

Weather Clim. Dynam. Discuss., referee comment RC2
<https://doi.org/10.5194/wcd-2021-9-RC2>, 2021
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Comment on wcd-2021-9

Anonymous Referee #2

Referee comment on "Drivers of uncertainty in future projections of Madden–Julian Oscillation teleconnections" by Andrea M. Jenney et al., Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2021-9-RC2>, 2021

This study examines the uncertainty in MJO teleconnection amplitude changes in North Pacific and North America in January based on results from the linear baroclinic model (LBM). The model is forced with the CMIP6 mean state and an idealized MJO heating with multiple sensitivity runs to test the relative contributions of different MJO and mean state characteristics to the uncertainty in MJO teleconnection amplitude changes. The results indicate that future changes in the mean state wind largely lead to uncertainty in the teleconnection amplitude over both the North Pacific and North America regions. Changes in the MJO propagation speed also contributes to the teleconnection uncertainty. Although some comprehensive simulations are run, the analysis approach in this study may need justification/improvement. Some results are not convincing as they are based only on the LBM and lack other scientific support. I, therefore, recommend this manuscript go through a major revision based on comments below.

Major comments:

Some analysis method needs justification/modification. For example:

- L91-93. In terms of the mechanisms related to teleconnection amplitude, the LBM could be TOO simple. Teleconnection amplitude is largely influenced by nonlinearity (Lin and Brunet 2018). Some relationships that may not found in full GCMs such as weaker MJO circulation leads to weaker teleconnection amplitude (Zhou et al. 2020) can be found in the LBM because the LBM is linear. This will make some results unrealistic/less convincing. Therefore, the authors may need to compare their results from the LBM with CMIP6 to discuss which relationships may be due to the linear setting, and which are not. Another way to address this issue is that the LBM package also has a nonlinear version (used in Henderson et al. 2018) which may be better for the investigation of the teleconnection amplitude changes.

- The authors investigated changes in the MJO propagation speed, zonal extent, intensity, and mean state dry static stability and winds. It is not clear why only these changes are examined. Are the authors choosing them randomly or based on some studies that these are the most robust and significant changes in the MJO and basic state?
- The authors force the LBM using only the January condition. The authors may need to provide a strong reason why they are not using the entire winter season mean.
- The authors only use one ensemble member from the CMIP6. However, there can be very large uncertainty coming from different ensemble members. Also, these ensembles may not be the ensembles that produce the most realistic MJO teleconnections in their historical runs. Is there a reason why the authors are not using the ensemble mean?
- When deciding the lower and upper bounds of the MJO changes, the authors used the estimate from CMIP5 models from previous studies. It makes the analysis inconsistent in this study. It would be better to estimate the MJO changes based on the CMIP6 models. CMIP6 produces a much more realistic MJO propagation (Ahn et al. 2020), the authors would need to discuss if there are any sensitivity of the results. Also, the authors did not show evidence why they choose to extend the MJO to 20° eastward.
- The amplitude metric in this study may not be clean enough to represent the amplitude only. The metric can also be influenced by changes in the location of MJO teleconnections. Is there a reason why the authors not just simply calculate the spatial variance of MJO teleconnections as in Wang et al. (2020)?
- The authors investigated the 850hPa meridional wind because they indicate that low-level circulation will be more important to regulate near-surface weather. However, the main purpose of this study is not to examine the MJO impacts on near-surface weather. Then it would be better to use either middle or upper tropospheric wind given that MJO teleconnections have larger amplitude over the middle to the upper troposphere and the results could be used to compare with previous studies.
- L232-234. Are these supported by CMIP6 projections? Here, it would be nice if the authors can compare their results with the CMIP6 projections.
- Teleconnection changes are analyzed at the lower troposphere, whereas mechanisms (RWS and Ks) are analyzed at the upper troposphere. This can lead to inconsistency of the results. The authors may want to confirm their results by looking at teleconnection changes in the upper troposphere.
- The authors analyze the amplitude changes by separating the North Pacific and North America regions. Please provide a reason for looking into different regions.
- Eq (3): Here, the stationary wavenumber is for the barotropic model and mean wind with no zonal variation and no meridional wind. That being said, this equation is not fully consistent with the basic state they used for the calculation. If the authors are using the full basic state, they would need to use the more complete format of stationary wavenumber (Li et al. 2015). The authors may need to justify why they neglect the zonal asymmetry and meridional wind in Eq (3).
- Fig. 3. The larger spread of MJO teleconnection amplitude could be due to the larger spread of mean state wind projection by CMIP6s. The authors may need to show 1) whether the wind projection is more uncertain in CMIP6 models than DSE. If this is the case, it is very easy to interpret Fig. 3 that the larger uncertainty in teleconnection amplitude is due to larger uncertainty in future mean wind projection, which is important to look at. 2) However if the projection uncertainty is similar between wind and DSE in CMIP6s, the authors may need to discuss why changes in the wind would lead to larger uncertainty in teleconnection amplitude than changes in DSE.

