

Weather Clim. Dynam. Discuss., referee comment RC3
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Comment on wcd-2021-9

Anonymous Referee #3

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-RC3>, 2021

Review of " Mechanisms driving MJO teleconnection changes with warming in CMIP6"

By Jenney et al

The paper presents a diagnosis of changes in the strength of the Madden-Julian Oscillation (MJO) teleconnections in future climate simulations. The paper compares changes associated with an altered basic state (including static stability and the basic state winds) with those associated with a few selected characteristics of the MJO. The results also quantify the degree to which these factors differ in the various CMIP 6 models. The results are mostly convincing and merit publication. However the authors don't do a very good job of discussing the limitations and caveats of their study, but this should be relatively simple to fix.

General comments:

- The authors mainly examine two aspects of how changes in the MJO itself might lead to an altered extratropical response, namely the propagation speed and eastward extent. Given the relatively poor simulation of the MJO in many previous models, it is not clear to me whether these are the only two possible changes that may occur. (I tend to have

little faith in the future projections of the MJO in CMIP5 - maybe CMIP6 will be better – and most of the estimates of future behavior in the MJO seem to come from papers that analyze on the order of 10 CMIP5 models)

a. The authors use a very idealized heating perturbation in their equation 1, which clearly will not capture inter-model variability in how well the MJO is represented in the present climate, let alone the future climate. This caveat needs to be mentioned in the discussion.

b. The authors rightly note that their linear model is poorly suited for understanding whether a change in the magnitude of the MJO circulation might lead to a change in teleconnection, but it is conceivable that a stronger MJO circulation might lead to disproportionately stronger (or weaker) teleconnections if nonlinearities were allowed. While this is mentioned near line 320, this caveat needs to be mentioned more clearly both in the discussion and methods as well.

c. The authors are relying on some previously published work on changes in the MJO, but other possible changes might occur in the MJO besides changes in amplitude, propagation speed, and eastward extent. If for example, the meridional extent of the MJO were to become broader as tropical GMS changes its structure under climate change, then the subtropical RWS could change its entire character. Whether this might occur is hard to know given the poor state of MJO's in CMIP models (at least CMIP5), but tropical GMS is indeed projected to change its structure and I tend to believe projections of, say, large scale GMS than of the MJO itself. The key point is that our current understanding of how the MJO itself is projected to change is incomplete.

d. Another limitation is you only examine January mean state changes. The January Ks should feature more of a barrier in the West Pacific as compared to the shoulder months as the East Asian subtropical is strongest in January. It is conceivable that changes in the

mean wind will matter less if you look at e.g. November or March. This limitation also isn't mentioned in the discussion.

More generally, the authors need to acknowledge in the discussion that they examine only a subset of the possible changes in the MJO that may actually occur. The current statement on lines 407-409 is very much overstated at present and needs to be deleted. I don't think the authors need to actually perform additional analysis to satisfy these point, rather add a paragraph or so that more fully discusses limitations.

2. Rossby wave source and Ks are both more clearly related to upper level metrics of the flow than lower level metrics. It is important to confirm that results are generally unchanged if some metric of the upper level flow is examined.

3. Figure 5a: This climatology of Ks doesn't look very much like that of Hoskins and Ambrizzi, their figure 3, especially in the subtropical East Pacific. If you plot Ks of reanalysis data, does the correspondence improve?

Minor changes

Line 12 : I would replace "Rossey wave excitation" with "Rossby wave source", to be more precise

Line 87-88 this sentence is missing a few words

Line 226 panel b hasn't been discussed yet

Line 324 "models' mean-state" shouldn't this be amplitude of the MJO related heating?

Line 326: "linearly sensitive to the mean state" – this was confusing to me

Line 322-323 "to changes to the MJO itself" I found these words confusing and unnecessary