Dear Editors of GMD and dear Reviewers:

We greatly appreciate reviewer's insightful and helpful comments regarding our manuscript. The manuscript has been revised based on reviewer's comments. Below are the point-by-point replies to reviewer's comments and concerns, whereas our corresponding revisions in the manuscript (version R1) are identified by colored text. Specifically, red text indicates changes made in response to the suggestions from Reviewer #1, blue text demonstrates changes made according to Reviewer #2, and green text shows changes made to better clarify model descriptions in a clear, concise, and well-structured way. Moreover, we revised the manuscript carefully to ensure that it is grammatically and typographically error-free and hopefully meets the high quality standards of GMD.

Sincerely,
Yung-Yao Lan, Huang-Hsiung Hsu, Wan-Ling Tseng, and Li-Chiang Jiang

Anonymous Referee #1

The reviewer comments are formatted in italics and the authors response to the comments are formatted in bold.

Notation *RC1.P#* represents Reviewers Comment. Paragraph Number

RC1.general comment 1. 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.

Response:

Thank you for your comment. We did not attempt to argue that the effect of 1-D model enough for the forecast or simulation of the MJO; instead, we demonstrate that a 1-D model with high vertical resolution in the first 10 meters could have significant improvement. At the end, we suggested that using extra fine vertical resolution in the first few tens of meters of 3-D ocean model could further improve the simulation of the MJO. The improvement due to high resolution had been demonstrated using ECHAM5 (Tseng et al. 2014). This study demonstrated the same effect in CAM5 and suggested that the improvement is not model dependence. By coupling the 1-D SIT model to an AGCM different from Tseng et al. (2014), this study confirms the scientific reproducibility for the improvement of MJO simulation in modeling science.

We further explored the dependence of the improvement on various factors

such as coupling depth, frequency and domain that have not been explored in previous studies, and we considered our results valuable insights for the MJO simulations. We have revised the introduction and summary following the discussion above to state more clearly the motivation and contribution of this study.

RC1.general comment 2. 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.

Response:

Thanks for your suggestion. We summarized specifically in the original (and revised) manuscript what are the better settings and important factors for MJO simulations. We did not attempt to quantify the degree of improvement because it is likely model dependent. Nevertheless, the improvement is evident in many presented figures, e.g., the summarized figure (Figure 10 in revised manuscript) shown in the Summary. The findings are as follows.

(1) Better resolving the fine structure of the upper-ocean temperature and therefore the air–sea interaction led to more realistic intraseasonal variability in both SST and atmospheric circulation.

(2) An adequate thickness of the oceanic mixed layer is required to simulate a delayed response of the upper ocean to atmospheric forcing and lower-frequency fluctuation.

(3) Coupling the tropical eastern Pacific, in addition to the tropical IO and the tropical WP, can enhance the MJO and facilitate the further eastward propagation of the MJO to the dateline.

(4) Coupling the southern tropical ocean, instead of the norther tropical ocean, is essential for simulating a realistic MJO.

(5) Stronger MJO variability can be obtained without considering the diurnal cycle in coupling.

In general, upper-ocean vertical resolution and coupling with the southern tropical would be of relative importance compared to other factors for the eastward propagation of the MJO.

RC1.P1 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.

Response:

The purpose of the comparison between A–CTL and C–30NS was not just to demonstrate again that air–sea coupling could improvement MJO simulation. It also served as the basis for the evaluation of sensitivity experiments that tested the key ingredients for the improvement, in addition to showing that significant improvement in MJO simulation can be achieved by simply coupling a numerically efficient 1-D ocean model. For this purpose, the C–30NS experiment served as a control coupled experiment is essential. We therefore prefer to retain this experiment and relevant discussion, and hope for reviewer’s understanding.

RC1.P2 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.

Response:

Thank you for the suggestion. We modified the manuscript to mention directly the name of data used for comparison, instead of referring them as observation. Please see Page 11, lines 244, 247 and 260, Page 12, lines 272, 274 and 280 as well as section 3 with red text in the revised manuscript.

RC1.P3 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).

Response:

The comments are well taken. We have removed the background information about SIT and the units are corrected. Thank you for the reminder. Please see Page 7, lines 159-161 and Page 8, lines 162-180.

RC1.P4 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.

Response:

Thank you for the suggestion. We feel a brief discussion of experiment setups could be useful for completeness and the readers. Content of Section 3 is now moved to Section 2.3. The essence of each experiment was briefly mentioned again in other sections when relevant results were presented. Detailed information of each experiment is also presented in a table and in supplementary

material.

