Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC1
https://doi.org/10.5194/gmd-2021-346-RC1, 2022

Comment on gmd-2021-346

Anonymous Referee #1

Referee comment on "Embedding a one-column ocean model in the Community Atmosphere Model 5.3 to improve Madden–Julian Oscillation simulation in boreal winter" by Yung-Yao Lan et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-346-RC1, 2022

This manuscript focuses on the development of a global coupled model on forecasting MJOs. The propagation of MJOs along the equator can significantly affect the precipitation in many regions, so the relevant model works have been devoted by many previous studies. I appreciate the authors' efforts for continuously improving the model forecast on this multi-scale weather system. Unfortunately, one thing I am trying to find in this manuscript is their unique contributions to the broad society. According to the title, it seems like the authors feeling confident in the usage of a 1-D SIT model for predicting MJOs. At the end of Introduction, the authors barely mention their motivation is to "examine how air–sea coupling can improve MJO simulation, especially that of the eastward propagation that has been poorly simulated in many climate models". Because many global coupled models use the 3-D ocean models, the connection between the title (1-D SIT model) and motivation (effect of air-sea coupling on MJO propagation) is unclear. Are the authors trying to convince readers the effect of 1-D model enough for the forecast? Or is there anything special inside the SIT model? The importance of air-sea coupling should have been extensively emphasized and agreed by many studies, and I do not think any ongoing research still trying to use a global model without ocean parts. Repeating the work may be meaningless. I believe their motivation needs to be rewritten.

On the other hand, because the authors introduce some models unable to simulate the MJO propagation reliably, I believe one of their expected results is to improve the motion of MJOs (also mentioned in the motivation). However, it seems like the authors do not summarize how much improvements can be seen in their results, or which factors can affect the simulation the most. Because there are some interesting experiments inside this manuscript, such as the coupling regions, I do not think it should be rejected at this moment. However, the structure and quality of the manuscript are very poor. It is very close to my standard for rejection (too many things to be fixed). I only list some problems below, not all. I recommend a major revision for this work in this review.

- I do not think conducting an experiment for studying the difference between A-CTL and C-30NS is needed. In my point of view, we do not need another paper talking about the importance of coupling the upper ocean in the global models. In other words, please simplify the description in section 4.1. All you need is to show your coupled model sufficient for simulating the MJOs.

- I am super uncomfortable in the description of the ERA-interim results as the "observation". It is impossible to measure the global wind at 850 hPa directly. Besides, the precipitation data looks like a post-processed product constituted by many satellite measurements. It happens to the OISST as well.

- I think you need to reconsider your structure in the main text. There are some unnecessary and redundant materials that can be moved to the appendix or supplemental material. For example, you do not adjust the coefficients in the 1-D TKE closure scheme. Why do you need to describe the full equations? I also don't care about the numbers of depths from lines 207 to 212 (yes, your units are wrong).

- You do not need section 3, because people like me already forget the details when we are reading sections since 4.2. Please reorganize the structure.

- I do not think that section 4.2 is discussing the vertical resolution… It is more like the thickness of the first layer. A lot of information is missing here. For example, what is your surface mixed layer depth? If the surface mixed layer depth is less than 30 m or

10 m, what do you do for C-LR34m C-LR12m? Are you trying to test the effect of a slab model in your global coupled model?

- What do you mean “ocean bottom” at line 476? Is it seafloor?

- Rewrite section 4.6. I cannot understand which fluxes you are using.

- I cannot understand why the runs are 30 yr? What are the initial conditions of atmosphere and ocean? Is the forcing the same as the values in the real world from 1990-2020?