Some figures that need to be improved or questions regarding the figures:

- It would be quite useful if the authors could provide the spatial pattern of MJO corresponding to their perturbed cases. Only the upper bound case would be sufficient to visualize the MJO pattern to facilitate understanding.
- Fig. 2a. Is this location arbitrary? Why are the authors not showing the multi-model mean of the CMIP6 which may be more useful?
- Please be very clear here that Fig. 2b is based on LBM response not based on real multi-model mean CMIP6.
- Fig. 2b. very hard to see the strengthening (highlight of the contour). The authors did not provide the longitude of this eastern boundary to quantify the eastward extension. Also, it may be useful if the authors could provide the difference map with the control runs to help interpret the changes.
- Fig. 3. 1) No changes in DSE is shown, then how the authors know there is an increase in dry static stability? Is it based on previous studies? Some CMIP6 may project different changes in DSE. The authors may need to show how each CMIP6 model project DSE change and how that relates to teleconnection change. That being said, Fig.3 can be separated into 8 figures. In one figure, one axis represents DSE change in the CMIP6 models, one represents teleconnection amplitude change (either absolute change or fractional area). The same to the wind. By doing this, the authors could see how DSE or wind influences teleconnection amplitude. 2) Also, the authors have never shown how DSE and wind are changing in CMIP6. 3) Very hard to separate the models with these markers here. Using numbers may be better to separate.
- Fig. 5b and c. Still not clear how these maps are calculated? What region is used for the average of the $15^{\circ} \times 15^{\circ}$ box? Also, the authors may want to only focus on the regions that have a high correlation coefficient. That means the authors can draw plots only with significant correlation in Fig. 5.
- Fig. 6. Very hard to imagine why the spread can be this large over the North America region for Fig. 6e. The MJO is changing with the same propagation speed, and the mean state is not changing between the two runs. It may be helpful if the authors could provide the spatial maps of the two runs (historical MJO value and future MJO value) for these 10 models.

Some mechanisms that need more discussion:

- L300-305. Better to show if this can be found in any CMIP6 model. Also, it is not clear why an increase in the number of Rossby waves can lead to stronger MJO teleconnections?
- L329-331. What is the underlying mechanism? The more eastward extent leading to larger teleconnection amplitude has been found in Adames and Wallace (2014).

Minor comments:

