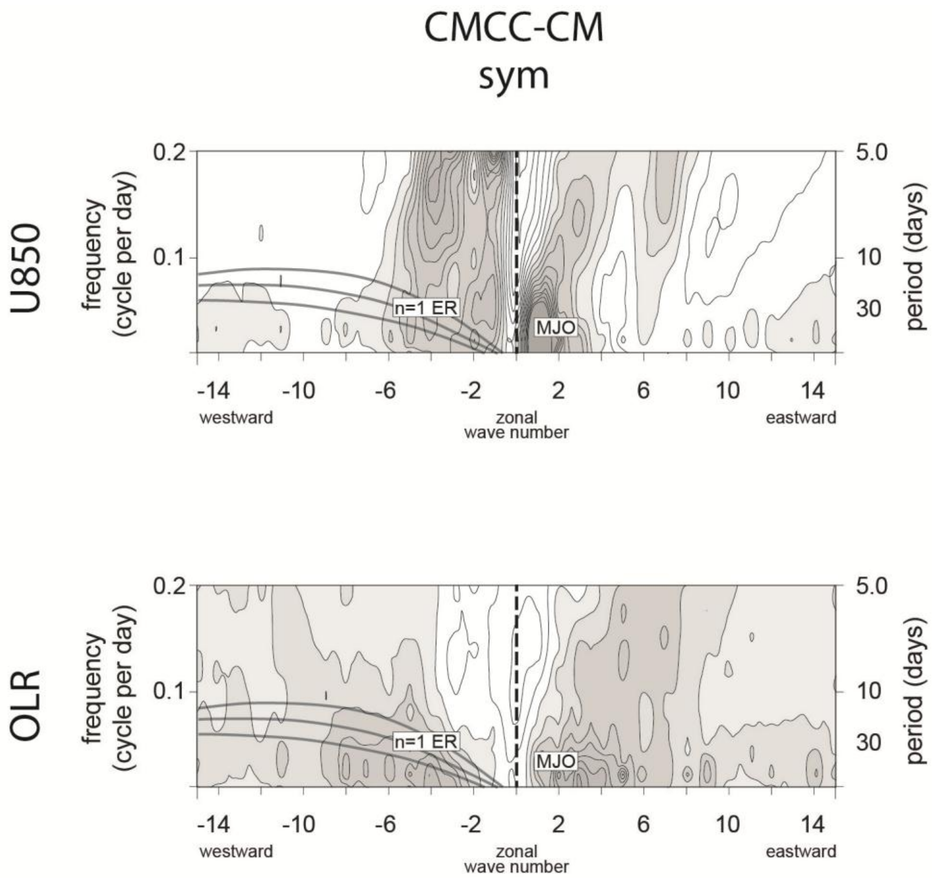

Figure B1: Space–time spectrum of the 15°N–15°S symmetric component of U850 (upper panel) and OLR (bottom panel) divided by the background spectrum for CMCC-CM model

5. How are the values in Table 3 calculated?

We agree with the reviewer that the estimates presented in Table 3 are rather uncertain. We have removed this Table and added to the revised version a figure that is comparable to figure 9 of Hung et al. (2013) and that indicate the main values of expected phase speed (figures 7-8).

6. Page 13, lines 13-15. This is poor style for scientific writing. Please refer to this paper: http://onlinelibrary.wiley.com/doi/10.1029/2010EO450004/full

We have improved the style, which should ease the readability of the revised manuscript.