

Response to Reviewer's Comments

Reviewer #1

This manuscript evaluates the QBO–MJO connection in 12 QBOi phase 2 models with a nudged tropical stratosphere. The dataset is valuable and the presentation is clear, but the contribution beyond existing work is limited. The analysis follows the standard set of diagnostics (QBOE–QBOW composites, MJO-filtered variance, Wheeler–Kiladis spectra, RMM correlations, lead/lag regressions) used in Lim and Son (2020), Kim et al. (2020), and Martin et al. (2023). These are the expected diagnostics for an MJO–QBO evaluation, applied appropriately, though no new analysis is introduced. The main findings, that the simulated MJO is too weak over the Maritime Continent and that no QBO–MJO connection appears even with a realistic QBO, are the same as previous studies, as the authors mentioned. The larger model set confirms the null result but does not change the previous findings, and the proposed explanation (weak MJO convection) is inherited rather than tested. EC-Earth3, with the smallest MJO biases, still shows no connection. Overall, I do not have major concerns, as the analysis is the standard, expected one for this type of evaluation. My main suggestion is that the authors state explicitly in the introduction what is new relative to previous studies.

Response: We would like to thank Reviewer #1 for the valuable time reviewing this manuscript and providing constructive feedback. We have revised the manuscript accordingly. Please see the comment-by-comment responses below for details. In particular, we clarified the novelty of this study in the Introduction by highlighting the use of 12 QBOi phase 2 models with stratospheric QBO nudging.

(Minor)

L19 "convective"

Response: Done.

L141 "stratospheric"

Response: Done.

L234 remove "and"

Response: Done.

Fig. 2: The legend in panels (b) and (c) is very hard to read.

Response: The legend is enlarged and appended to the right of the panel (c).

Inconsistent model names: BCC-CSM2-MR vs -HR; MIROC6.1 vs 6.3

Response: The model names are revised to be consistent with "BCC-CSM2-MR" and "MIROC6.1".

Fig. 6 caption should refer to Fig. 5, not Fig. 4.

Response: Done.

Fig. 2 caption: "global mean" to "zonal mean"

Response: Done.

Reviewer #2

Review of “Biased MJO causing a lack of QBO–MJO connection in multi-model simulations with a nudged QBO” by Huang et al.

Recommendation: Accept after minor revisions

Summary:

This study describes an effort to examine QBO-MJO connections in a suite of models (with multiple ensemble members) where the zonal mean zonal wind is nudged to constrain the stratosphere toward realistic QBO behavior. In general, realistic variations in upper troposphere and lower stratospheric stability are qualitatively reproduced, if not quantitatively. Biases in MJO simulations result in weak MJO-QBO connections, although the strength of radiative feedbacks in models that is typically thought to be important for MJO maintenance show no systematic bias.

This is an interesting and worthwhile study showing a general lack of relationship between MJO activity and the QBO across climate models. I feel that it will make a good contribution to the journal after some relatively minor revisions. I have a few comments related to the interpretation of results, and some suggestions for further analysis to improve the story, for the authors to consider in a revision. Minor line-by-line comments follow.

Response: We would like to thank Reviewer #2 for the valuable time reviewing this manuscript and providing constructive feedback. We have revised the manuscript accordingly. Please see the comment-by-comment responses below for details.

Comments:

line 35. “but” -> “the”

Response: Done.

Line 53. Based on the results of the current paper, is a biased QBO the only reason for the poor MJO-QBO relationship in the prior study? Or is it one possible contributor based on the new information found here?

Response: Thanks for this question. We acknowledge that the biased QBO is one possible contributor to the lack of QBO-MJO connection in CMIP6 models. Therefore, we revised this sentence as “One potential reason for the lack of QBO-MJO connection in these studies is the biased QBO simulated by the models which is usually too weak in the UTLS levels.”

lines 127-129. Based on this statement, can it be ruled out that poor representation of the QBO might also contribute to the inability of the models to capture the MJO-QBO connection?

Response: Thank you for this comment. We added some discussions about these biases and how they may contribute to the models’ inability to capture the QBO-MJO connection. Please see the response to the next comment for details.

line 139. I think the phrase “slightly underestimated” may be an understatement here. It looks like the temperature anomalies may only be half of the observed temperature anomalies on average. This adds a further caveat to the conclusion that it is only the MJO that produces the weak link in the MJO-QBO connection. I think line 145 below more honestly states this discrepancy.

