

## ***Interactive comment on “A multi-year short-range hindcast experiment for evaluating climate model moist processes from diurnal to interannual time scales” by Hsi-Yen Ma et al.***

**Hsi-Yen Ma et al.**

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Anonymous Referee #1 This study presents an innovative approach by conducting a series of short-range hindcasts based on a climate model that can be used to expose deficiencies in model parameterizations that are responsible for biases in climate simulations from the same model. Justification of this approach includes that model biases in climate mean and variability in long-term simulations start to emerge in the first several days of hindcasts by this model, which is thought to result from model parameterized processes since the large-scale state is still very close to the observations specified in the hindcast initial conditions. This study also proposed to conduct these

C1

short-range hindcasts for multiple years to achieve stable statistics when examining model deficiencies and associated processes, although it is shown that a systematic association between model biases in short-range hindcasts and long-term simulations do not significantly change in different individual years. Three examples are further illustrated for applications of this approach to understand model biases in simulating the diurnal cycle of warm season precipitation over central U.S., the tropical MJO, and the local and remote influences by ENSO on the interannual time-scale. This approach along with the long-term hindcast datasets based on CAM5 produced from this project, can be used in many other studies for both tropical and extratropical climate phenomena, thus are valuable for the climate research community. In addition to identify model parameterization deficiencies as mentioned in this study, this approach can also provide a very useful avenue to diagnose and understand critical processes regulating various climate and weather phenomena by taking advantage of detailed model output with largely realistic representation of these phenomena in hindcasts at day-2. This can also be stressed in the manuscript. The paper is generally very clearly written although there is room for further improvement. I recommend this paper for publication after some minor revisions as listed below.

Response to reviewer:

We thank the reviewer for all the comments. Those certainly helped to improve our manuscript. A pdf supplemental file of this response is also available in the link at the end.

We have stressed the point raised by the review regarding “This experiment can also provide a very useful avenue to diagnose and understand critical processes regulating various climate and weather phenomena by taking advantage of detailed model output with a largely realistic representation of the large-scale state in hindcasts” in the revised manuscript over Lines 339-341 in the Summary Section.

Please see our point-by-point responses to the minor moments below.

C2

Minor comments: Line 12: suggest to change “3-day long hindcasts every day” to “3-day hindcasts initialized every day . . .”. Also many grammatical errors need to be corrected throughout the manuscript, which I may not list all of them in the following.

Response to reviewer: The sentence is revised as suggested and we have carefully checked the grammatical errors throughout the entire manuscript.

Line 14: suggest to delete “propagation”, since MJO amplitude is also involved.

Response to reviewer: Propagation deleted as suggested.

Line 14: also not sure about “the responses of moist processes to sea surface temperature anomalies. . .” here. Why “moist processes” since net heat fluxes and wind stresses are also shown in Fig. 7?

Response to reviewer: We changed the sentence to “the response of precipitation, surface radiative and heat fluxes, as well as zonal wind stress to sea surface temperature anomalies associated with the El Niño-Southern Oscillation - . . .” to better matched what is shown in Figure 7.

Line 21: again, in addition to “parameterized moist processes”, could other parameterizations also related, for example, boundary layer and radiation parameterizations?

Response to reviewer: Boundary layer and radiation parameterizations are certainly related based on our previous studies and experience. We did not mention these two in the abstract because we didn’t show any results directly linked to these two schemes. In the revised manuscript, we added the information “Although we only showed examples relevant to moist processes, other processes related to planetary boundary layer or radiation schemes can also be examined through this suite of experiments.” in the summary section over Lines 337-339.

Line 52: “AMIP” first appears here, but is defined in Line 99. Also, suggest to remove “of years”.

### C3

Response to reviewer: AMIP is now defined when it first appears in the introduction, and “of years” is removed as suggested.

Line 57: CAM5 is mentioned here, but CESMv1 is mentioned in abstract and other places. Better be consistent.