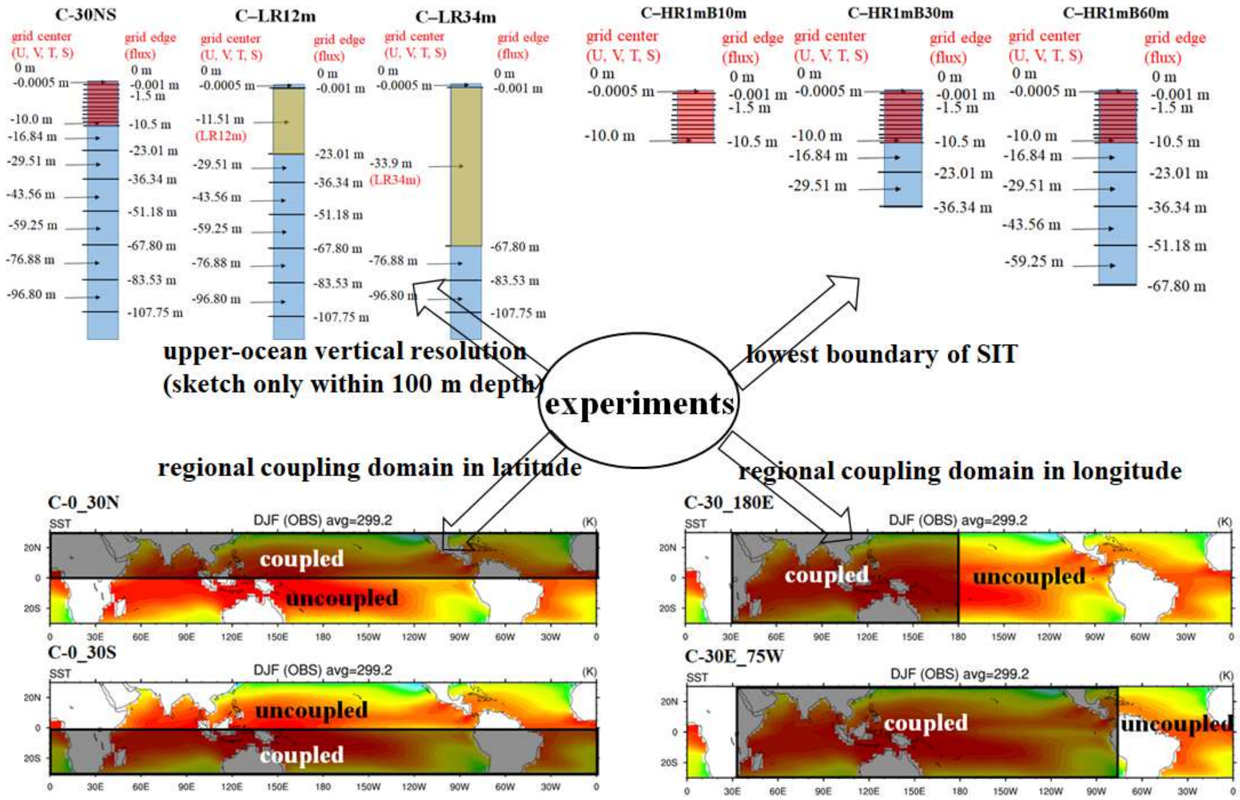

Fig. RC1.1 Schematic diagram of a series of 30-year numerical experiments.

Table 1. List of experiments

Section	Category	Experiments	Description
3.1	Coupled or uncoupled	A–CTL	Standalone CAM5.3 forced by forced by the monthly mean Hadley Centre SST dataset version 1 climatology
		C–30NS (the control coupled experiment)	CAM5.3 coupled with SIT over the tropical domain (30°S–30°N), with 41 layers of finest vertical resolution (up to the seabed) and diurnal cycle; the frequency of CAM5 being exchanged with CPL is 48 times per day
3.2	Upper-ocean vertical resolution	C–LR12m	The first ocean vertical level starts at 11.5 m with 31 layers (beside SST and cool skin layer are 11.5 m, 29.5 m and 43.6 m up to the seabed)
		C–LR34m	The first ocean vertical level starts at 33.9 m with 28 layers (beside SST and cool skin layer are 33.9 m, 76.9 m and 96.8 m up to the seabed)
3.3	Lowest boundary of SIT	C–HR1mB10m	The lowest boundary of SIT has a depth of 10 m (model depth between 0 m and 10 m)
		C–HR1mB30m	The lowest boundary of SIT has a depth of 30 m (model depth between 0 m and 30 m)
		C–HR1mB60m	The lowest boundary of SIT has a depth of 60 m (model depth between 0 m and 60 m)
3.4	Regional coupling domain in latitude	C–0_30N	Coupled in the tropical northern hemisphere (0°N–30°N, 0°E–360°E)
		C–0_30S	Coupled in the tropical southern hemisphere (0°S–30°S, 0°E–360°E)
	Regional coupling domain in longitude	C–30_180E	Coupled in the Indo-Pacific (30°S–30°N, 30°E–180°E)
		C–30E_75W	Coupled over the Indian Ocean and Pacific Ocean (30°S–30°N, 30°E–75°W)
3.5	Absence of the diurnal cycle	C–30NS–nD	Absence of the diurnal cycle in C–30NS; the CAM5.3 daily atmospheric mean of surface wind, temperature, total precipitation, net surface heat flux, u-stress and v-stress over water trigger the SIT and daily mean SST feedback to atmosphere; the frequency of CAM5 is exchanged with CPL 48 times per day

Experiment abbreviations: "A" means standalone AGCM simulation. "C" means the CAM5.3 coupled to the SIT model.

