

GMD review

Title: A pathway-based evaluation of MJO simulation in the Community Atmosphere Model version 6 (CAM6): linking parameter sensitivities to moisture and cloud–radiation processes

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Reviewer number: N/A

General Comments:

The study uses a multi-step approach that identifies physics parameters that are most influential to MJO simulation skill, proposes a parameter set that produces a much-improved MJO compared to the default parameter set, links parameter adjustments to physical processes, and demonstrates that the tuned parameter set does not also degrade the time mean of precipitation. Results from a perturbed parameter ensemble (PPE), similar to those of previous studies, are fed to a generalized linear model (GLM) that identifies *dmpdz*, *tau*, and *ai* as the most impactful parameters for MJO simulation skill. A final perturbed experiment “DTE” in which *dmpdz* is maximized and *ai* and *tau* are minimized (as suggested by the GLM results) exhibits a considerably improved MJO relative to the control run “CTL”. The authors use the moist static energy (MSE) budget to link the parameter adjustments to physical mechanisms that favor MJO improvement: (1) *dmpdz* improves the climatological moisture distribution (particularly the meridional gradient) on which MJO circulation anomalies act to promote MJO eastward propagation, and (2) *ai* improves cloud-radiation feedbacks (and thus MJO maintenance) by increasing the amount of ice cloud produced per unit precipitation. The specific influence of parameter *tau* remains unclear. The parameter changes in DTA do not degrade the time mean precipitation relative to CTL, but the impact on other state fields in DTA was not examined.

Reconsider after major revisions. The text is logically organized, the figures are easily understood, and the scientific interpretation of the results is generally sound. Although PPE, GLA, and the MSE budget analysis separately are not novel approaches (these approaches have been used in previous studies to identify dominant MJO parameters and key MJO mechanisms), linking specific parameter modifications to specific MJO-relevant mechanisms through the MSE budget is innovative and informative. Figs. 10 and 11 are particularly interesting as they connect the influence of parameter tuning on the impact of cloud-radiative feedbacks isolated to light-rain conditions. Below are a few specific comments and several technical comments that I encourage the authors to respond to.

Specific Comments:

1. Sec. 2.3.1: Some more background information on the GLM equation is needed.
 - a. While the application of GLM to MJO performance is conceptually straightforward, it would likely benefit many readers to elaborate on this approach and its results. The cited Guo et al. (2014) paper presents a helpful summary in their section 3.1.1, and I'm wondering if the authors can produce a figure like Guo's Fig. 1.
 - b. Please explain why two-way interaction terms are not considered in the GLM, though they are used in Eq. 1 of Guo et al. (2014). It may be of particular interest to understand interactions of *tau* with *dmpdz* and *ai*, given that DTN uses a strongly reduced *tau* value yet a maximized *dmpdz* to obtain the best MJO.
 - c. Parameters N and σ in Eq. (1) should be defined/quantified.
2. L524-532:
 - a. The longitudinal bounds selected to produce Figs. 9-11 are confusing. The authors initially describe the choice of longitudes 90°E-180°E as designed to encompass the eastward-propagation pathway of the MJO (L490-491). But based on Figs. 4 and 10, this seems to include both the MJO deep convective center (its eastern half) and the suppressed-convective eastern region toward which the MJO deep convective center is moving. Regarding the choice of using 90°E-180°E, this would seem to include both the near-active MJO phase as well as the fully suppressed MJO phase. Is it known whether the results of Fig. 11 are influenced mainly by the more active MJO longitudes (say, 90°-135°E) or the suppressed longitudes (say, 135°E-180°E).
 - b. For Figs. 10 and 11, the authors conditionally sample the data to retain only days when the precipitation anomaly is +1-5 mm/d ("light rain" anomalies), so intense-rain grid cells are masked out. Fig. 11 indicates that DTA's improved MJO is associated with a higher GEF, HCC, precipitable water, and upward motion over the 90°E-180°E longitude band, compared to CTL. However, several previous studies have shown that MJO skill is higher for models that strongly *suppress* deep convection ahead of the MJO convective center, which allows atmospheric preconditioning to occur and instability to build. Is it the case from Figs. 9-11 that, although the large-scale convective suppression to the east of the MJO convective center is still happening, the light-rain events that do occur in that region are more vigorous (Fig. 11d) and result in a larger cloud-radiative feedback (Fig. 11a,b) that preconditions the atmosphere? Explaining this apparent distinction between the large-scale suppression of deep convection over the west Pacific, and the concurrent light-rain event characteristics in driving

enhanced cloud-radiative feedbacks (as seen in Fig. 11), is important and should be clarified in the manuscript.

