

Interactive comment on “The relationship between intraseasonal tropical variability and ENSO simulated by the CMIP5” by Tatiana Matveeva and Daria Gushchina

Anonymous Referee #3

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General Comments

The authors attempt to relate the ability of CMIP5 coupled models to simulate ENSO with their ability to correctly simulate the seasonal cycle and coupling between atmospheric intraseasonal equatorial waves, namely the MJO and Equatorial Rossby (ER) waves and the ocean. While the observational support for such a relationship in the real world has been well-established by the authors and others, unfortunately most of models studied here appear to only marginally simulate such relationships. The physical relationship between the zonal wind variability and ENSO in the models has not been explored in detail, therefore in my opinion the paper should be revised to include

C1

more diagnostics.

Specific Comments

The paper starts out with an interesting and useful analysis of the behavior of ENSO in the models. However, I did not get a sense from this manuscript of which aspects of the models lead to bad (or better) representation of ENSO. For example, no indication of the oceanic response to wind forcing has been shown. Is it possible that the ocean models might also be an issue? In the end, it seems to me that a paper like this one in a journal such as GMD should lead to some recommendations on how models could be improved. Have the authors checked any of the ocean data (TAO buoys, SODA reanalysis) as in Gushchina and DeWitte (2011) to see whether the wind signals they are isolating are actually related to oceanic Kelvin waves? Otherwise trying to relate ITV to ENSO seems speculative. In a better model such as CMCC-CM there should be a realistic relationship between the wind forcing and the ocean response. I encourage the authors to expand on these points in a revision.

The other major issue has to do with the isolation of CCEWs. The authors are using broadly defined filters that are based on OLR or brightness temperature signals from satellite data. Based on the spectra in Fig. 3, there is little basis for using the filter bands they have chosen, which ultimately derive from precipitation signals. Talking about “waves” such as the MJO and ERs in Figs. 7 and 8 is very suspect, since filtering of just red noise will give you similar results. I suggest more diagnostics to establish the existence of zonal wind signals associated with the MJO and ER waves (see below).

Technical Comments:

Pg. 2, line 21: see also Keen, 1982 Mon. Wea. Rev. pg. 1405.

Pg. 3, line 14: “loose” => “lost”

Pg. 5, line 16: except that Lin et al. and Hung et al. used precipitation not wind, this

C2

needs to be pointed out here.

Pg. 6, line 3: "the maximum of ITV/ENSO relationship is observed." It is not clear what you mean by this. Precisely how are the indices in Table 2 defined? Please provide more detail on this, perhaps by using the "NCEP-NCAR" data as an example, which should be the closest to reality. Also, only 4 models are shown in Table 2 yet other models are analyzed later.

Pg. 8, Line 8: The spectra in Fig. 3 should be replotted, since it is difficult to see the signals through the dispersion curves. In particular, the MJO peak should show be a wavenumber 1 signal but these are obscured by the dispersion lines.

A comparison with Hung et al. for those models that have overlap would be welcome. If I look for example at CanESM2 and CCSM4 spectra of rainfall in Hung et al., it seems that these two models have a good spectral peak for the Kelvin wave in rainfall, but there is no evidence for a corresponding zonal wind peak in Fig. 3. This just illustrates the problem with using wind to define the equatorial wave modes as used here.

Line 15: "lower" suggest "weaker"

Pg. 9, line 9: "The maximum. . ." I have no idea what this sentence means to say.

Pg. 10, line 1: I guess the periods chosen for Fig. 7 are chosen from random, but perhaps the authors looked for good examples from the reanalysis and each model? More detail is needed, including making the obvious but necessary point that the model fields in no way are expected to match the reanalysis or each other.

It is not clear where the statements on propagation velocity and intensity come from. These are only one year periods, and it seems that the authors are just making statements by visual comparisons between the plots. The characteristics of the waves in each model could be compared with reanalysis by using diagnostics of the type used by Wheeler et al. 2000 (J. Atmos. Sci. pg. 613) or Hung et al. (their Fig. 9). The characteristics of the "waves" identified here overwhelmingly determined by the filter-

C3

ing, which sets the phase speed in particular. The plots in Fig. 8 are great examples of getting "something from nothing" by filtering: The CCSM4 zonal wind spectrum in Fig. 3 shows no signal at all for ER waves, yet Fig. 8c shows lots of westward propagation, which must come primarily from the constraints of the filter. There are lots of other examples of this.

Pg. 11, top: Much more discussion of what the expected relationship between the zonal wind and ENSO is needed here. Although the authors refer back to Takahashi (2009) and their own previous work, it will not be immediately obvious what Figs. 9a and 10a imply for ENSO forcing. The caption for Fig. 9 does not help. A brief review of the concepts is needed at the start of Section 3.3.1 before Figs. 9 and 10 can be interpreted.

Unfortunately, the model results from Figs. 9 and 10 are not very impressive, with little statistical significance indicated. Also, it is difficult to even tell the sign of many of the signals, so I suggest using more color.

Pg. 11, line 9: Indian Ocean wind stress could not force ENSO.

Line 10: 9g => 9h

Line 21: Here it is difficult to even tell what the sign is in Fig. 10h.

Pg. 12, line 2: Puy et al. 2016

Line 15: I wouldn't say it's "very close", but certainly it's better than the rest. To be honest, since Figs. 10-12 are based on the models' own renditions of ENSO, the huge disparity between them leads to an obvious conclusion: what forces ENSO in most of these models is something different than what forces it in the real world. I think this is the statement you should make more forcefully.

Pg. 14, line 7: "The deficiency of INM-CM4. . ." Little basis for this statement is shown. More detailed diagnostics of the ocean response would bolster claims like this, even by using SST if sea surface height or thermocline depth cannot be obtained.

C4