RC1.P5 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?

Response:

At the first sight, it may seem as reviewer suggested "more like the thickness of the first layer". Although we did not conduct different vertical resolutions within the first 10.5 meters, a comparison between three experiments did suggest that the extra fine resolution in the first 10 meters contribute markedly to the improvement. With a 41-layer vertical discretization in SIT model in the control experiment, 12 layers are located above 10.5 m and 6 layers are located between 10.5 m and 107.8 m. High vertical resolution is needed to catch detailed temporal variation of upper ocean temperature. To test the effect of vertical resolution, we conducted C–LR12m and C–LR34m without vertical discretization in the first layer (Figure RC1.2) to explore the impacts of fine vertical resolution on MJO simulation. This comparison showed that the simulated MJO became more realistic with increasing the upper-ocean vertical resolution. This result has an important implication for the further development of fully coupled GCM that often has the first oceanic layer as thick as 10 meters (e.g., POP2).

The SIT is not a simple slab model that usually has just one layer. As shown in Figure RC1.2, the model is as thick as 107.8 meters and with several layers between surface and model bottom. C–LR12m and C–LR34m have a first layer with grid center at 12m and 34m, respectively, but have the same vertical discretization as in the control experiment (C–30NS). We apologize for the confusion. Figure RC1.2 is now included in supplementary material. Readers can better understand the experiment setups.

SIT vertical grid mixing processes are based on eddy and molecular diffusivity for heat and momentum. The numerical treatments of C–LR12m (31 vertical layers) and C–LR34m (28 vertical layers) would still be computed from 0 m to seabed if the mixed layer depth was less than 30 m or 10 m.

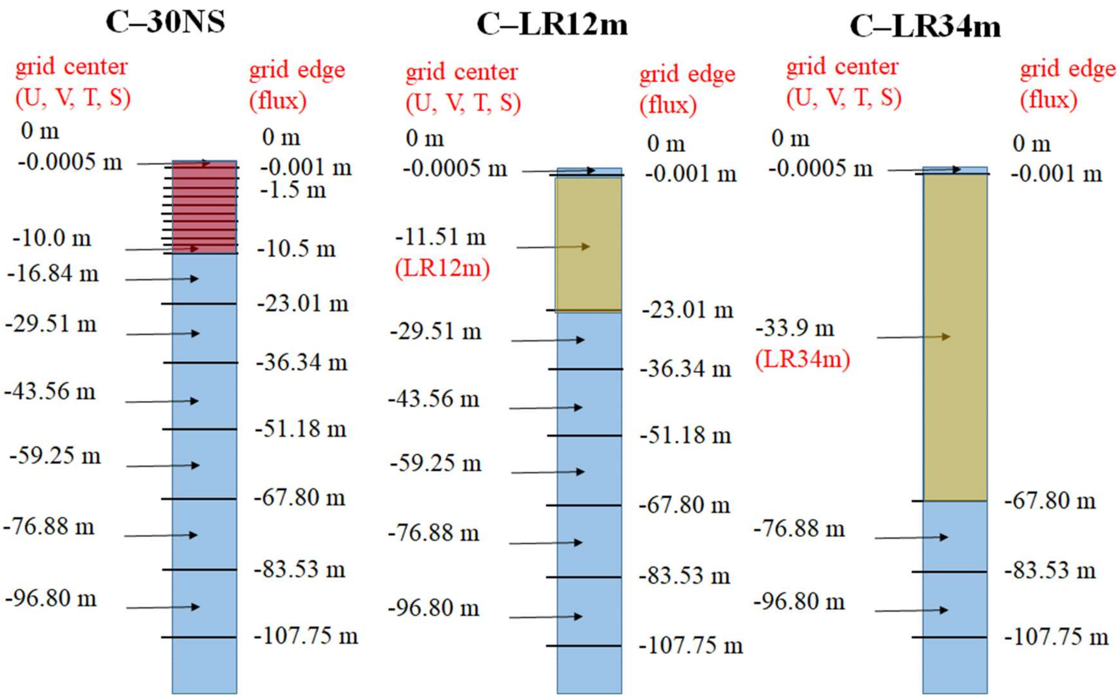

Fig. RC1.2 Diagram showing the vertical grid within 107.8 m in C–30NS, C–LR12m and C–LR34m.