Response: Thank you for this comment. Combined with the previous comment, we added some discussions about the underestimated QBO wind and temperature responses around the UTLS and how they may contribute to the uncaptured QBO-MJO connection around Lines 147-150: “The underestimated QBO-induced temperature responses may partially hinder the model capabilities to capture the QBO-MJO connection. However, such responses are qualitatively represented across the models. The distinct model amplitudes in the QBO-induced temperature anomalies also provide an opportunity to examine whether any potential MJO responses to QBO is correlated with it.”

Figure 4. As OLR is an indirect proxy for convection that depends on microphysical quantities, their detrainment, and other factors, it might also be good to reproduce Figure 4 with precipitation. Precipitation is used in Figure 5, but it shows signal to noise ratio rather than overall power.

Response: Thank you for this suggestion. We recreated Figure 4 using precipitation from the observations and QBOi phase 2 models as below (Fig.r1). The conclusions do not change, i.e., all the models underestimate the MJO variances either by OLR or precipitation rate. However, the MJO precipitation variance is even more underestimated by the models compared to that for OLR. We added a sentence in the manuscript to briefly summarize this around lines 179-181: “Daily precipitation is also used to examine the MJO convective variance (not shown here) where the universal underestimation by the models is also found and the underestimation is even more compared with the OLR variance shown by Fig.4.”