3. L582-583: It is stated that the influence of parameter *tau* remains unresolved, but couldn't this influence be highlighted by rerunning the DTA experiment by only changing *dmpdz* and *ai* (i.e., experiment "DA") and keeping *tau* at its default value? The resulting DTA–DA difference might elucidate the impact of the *tau* change on the simulation. This idea is mentioned in L588-589 and 593-595, and later in Sec. 6. A large number of simulations were conducted for this project, so it would seem reasonable to run one additional simulation to more cleanly identify the effects of *tau*. It is of particular interest to understand if minimizing *tau* artificially reduces biases (i.e., error compensation) that would otherwise degrade the mean state. That is, if *tau* is reverted to its default value in DTA, would the mean state worsen?

Technical Corrections:

1. L50: Klotzbach (2014) is another good paper to cite here, related to MJO impacts on tropical cyclones worldwide:
Klotzbach, P. J., 2014: The Madden–Julian Oscillation's Impacts on Worldwide Tropical Cyclone Activity. *J. Climate*, **27**, 2317–2330, <https://doi.org/10.1175/JCLI-D-13-00483.1>.
2. L60-61: Sensitivity of the MJO to the deep convection parameterization has been known for a long time. Consider adding this paper, which notes that MJO skill differs based on deep convective scheme in a model intercomparison:
Slingo, J.M., Sperber, K.R., Boyle, J.S. *et al.* Intraseasonal oscillations in 15 atmospheric general circulation models: results from an AMIP diagnostic subproject. *Climate Dynamics* **12**, 325–357 (1996). <https://doi.org/10.1007/BF00231106>
3. L202-203: It's recommended to cite this statement using:
Zhang, C., and M. Dong, 2004: Seasonality in the Madden–Julian Oscillation. *J. Climate*, **17**, 3169–3180, [https://doi.org/10.1175/1520-0442\(2004\)017<3169:SITMO>2.0.CO;2](https://doi.org/10.1175/1520-0442(2004)017<3169:SITMO>2.0.CO;2).
4. L212: "*j*-th parameters" → "*j*-th parameter"
5. L230-231: Is *Qr* derived from a vertical integral of multi-level longwave (QRL) and shortwave (QRS) radiative heating rates, or the difference in single-level model-top (FLNT, FSNT) and surface (FLNS, FSNS) heating rates?
6. L278 & 352: regression → regressing
7. Fig. 4: Are negative column MSE contours omitted from the figure? If so, this should be stated in the caption.

8. L369-371: I have no doubt the authors carefully considered their decision to project *model*-derived column MSE budget terms onto *observation*-derived column MSE and MSE tendency patterns to produce Fig. 5. However, I am wondering about the validity of this approach. It would seem that this approach could potentially result in comparing a MSE budget term that is physically inconsistent with the column MSE and MSE tendency pattern onto which it is projected. This would seem to be particularly true in the case of the CTL simulation, which shows column MSE and MSE tendency patterns that differ considerably from OBS in Fig. 4. It would benefit the manuscript to further justify their approach.
9. Fig. 6 (and L239): Connecting the decomposed vadv budget terms between Eq. 5 and Fig. 6 could be improved. Consider either adding number labels to each term on the rhs of Eq. 5, or explicitly labeling each numbered tickmark in Fig. 6 with the budget term.
10. L401-402: It is recommended to specify that this term is the “intaseasonal anomaly of the column-integrated intraseasonal meridional advection of the low-frequency background MSE”.
11. L407: facillates → facilitates
12. L411: Add “not shown”?
13. L447-448: Is this statement in reference to a particular longitudinal range in Fig. 8b?
It seems to be most relevant to the Indian Ocean basin.
14. L459: HCC has not yet been defined.
15. L581: 12d → 11d
16. L613-614: The “weak negative anomalies” in time mean precipitation aren’t able to be seen because the multi-panel color bar in Fig. 12 is positive-only.
17. L673-675: It would be very helpful to add figure references, where appropriate, to the sentences in the Conclusions section. The sentence L673-675 is one good example, where a reference to Fig. 8a could be added.
18. L687: “higher” → “more”