RC1.P6 What do you mean "ocean bottom" at line 476? Is it seafloor?

Response:

Thank you for the question. "Ocean bottom" is misleading. It should be the bottom of the SIT as shown in Fig. RC1.3. Their ocean model bottoms are 10, 30, and 60 m, respectively, unless the seabed is shallower than the above depth. For example, if the seafloor of ocean grid is deeper than 67.8 m, this ocean grid of C–HR1mB60m would be computed from 0 m to 59.3 m depth. IF the seafloor is 52 m depth in one of C–HR1mB60m ocean grid, this grid would only be computed from 0 m to 43.6 m depth. We have change "ocean bottom" to "ocean model bottom" in the manuscript. Please see Page 9, lines 211-213 and Page 19, line 464.

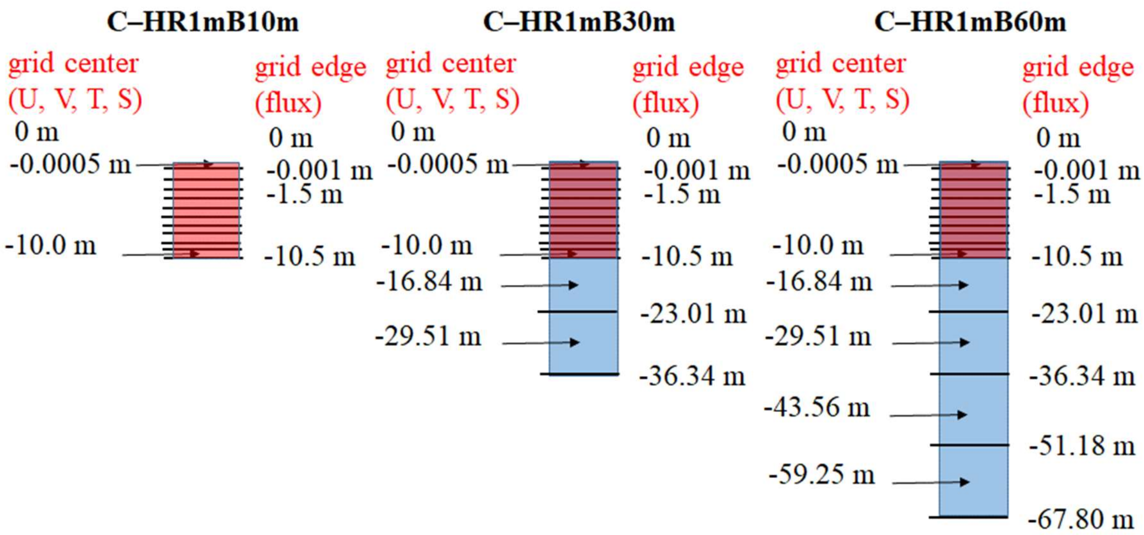

Fig. RC1.3 Diagram showing the totally vertical grids in C–HR1mB10m, C–HR1mB30m and C–HR1mB60m.

> *RC1.P7 Rewrite section 4.6. I cannot understand which fluxes you are using.*

Response:

Heat fluxes here were sensible and latent fluxes that were calculated based on simulated winds, moisture, and temperature. We have modified the text accordingly in revised manuscript. Thank you for the reminder. Please see Page 3, line 50 and Page 22, lines 539-542.

> *RC1.P8 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?*

Response:

A 30-year period is commonly used to define a current climate by the WMO and IPCC (2013) and has been a common length adopted in climate simulations to produce stable statistics. It is natural for us to adopt the same simulation strategy.

All simulations were driven by the same emission and annual cycle of SST for 30 years. The strategy is to evaluate the ability of model under the same

conditions without considering interannual variation. This approach has been widely adopted in many studies (Delworth et al., 2006; Haertel et al., 2020; Subramanian et al., 2011; Tseng et al., 2014; Wang et al., 2005). Based on the atmosphere component of the Community Earth System Model version 1.2.2 (CESM1.2.2) framework development, all experiments of CAM5–SIT were conducted under the F_2000_CAM5 component set that provides the near-equilibrium climate responses. The sea surface temperature (SST, HadSST1) used to force the model was the climatological monthly means SST averaged over 1982-2001. The monthly SST was linearly interpolated to daily SST fluctuation that forced the model. The SST in air–sea coupling region was recalculated by SIT during the simulation, while the prescribed annual cycle of SST was used in the areas outside the coupling region.

Atmospheric initial conditions and other external forcing such as CO_2, ozone, and aerosol representing the climate around year 2000 were taken from the default setting of F_2000_CAM5 component set that has been commonly used in present-day simulation using CAM5 (e.g., He et al., 2017). Initital conditions were not needed for the SST that was prescribed as lower boundary condition in the experiments. This information is now included in the revised manuscript.

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