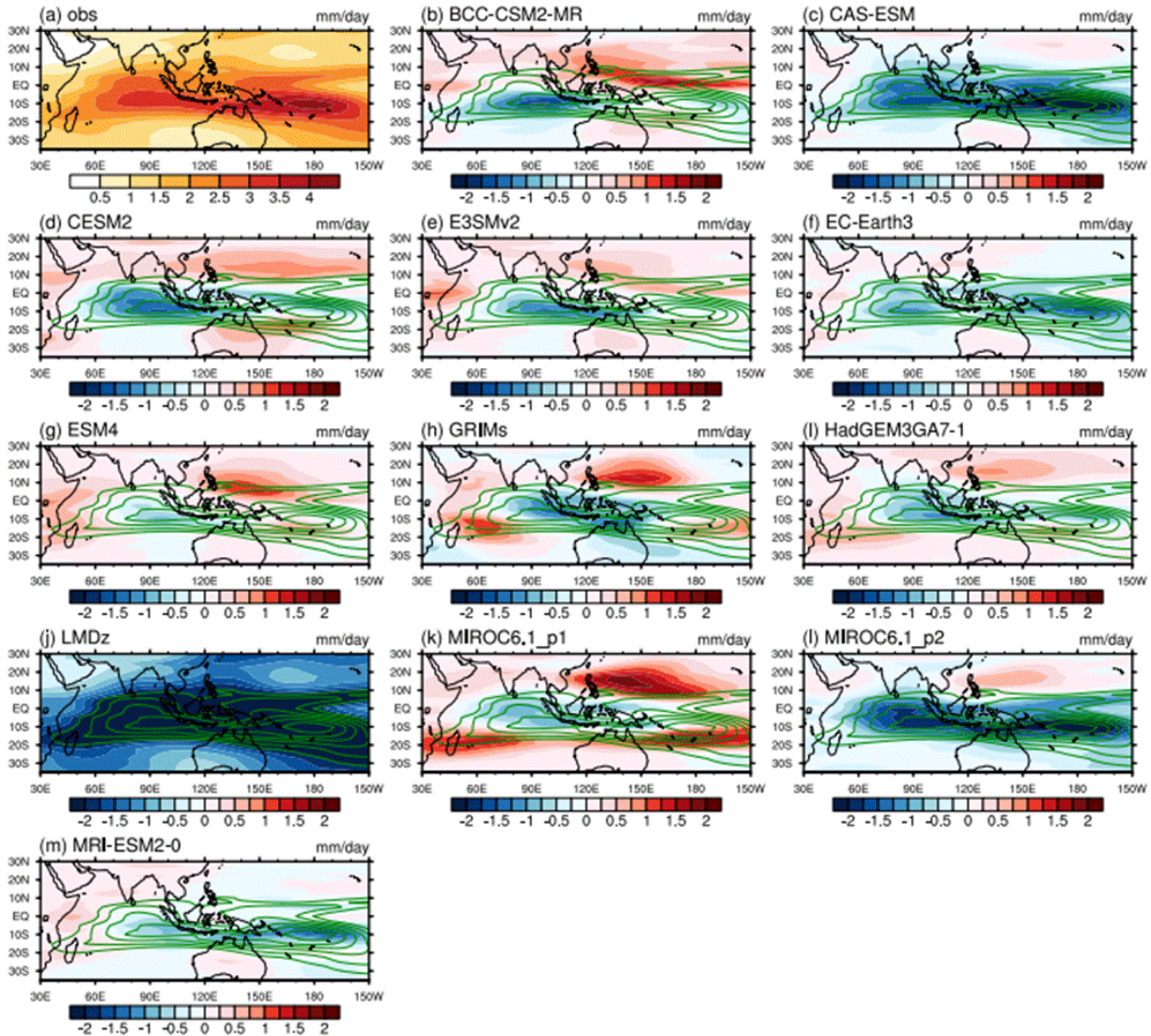


Figure r1. Maps for the MJO-filtered (20-100-day, and eastward-propagating with zonal wavenumber of 1-10) precipitation standard deviation in the boreal winter seasons for (a) observations, and (b)-(m) model biases from the observations (shadings). The contour lines in (b)-(m) denote the observational values starting from 2 mm/day with an interval of 0.4 mm/day.

line 186. The authors might check on whether Liu and Wang were referencing the forced Kelvin-wave like response to MJO heating, or the freely-propagating convectively coupled Kelvin waves that lie along the dispersion curves in Figure 5. The authors might also comment on biases in Kelvin wave propagation speed?

Response: Thank you for this comment. We noticed that Liu and Wang were referencing the forced Kelvin-wave like response to MJO heating, rather than the free-propagating convectively coupled Kelvin waves we are examining here. Therefore, we changed the cited reference to Guo et al., (2015) where they identified a systematic relationship between the representations of convectively coupled equatorial wave activity including that of the free-propagating convectively

coupled Kelvin waves and the MJO in climate model simulations. The discussions of the Kelvin wave phase speed are added around lines 191-193:

“The simulated Kelvin wave phase speed, as suggested by the slope of Kelvin wave energy peaks, also varies across the models which is overestimated by most of the models including CAS-ESM, EC-Earth3, LMDz, and MIROC6.1_p2, while BCC-CSM2-MR, ESM4, and MRI-ESM2-0 underestimate the simulated Kelvin wave phase speed.”

line 211. But the observations are only one realization too, and the GRIMs shows large regions of significance. The authors maybe are a bit too dismissive of this signal in that model?

Response: Thank you for this comment. We’ve revised this sentence to “However, note that the GRIMs simulation consists of only one member, which is fewer than the majority of the participating models (Table. 1).”

Figure 9 and line 239. This plot and line 239 do make me wonder if we were able to do another ensemble member with the observed Earth (impossible!), whether the real world would also show no relationship between the MJO and QBO for some realizations. This could be worth some discussion.

Response: Thank you for this suggestion, and this is an interesting point. However, we respectfully decline to add more discussions about this as it is more about whether the observed QBO-MJO connection may vanish with multiple observational realizations, which is out of the scope for this work.

line 277. Is this statement entirely true? EC-Earth seems to have little bias (e.g. Figure 4).

Response: Thanks for this comment. In the revised version, we pointed out that EC-Earth3 shows the least biases around this line.

lines 302-303. I’m a little confused as to how Figure 13 is constructed. It is derived independently of Figure 11 and Figure 12? If so, why are Figure 11 and 12 shown?

Response: Sorry for the confusion. Figure 13 is derived by a linear regression fitting between the MJO-related OLR anomalies and precipitative heating, which are obtained by lead/lag regressing their anomalies against the RMM1 index from day -20 to day 20. Figure 11 and Figure 12 show their standard deviation, and Figure 13 shows the linear regression coefficients using the grid point data gathered over the Indo-Pacific region (15S-5N, 60E-180). We revised this sentence into “For all the grid points over the selected Indo-Pacific region (15S-5N, 60E-180), a linear regression is conducted between the MJO-related OLR anomalies and precipitation-induced latent heat release after both fields are lead–lag regressed against the RMM1 index from day -20 to day +20 in each model (Fig.13).”

lines 311-312. There is quite a bit of literature suggesting that strong radiative feedbacks produce a strong MJO. Is the point being made here independent of that? Is the strength of radiative feedbacks uncorrelated with MJO strength across models, which it appears to be by eye?

Response: Thank you for this question. We are not challenging the importance of radiative feedback for the simulation of a strong MJO. Instead, we are simply suggesting that apart from

the sufficient radiative feedback, a frequent enough occurrence of very negative OLR anomalies along with very positive precipitation anomalies, which are quantified by the fractions of these dots shown at the bottom right corner of each subplots in Fig.13, is likely to be also important to capture the QBO-MJO connection in the models.