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Comment on wcd-2021-9

David Straus (Referee)

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-RC1>, 2021

The paper presents a well thought-out and intelligent approach to determining the mechanisms behind changes in the strength of the Madden-Julian Oscillation (MJO) teleconnections in future climate simulations. In particular the paper explores the roles of the changes in basic state (including static stability and the basic state winds) as well as future changes in the characteristics of the MJO itself. The results also indicate the degree to which these mechanisms differ in the various CMIP 6 models. The results are fairly convincing. Subject to some revisions, the paper should be published.

There is one relatively major point I would like to bring up, namely the choice of the teleconnection metric. I should first say that I applaud the use of the temporal variance, since in contrast to many other MJO studies, this choice implies the recognition that the MJO forcing (and hence the wave-train response) propagates in a continuous fashion, and does not make the stationary-wave response hypothesis so often invoked. However, I am puzzled by the choice of the low-level v-wind field. The authors state that: "We use low-level meridional wind in this calculation (instead of other dynamically relevant variables like mid-tropospheric geopotential height, for example), because the MJO primarily drives surface temperature variability across North America via low-level temperature advection ..." This is taking a rather limiting point of view, appropriate if the North America T_{sfc} teleconnection were the main teleconnection of interest. But I think that the authors should (and do) take a wider point of view, for which a teleconnection metric involving perhaps the upper-level (300 hPa) temporal variance of height or v-wind would be more relevant, and would enable the authors to compare their results with the wealth of literature using upper-level fields to gauge the MJO teleconnection. Applying this metric would mean simply re-running the various codes the authors have on a different field, and the comparison between the results for the upper-level and low-level metrics would be quite illuminating.

There are a few conceptual points on which the authors need to expand further (if briefly). As described, the linear baroclinic model is damped enough to prevent the growth of baroclinic waves, which means that the role of the storm-track transient eddy convergences of heat and momentum are not taken into account explicitly. However, since

the mean state u-wind configuration is taken from the CMIP6 simulations, the eddy-mean flow feedback is taken into account implicitly. This should be mentioned in the discussion of the LBM. Secondly, Hoskins and Ambrizzi (1993) show that the waveguide is formed in areas near the maximum in K_s as a function of latitude, and has a width that extends between the points of inflection on either side of the maximum (see their Figure 2E). The width of the waveguide may play as large a role as the maximum value of K_s . The discussion of Figure 2 should be modified in this light.

Minor Points:

(1) The paragraph regarding effect of propagation speed (lines 59-64) should refer to Yadav and Straus (2017) who discuss the observed teleconnections from observed fast and slow MJO episodes.

(2) Figure 1 – is the “Heating rate” (y-axis) the heating amplitude A in equation 1?

(3) The definition of the teleconnection strength is confusing. In lines 183-184: “...calculate the teleconnection strength at each point as the square root of twice the variance of the ensemble mean meridional wind at 850 hPa” Presumably you mean twice the *temporal* variance (and not the ensemble spread). Also, do you take the ensemble mean first and then take the variance (as indicated in the text above), or take the variance of each run and then take the model mean (as indicated in the caption to Figure 2)?

(4) lines 209 – 213: the eastward extension of the teleconnections near California is not convincing from Figures 2c-2e.

(5) lines 239-241: is the multi-model mean teleconnection strength different from 0 in any meaningful way?

(6) Rossby wave source: equation (2) – what is the definition of the “anomaly” for each model. Is this the difference between future and historical runs?

(7) line 303-305: Figure 5. This figure is not explained well. I am still not sure which is the index in this figure and which is the geographically varying field. I think the index is the area-averaged teleconnection amplitude and the varying field is K_s (albeit smoothed by a running 15 x 15 degree average). This needs to be clarified.

Since the change in K_s between historical and future runs is not shown, how do we interpret the results of Figure 5 in the context of the general statement made in lines 352-353: "Over North America, the change in the mean state alone also leads to decreases in the teleconnection amplitude for most CMIP6 models"?

How does Figure 5 explain (as stated in the conclusions): "For teleconnections to North America in particular, we have identified a region over the eastern Pacific where changes to the winds appear to explain much of the inter-model spread"?

(8) Figure 6a,d : Does a change in fractional area of 1 mean a doubling in size of areas where teleconnections > 0 ?

(9) line 396: "Overall, over the North Pacific, despite a relatively large range of -21 to 0.6 % $K-1$..." The rest of the sentence never states what quantity is being referred to.

References:

Yadav, P. and D. M. Straus, 2017: Circulation Response to Fast and Slow MJO Episodes. *Mon. Wea. Rev.*, 145, 1577-1596.