Referee comments in black. Author responses to referee comments in red.

Report #1 (Anonymous Referee #2)

The manuscript is substantially improved compared to the last version. The discussion is more thorough and clear. The method is also clearly described and the results are convincing and interesting. I thus recommend acceptance for the publication after some minor revision.

Minor comments:
1. L6. "over much of North America". Better to provide the specific region.
We have made the suggested change.

2. L10-11. A little misleading. If I understand correctly, this conclusion is from the LBM simulations using the output from CMIP6? If this is the case, the authors may want to clearly point out the results are based on LBM forcing with the CMIP6 to avoid confusion.
We have made the suggested change.

3. It would be nice if the authors can discuss a little bit about the relative importance of the MJO and mean winds in the teleconnection uncertainty in the abstract. For example, which one is more important?
We have included the following sentence in the abstract,
"While quantitatively determining the relative importance of MJO versus mean state uncertainties in determining future teleconnections remains a challenge, the LBM simulations suggest that uncertainty in the mean state winds is a larger contributor to the uncertainty in future projections of the MJO teleconnection than the MJO."

4. L32-33. Better to clearly discuss how models agree on the future changes in MJO teleconnections over the NP. I see this is discussed in the later paragraphs, but it is better to also state here that the MJO teleconnections near the West Coast will be stronger? (Zhou et al. 2020)
We have made the suggested change.

5. Eq. (3). Please see Eq. (8) in Li et al. 2015 (reference provided in the first review comment). If zonal asymmetry is included in the zonal wind (as the authors argued here), zonal gradient of absolute vorticity will not be zero as in Eq. (3). This form already assumes that the zonal wind is zonally symmetric, although the authors used the full zonal wind to calculate Ks. The results are therefore inconsistent. If the authors can show that the zonal gradient of absolute vorticity is small might be helpful.
Many previous studies have used the same zonally asymmetric form of the stationary Rossby wave number that we have used here with the full (i.e., non-zonally uniform) zonal wind (see Equatin 2.4 in Hoskins and Ambrizzi, 1993). Eq. (3) does not assume that the zonal wind is zonally symmetric. As you note in the subsequent comment, we do ignore meridional wind. We have updated the following sentence (bold = new):
"Analyses of the stationary Rossby wave number have been useful for assessing how the mean state winds affect Rossby wave propagation
\citep[e.g.,][]{Henderson2017,Karoly1983,Tseng2020,Wang2020,Zheng2020}. The stationary Rossby wave number on a Mercator projection (to account for spherical geometry) **for a zonal flow** is defined as..."

Reference:
Hoskins, B. J., & Ambrizzi, T. (1993). Rossby Wave Propagation on a Realistic Longitudinally Varying Flow. *Journal of the Atmospheric Sciences*, 50(12), 1661–1671.

6. L387. Although previous studies generally used this form, the authors may still want to provide their own reason. Please see Li et al. 2015 who showed that meridional wind will be more important in determining the inter-hemispheric wave propagation. While here, the authors are more interested in the impacts of the zonal jet in NH only.
We have included the following to the section containing this content (bold = new):
"More work is needed to understand how changes to the mean flow will impact future MJO teleconnections, which our results indicate are likely a large source of inter-model spread in future projections of MJO teleconnections. **For example, consideration of meridional wind as in \citet{Li2015}, which is omitted in the form of the stationary Rossby wave number that we use, may reveal mechanisms due to changes to the meridional, rather than the zonal wind. We leave this for future work.**"

7. Section 4. The authors may want to point out their results are based on the LBM and may have difference if using CMIP6 simulations. A little more comparison with the results found in Zhou et al. 2015 will be helpful.
We have added the words "LBM-simulated" in multiple places in the conclusions to hopefully alleviate this confusion.

Report #3 (Referee #1: David Straus)

(1) In the discussion of the experiments to test changes in the MJO characteristics, it is mentioned once only (line 167) that the MJO characteristic zonal wave number is decreased (zonal scale is increased). Thus an increase in the value of the x-coordinate in Figures 7(d)&7(f) (labeled "Wavenumber") correspond to a decrease in wave number. This is a bit confusing.The authors should emphasize the fact the wave number is decreased, and relabel the x-axis in panels (d) and (f) of Figure 7.
We have added one more place where we clarify that the decrease in the zonal wavenumber corresponds to an increase in the zonal scale of the MJO. However, the horizontal axis that you point out in Figure 7 does not actually reference the change to the MJO, but rather the change in the teleconnection (as measured in geopotential height) that results from the prescribed change to the MJO. We point the referee to the following sentences where Figure 7 is introduced, which we have written to hopefully alleviate this confusion,
"Figure 7 summarizes the results of these simulations. As in Figure 4, the horizontal axis shows the regional mean difference in the teleconnection amplitude while the vertical axis shows the fractional area of the region that has stronger teleconnections (perturbed MJO compared to control MJO)."

(2) The y-axis label to Figure 2(a) is wrong: it is geopotential height that is plotted, not meridional wind speed.
Thank you for pointing out this error. We have fixed the label.