

Interactive comment on “Subseasonal Midlatitude Prediction Skill Following QBO-MJO Activity” by Kirsten J. Mayer and Elizabeth A. Barnes

Anonymous Referee #1

Received and published: 20 January 2020

Review of " Subseasonal Midlatitude Prediction Skill Following QBO-MJO Activity" Author(s): Kirsten J. Mayer and Elizabeth A. Barnes

Recommendation: Major revisions

The authors analyze the possibility that the state of the QBO can lead to enhanced predictability of MJO teleconnections. After first demonstrating that the models capture in a gross sense the MJO teleconnections in the Pacific and Atlantic sector, and that the stripey-ness of the teleconnection is generally similar in all QBO phases, the authors compare the ACC for different combinations of active/inactive MJO and EQBO in their various phases. They find significantly higher ACC when both are active, with the increase in ACC even larger for WQBO as compared to EQBO.

[Printer-friendly version](#)

[Discussion paper](#)



I had trouble accepting the conclusions the authors reach from figures 4,5, and 6 regarding any interaction between the QBO and the MJO in their extratropical teleconnections. Rather, these figures seem to separately reflect skill added alternately by either the QBO or the MJO. Please see my general comment below. My other two major comments concern the accessibility of this paper to someone not already very familiar with the author's previous work, and they should be easier to address.

Major comments:

1. While I appreciate the power of the STRIPES analysis, I must point out that the first time I read the paper I did not understand at all what the authors were doing. Only after skimming Jenney et al 2019 and looking at supplemental figure 1 did I fully understand what was happening. I worry that a casual reader may be less patient. To be constructive, I suggest that supplemental figure 1 be included in the main text, and I would also suggest adding a figure of lat vs. lon Z500 with a few panels corresponding to different periods explicitly showing how the wave train leads to Z500 alternating anomalies. I realize this is already in Jenney et al but a new, at first not intuitive, index needs a certain amount of repetition. As an aside, I was surprised that the STRIPES was just as strong in the European sector as in North Pacific/ North America. I would have expected a stronger response closer to the Pacific.

The ACC results also indicate that the additional predictability from the MJO is mainly in the Atlantic sector too rather than the North Pacific (Figures 4 and 5). To me this is counter-intuitive, as the MJO should immediately and directly affect the North Pacific, especially in the first few weeks, and then affect the Atlantic more weakly later on. Additional discussion would be helpful. (I can try to reason why my intuition is incorrect, but really the authors should help with this)

2. Between lines 192 and 203 the authors form an argument that I don't find convincing. As this argument underlies the rest of the paper, this is a major issue.

To this reviewer, the clearest evidence that the QBO can enhance MJO related pre-

diction skill would be if the difference in ACC between EQBO/MJO and EQBO/noMJO or between WQBO/MJO and WQBO/noMJO is larger than the difference between noQBO/MJO and noQBO/noMJO. Based on supplemental table 1 it seems that this kind of comparison isn't possible due to possible contamination by the ENSO signal, though perhaps the authors could compute the mean Nino3.4 index for each composite included on supplemental table 1. If the mean Nino3.4 value for each composite is small, then La Nina and El Nino events balance out and the net prediction skill added by ENSO is small.

Instead the authors evaluate a pair of differences that only partially reflect on whether the QBO is enhancing MJO related prediction skill, but rather reflect alternately on whether there is prediction skill associated with the MJO, and separately whether is prediction skill associated with the QBO (in Figures 4-6). Unless the authors perform the test in the previous paragraph, there is no basis for this statement of the authors "When these two significances appear together, we can say that a particular strong QBO increases the impact of the MJO on midlatitude prediction skill".

Stated another way, the difference EQBO/MJO minus noQBO/MJO does not reflect anything about the MJO per se. Rather it reflects skill associated with EQBO. Hence I don't find figure 6 useful, other than the fact that it shows that the QBO enhances skillful forecasts in the Atlantic sector (which is a nice result, and consistent with Garfinkel et al 2018 already cited and Boer and Hamilton 2008, but the authors interpretation is completely different). In order for Figure 6 to have any bearing on the MJO, the authors need to include an additional figure showing EQBO/noMJO minus noQBO/noMJO to which we can compare the difference shown in figure 6. If there is a significant difference between EQBO/MJO minus noQBO/MJO as compared to EQBO/noMJO minus noQBO/noMJO, then there is evidence that there is some mutual interaction between the MJO and the EQBO. The authors could then rinse and repeat for WQBO.

In its present form, the authors analysis only convinces me that both the QBO or the MJO separately enhance predictability on S2S timescales in these models as com-

[Printer-friendly version](#)

[Discussion paper](#)



Interactive comment

pared to noQBO or noMJO.

3. I found section 3.2.5 extraneous and hard to understand without first skimming Tseng et al 2018. Consider deleting.

Minor comments: Line 13 "7-14 days", actually there is enhanced predictability up to day 28 in figures 4-6. Why limit to 14 days?

Line 77 There is earlier work that argues that the QBO may modulate ENSO teleconnections. See Garfinkel and Hartmann 2010, Richter et al 2015, and Hansen et al 2016 Technical comments Line 2 stationary Rossby wave **and** tropica-extratropical teleconnections

Line 19 excitation of **quasi**stationary Rossby waves (the MJO can't force stationary waves on monthly mean or seasonal mean timescales)

Line 126 the reference to figure 3 seems incorrect. Figure 3 shows something else entirely.

Figure 1, title of bottom-right panel is incorrect (It probably should be WQBO-MJO)

Garfinkel, C.I. and Hartmann, D.L., 2010. Influence of the quasi- \bar{R} biennial oscillation on the North Pacific and El Niño teleconnections. *Journal of Geophysical Research: Atmospheres*, 115(D20).

Richter, J.H., Deser, C. and Sun, L., 2015. Effects of stratospheric variability on El Niño teleconnections. *Environmental Research Letters*, 10(12), p.124021.

Hansen, F., Matthes, K. and Wahl, S., 2016. Tropospheric QBO–ENSO interactions and differences between the Atlantic and Pacific. *Journal of Climate*, 29(4), pp.1353-1368.

Boer, G.J. and Hamilton, K., 2008. QBO influence on extratropical predictive skill. *Climate dynamics*, 31(7-8), pp.987-1000.

[Printer-friendly version](#)

[Discussion paper](#)



Interactive
comment

[Printer-friendly version](#)

[Discussion paper](#)