- The title is too misleading. Firstly, this paper discusses mainly the amplitude or strength of the MJO teleconnections which should be pointed out in the title. Secondly, the paper runs the simple model using the CMIP6 mean state but the results are not based on the CMIP6 projections, and the MJO heating is even idealized. So the authors may need to change the title to the numerical study of the mechanisms driving MJO teleconnection amplitude changes, etc. Also, this paper is more focused on the important factors contributing to the teleconnection amplitude changes rather than the mechanisms. Most mechanisms discussed in this study are based on findings in previous studies.
- L5-6. For example? The more eastward extension seems to be a robust change in most models (Zhou et al. 2020).
- This study is focused on which season? How MJO teleconnections are characterized? These would need to be mentioned in the abstract.
- L9-10. This is found in Wolding et al. (2016), which would need to be mentioned here.
- L11-12. Also found in Wang et al. (2020), which would need to be mentioned here.
- L12-13. Briefly explain more here about the mechanism.
- L14-16. May be better to put it right after L11.
- L16-17. This may not be a good argument. The reduction is implied by the LBM experiment forced by the CMIP6 mean state rather than in the majority of CMIP6 models.
- L23-25. It is not clear how MJO teleconnections would be impacted by these factors. Since these are largely discussed in this study, a much more detailed introduction is needed to emphasize the motivation.
- L26. How do the authors come to this conclusion? No references are discussed and cited here.
- L26-27. Why only these two factors are mentioned among very many factors that influencing the MJO simulations? The authors may want to point out that MJO simulation is relatively poor in some CMIP5 models as that is the main focus in Ahn et al. (2017).
- L28-29. Zhou et al. (2020) already provided some very good analysis of the teleconnection changes. The authors may want to discuss a bit about their results and what remains unknown. This sentence is not their main conclusion. Also, what is the relationship between this and the previous sentence?
- L30. "MJO teleconnection strength" over which region?
- L31. L35-36. Need references.
- L36-37. Discuss how "MJO precipitation intensity or cloud optical properties" would change
- L39-41. Do the authors mean the large spread in circulation projection is because of the spread in the projection of precipitation?
- L41-43. what are the relationships between these two studies? One is talking about static stability, one is circulation change? Better to make consistency here although they may be coupled.
- L57-58. What changes in mean wind and what changes in teleconnections the authors believe would be important to look at? Is there any evidence from previous work which could serve as motivation?
- L64-65. Is this found in CMIP6 projections? any reference? Or just the authors' hypothesis?
- L70-71. References or hypothesis? There are many other changes in MJO, such as frequency of events, the authors may want to point out why they are focused only on

propagation speed and extent. Is it because these are the most robust changes? If so, please provide references.

- L73. Please clarify what the authors mean by "MJO teleconnection change". It has been quite confusing throughout the text. How the authors quantify the uncertainty? Please briefly specify here.
- L79-81. It is better to have the main motivation to fill the gap of previous studies which authors should discuss more rather than from the prediction perspective which is too operational oriented.
- L81-83. This could sound like a big problem here. The authors may need to prove that this would not be an issue such as the authors are using an idealized MJO.
- L84. Please clearly describe here what analysis will use CMIP6 model output and what will use LBM. This is quite confusing in the introduction.
- L85-86. Why the authors hypothesize the uncertainty in projections is large? I did not see this point in the previous paragraphs, only a bit was discussed in Zhou et al. (2020). But this cannot be considered as the authors' hypothesis. The authors may need to provide more evidence to support this hypothesis.
- L86. "various mechanisms" such as...? Please also briefly describe how to quantify the contribution?
- Fig. 1. Any references that used similar idealized MJO heating?
- Please make the discussion consistent. Only use "amplitude" or only use "strength". Now is a bit confusing.
- L217. Are the authors changing both the zonal and meridional mean winds in LBM?
- L222. How fractional area is calculated?
- L244. Can only say "changes in dry static stability". The authors did not show how DSE is changing in CMIP6s.
- The finding that RWS is not the key role in changing the teleconnection amplitude is also implied in Wang et al. (2020). They found that changes in the amplitude of RWS does not necessarily lead to the same changes in the amplitude of teleconnections.
- L290. What sizes of the boxes have the authors tried?
- I am not sure why the experiments of the MJO intensity are necessary? It is very obvious that with a linear model the teleconnections would change as to how heating intensity changes.
- L315-316. Is this arbitrary? How the authors chose the models?
- L326-329. Impacts of propagation speed on the teleconnection amplitude have been found in many previous studies already (e.g., Yadav and Straus 2017; Goss and Feldstein 2018; Zheng and Chang 2019; Wang et al. 2020).
- L333. "uncertainty in changes to the MJO". I do not understand why there is uncertainty in the MJO? Isn't the MJO difference the same between different models? The MJO used in the forcing is just an idealized MJO which is certain. "uncertainty in the mean state". Why there is uncertainty in the mean state? Isn't the mean state just used the historical value? Is here the uncertainty mainly comes from the model difference of their mean state?
- L350-351. Be careful to make this conclusion as this is only a finding in the LBM.

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