Geoscientific Model Development Discussions

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Comment on gmd-2021-381

Anonymous Referee #2

Referee comment on "Improving Madden–Julian oscillation simulation in atmospheric general circulation models by coupling with a one-dimensional snow–ice–thermocline ocean model" by Wan-Ling Tseng et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-381-RC2, 2022

This paper incorporates a one-dimensional ocean mixed layer model into three atmospheric models, ECHAM5, CAM5 and HiRAM. Specifically, the Madden-Julian Oscillation (MJO) is significantly improved in these three coupled models due to a more realistic simulation of SST variation. The coupled simulations can correct the surface latent heat flux biases during the preconditioned MJO phase over Maritime Continent (MC). The change of meridional circulation during the strong convection phase also control the improvement. In general, this manuscript clearly shows the atmospheric dynamics associated with the enhancement of MJO regardless of model configurations/physics. The budget analysis also details the relative contribution. However, the fundamental driver from the coupled air-sea interaction process which changes the boundary layer through the SST update is still unclear. The authors may have to comment on this further. Finally, the English usage needs further improvement. Careful proofread by a native English writer is required. This paper is appropriate to be published in GMD after considering the following comments.

- Throughout the manuscript (including the abstract), the use of CWBCFS is mentioned several times. However, we do not see the results until Fig. 11 (section 3.4). The description is also very minimal (one paragraph). Unless more discussion is included, I suggest to remove all discussion about this model results which cannot add any new information in this study.
- Introduction: line 56-59, what's the meaning of this sentence? "MJO and oceanic wave are also suggested"? What? Do you want to say they are related? This sentence has to be rewritten.
- Line 66, suggest to remove "evaluating the mechanism of ocean-atmosphere coupling" since the following description is to discuss the mechanism of ocean-atmosphere coupling already.
- Line 73-79, this sentence is unclear, particularly after "Such as". The whole sentence needs to be rewritten.
- Line 96-98, this sentence does not have a verb.

- Section 2: I suggest to separate into two subsections. “2.1 Observation and atmospheric/oceanic data” and “2.2 Model experiments” to better clarify the information.
- Line 103-Line 113. This paragraph describes the observational results used here. However, it is not easy to read. Also some information is unclear. What variables are used from ERA-interim since ERA-interim also has precipitation and outgoing longwave radiation? Also, the time periods used look different. Please clarify. What about the oceanic GODAS forecast and TOA array data? What time periods do you use? I suggest the authors to systematically list different datasets. True observation and model data should be clearly separated. Don’t mix them all together.
- The only difference between the coupled and uncoupled simulation is the update of SST. The uncoupled simulation specified the SST, however, the coupled version updated the SST at every time step. Is this correct? Does the coupled simulation feed other variables back to the atmospheric component?
- Line 115-116, “variations in the SST and upper-ocean temperature, including the” change to “upper-ocean temperature, including the SST, ” If your cool skin temperature is SST, you can skip “the SST”.
- Line 119, remove “a”.
- Line 123-127, the resolution used in CAM5 and HiRAM need to be described. Also, the boundary layer schemes used in these models should be briefly described to comment on the different boundary layer schemes used here in the coupling.
- Line 127-130, remove this model description since it doesn’t add any new information while no results are presented until Fig. 11.
- Line 133, is this 0.05mm your finest resolution at the top? Does the resolution increase with depth? Also, you have 12 layers in the top 10 m. How many are within the top 1 m so resolve the diurnal warm layer?
- Line 137, to my understanding, the top layer of GODAS is 10m. Do you mean you do not nudge the first top value (i.e., SST) but the values below. So you want to mimic the observed SST (from 1985 to 2005) but not the sea surface dynamic, right? I suggest to include a new plot showing this upper layer feature comparing to the OISST. This may be an important plot to show the major forcing difference on the atmospheric model.
- Line 138, if this is the case, all atmospheric models use the same time step? I believe these three models have different resolutions. So do you control the time step on purpose?
- Line 140, “prescribed climatological monthly mean SST” do you mean “prescribed monthly mean OISST”? If so, it is better to clarify this.
- Line 145-148, I suggest to remove the CWBCFS description and comment on this at the summary and discussion section.
- Figure 1: please include units in the caption or on the figure.
- Figure 2: please include the units in the caption or on the figure.
- Line 191-194, are Figures S1-S3 very important plots? If so, why they are on the supplementary figures? If not, why do the author discuss them at the beginning? What’s the purpose of putting this sentence?
- Line 200, change “MJO” to “MJO event”.
- Figure 3: what’s the purpose of showing this Figure? Do you want to imply the heat sources are not the key for the MJO development (because CAM5 v.s. CAM5-SIT does not have the corresponding change)?
- Line 213: How can you justify this is an intensified Kelvin wave-like perturbation? Can you identify the wave propagation or forcing?
- It is very interesting to see this large difference occurs just above the MC. This region includes both ocean and a large area of land. Particularly, the ocean is very shallow in general. Can you comment on this topography feature on the large impact of the coupling?
- Figure S5 seems to be a very important plot for the SIT to resolve the upper 10m ocean. However, the warm layer change look different among these three coupled models. The only consistence I can tell is the SST at different phases (which are used

by the atmospheric models). Can you also include the SST at different phases used to force the uncoupled model for the comparison? This may be a major difference in the forcing.

- Figure 6, please label the units.
- Line 260-262, this sentence is for the discussion next section, right? If so, please change "We diagnosed" to "We next diagnosed".
- Line 275, what do you mean by "smaller LH negative"?
- Figure 7, can you comment on the residual term within the observation? Why is it larger than many other terms? Also, the budget analysis suggests the enhanced LH plays a major role on the correction. However, the only difference is the change of SST (coupled SST has a different value from the specified SST in the uncoupled simulation, is that correct?). How does the change of SST modulate the change of LH?
- Figure 8, please label the units in the caption.
- Line 288, Is this really the equatorial Kelvin wave? If so, can you clarify the wave speed of this Kelvin wave?
- Figure 9 and 10 suggest the dominant role of meridional advection moisture term. Does that imply the instantaneous SST horizontal distribution plays a key role on this change due to the coupling effect? Then, the change of varying moisture induces the intraseasonal circulation change.
- Figures S6 and S7 also suggest the background mean states are not the key contributor for the enhancement. Can we conclude that the change of SST distribution indeed the main driver for the enhancement. However, many other coupled models which cannot resolve the surface warm layer show the coupling with ocean may make the MJO simulation worse. Can you further quantify the key role of resolving the surface warm layer on the resulting SST which consistently change the models' boundary layer? Diurnal cycle of warm layer? Or others?
- Discussion section: the fundamental driver from the coupled air-sea interaction process is still unclear from this manuscript. However, that is the major point of bringing the coupling of resolving the ocean surface warm layer. The coupled results change the boundary layer through the SST update. Can the authors comment on this and provide some further guidelines how the modelers may improve the MJO simulation practically through this approach?