Response to reviewer: We now changed it to CESM1 to be consistent across the manuscript.

Line 66: suggest to delete “from diurnal to interannual time scales” in this line.

Response to reviewer: Deleted as suggested.

Line 75: variation(s)

Response to reviewer: Revised.

Line 79: present(s)

Response to reviewer: Revised.

Line 96: of the 16-year duration

Response to reviewer: Revised.

Line 120-125: would be better if how these regimes are defined can be briefly explained.

Response to reviewer: The cloud regimes are now defined and briefly explained following Zhang and Klein (2010) in the revised manuscript over Lines 160-169.

Line 135: cloud regime(s)

Response to reviewer: Revised.

Line 147: parameterization deficiencies

Response to reviewer: Revised.

### C4

Line 150: suggest change “scheme developer” to “model developer”

Response to reviewer: Revised as suggested.

Line 153: the intraseasonal variability

Response to reviewer: Revised.

Line 166: present(s)

Response to reviewer: Revised.

Line 170-171: “the intensity of OLR decreases” is a little confusing since active convection corresponds to smaller OLR. Not sure if this means convection is enhanced or weakened.

Response to reviewer: The sentence is revised to “the intensity of convection decreases (with OLR anomalies increasing) after the core of MJO crosses over the Maritime Continent” to avoid the confusion.

Figure captions 3,6: CAPT are used but not defined.

Response to reviewer: We now removed CAPT from the figure captions.

The presentation of Figures 4,5 can be improved, particularly Fig. 5. It may be not needed to use both precipitation and OLR since both of them represent MJO convection. At their present forms, these figures look very busy and a little difficult to see some detailed features particularly in wind vectors.

Response to reviewer: We have removed OLR contours in Figure 5 to make the figure less busy in the revised manuscript.

Lines 179-180: Seems to me, the wet biases are largely evident over regions around the Maritime Continent, which may also relate to model biases in representing the regional diurnal cycle.

Response to reviewer: We agree that the wet biases may also relate to the biases in  
C5

the diurnal cycle of the Maritime Continent. We now revised the sentence to “Further, there is a persistent dry bias over Borneo and part of Sumatra and wet bias around the Maritime Continent for all the phases indicating a possible local effect of diurnal cycle of convection.” in the revised manuscript over Lines 228-229.

Line 220: the circulation anomalies are discussed here, but temperature and moisture fields are also used to constrain model in the initial conditions. “the response SST anomalies is” needs to be fixed

Response to reviewer: We revised the sentence to “The large-scale state is well constrained in the hindcasts and the response of those fields to SST anomalies in Figure 7 is much superior”.

Line 231: “larger” root mean square errors?

Response to reviewer: “larger” is added in the revised manuscript.

Line 254: Possibly to briefly mention these “cloud measures”.

Response to reviewer: We now added the following sentences “ETCA measures the error in total cloud amount, and ECTP-t measures the errors in the frequency of optically intermediate and thick clouds at high, middle, and low-levels of the atmosphere. ESW and ELW measures the errors in the impacts on top-of-atmosphere shortwave and longwave radiation in the same cloud-top pressure and optical depth categories used for ECTP- t, respectively” in the revised manuscript.

Line 284-285: “Indeed, GCMs usually perform more poorly for . . .”. Not quite follow the purpose of this statement.

Response to reviewer: We have removed this sentence in the revised manuscript.

Line 297: suggest change to “to understand the processes that control the diurnal and sub-diurnal variations of . . .”

Response to reviewer: This sentence is revised as suggested.

Line 301: CESM1 is mentioned here, and CAM5 in the model introduction part.

Response to reviewer: We now use CESM1 across the manuscript.

Please also note the supplement to this comment:

<https://gmd.copernicus.org/preprints/gmd-2020-39/gmd-2020-39-AC1-supplement.pdf>

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Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-39>, 2020.