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

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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 as aside, I was surprised that the STRIPES was just as strong in the European sector as in North Pacific/ NorthAmerica.

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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)

Response: We agree that the STRIPES index is new and may not be familiar to the reader. Therefore, as suggested, we have added supplemental Figure 1 to the main paper in Section 2.3: Methods. We have additionally added two panels of spatial z500 anomalies at lead 12 days following phase 6 and phase 2 of the MJO to additionally aid the reader in understanding STRIPES. We have also included additional text: "Mid-latitude circulations can be modified by quasi-stationary rossby waves initiated by MJO convective heating. In a phase-lead diagram (e.g. Figure 1a), these are apparent as slowly alternating-sign z500 anomalies with lead following a specific phase of the MJO. In addition, the MJO is a propagating phenomenon with a phase speed of about 5-8 days/phase. Therefore, if there is a teleconnection signal 10 days following phase 2, this signal is likely also present 5 days following phase 3 in the same region, in a composite sense. On a phase-lead diagram, this is apparent as a diagonal line or 'stripe' slanted at the phase speed of the MJO (Figure 1a). Therefore, if a region is sensitive to the MJO, we expect alternating z500 anomaly stripes approximately sloped at the average phase speed of the MJO, as in Figure 1a, which we refer to as the 'stripey-ness'. For further intuition of the phase-lead diagram, Figure 1a and 1b show composite z500 anomalies for the domain around 45\degree N and 5\degree W (marked by the white X) 12 days following phase 6 and phase 2, respectively. The value of the box in the phase-lead diagram is the same as the value plotted at the X in Figure 1b,c." (See attached Figure 1 for figure.) In regards to the STRIPES result of the North Pacific, the reviewer mentions that the Pacific and European sectors have similar STRIPES values. We hypothesize that the Atlantic and European sectors may have similar STRIPES values to the Pacific from enhanced blocking over the Atlantic and Europe following the MJO (Henderson et al. 2016) leading to more persistent stripes. This explanation has been added to Section 3.1: Extratropical Sensitivity: "Interestingly, the Pacific and Atlantic sectors have similar STRIPES values. One may expect higher STRIPES values over the Pacific since it is generally known to have a strong response to the MJO. We hypothesize that the Atlantic and European sectors may have similar STRIPES values from enhanced blocking over the Atlantic and Europe following the MJO (Henderson et al. 2016) leading to large STRIPES values." In terms of the ACC result showing additional prediction skill from the MJO in Atlantic/European sector rather than over the Pacific, this is likely because prediction skill on Week 1 timescales is already generally good over all locations, and it is on this weekly timescale that the Pacific is most strongly impacted by MJO teleconnections. Therefore, we may not expect the prediction skill to be significantly different over the Pacific for these early leads. Where we would expect the MJO to provide additional prediction skill is on longer than one week timescales. This additional explanation has been added to Section 3.2.1: "Note that forecast models already have relatively good prediction skill at one week leadtimes, and therefore, we would not expect the prediction skill to be significantly different over the Pacific following MJO versus non-MJO events for these early leads. Where we would expect the MJO to provide additional prediction skill is on longer than one week timescales."

2. Between lines 192 and 203 the authors form an argument that I don't find convincing. As this argument underlies the reset of the paper, this is a major issue. To this reviewer, the clearest evidence that the QBO can enhance MJO related prediction 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

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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 compared to noQBO or noMJO.

Response: This reviewer mentions that the two types of significance the authors use are insufficient as evidence for whether the QBO can enhance MJO related prediction skill. We appreciate this comment as the authors now realize how the results, as originally posed, were confusing. In fact, we completely agree with the reviewer on what must be done to make this convincing, but realize now that some of the important steps were too quickly glossed over since they ultimately had little impact on the result. With this in mind, we have now rewritten the results section on prediction skill, and in the process, added some additional analysis to make the argument even stronger in a statistical sense. Given the statistical tests added, the resulting figures have changed - however, the overall story remains the same as requirements 1-3 were already considered/included in the earlier version of the paper. We want to thank this reviewer for their insightful comments to help us improve the heart of this paper. Specifically, we now have added 3 “requirements” that can hopefully now be more clearly stated and followed throughout the results discussion and figures. The first requirement is the presence of an MJO impact on midlatitude prediction skill during specific phases of the QBO, where an ‘MJO impact’ on midlatitude prediction skill is defined as a significant difference in midlatitude ACC between active MJO and inactive MJO events. The second requirement is that the magnitude of the significant MJO impact under strong QBOs is significantly larger than the significant MJO impact under NQBO. As also highlighted by the reviewer, the second requirement is calculated through a comparison of the MJO impact during strong QBOs to the MJO impact during NQBO. These two requirements together ensure that (1) there is an MJO impact and (2) that this impact is enhanced during strong QBOs compared to neutral QBOs. The third requirement is the presence of regions/leads where E/WQBO-MJO events significantly lead to higher prediction skill than NQBO-MJO given requirement 1 and 2 are satisfied. We applied this requirement to see if regions with enhanced MJO impacts during strong QBOs also have overall greater prediction skill following active MJO events compared to NQBO-MJO events, as regions of enhanced prediction skill is the focus of this paper. The reviewer also points out the small sample size of NQBO-noMJO. We agree the sample size is small and while there is not much that can be done about it, our new results include significance tests at every step that take into account the small sample sizes. We include the following discussion in the paper: “It should be noted that inactive MJOs during NQBO events with ENSO removed only occur 12 times in ECMWF and 3 times in NCEP. If ENSO events are not removed, the sample sizes increase to 47 and 52, for ECMWF and NCEP respectively (see Table S1). While there is shading across all longitudes when ENSO is removed (Figure S4), when we calculate the MJO impact during NQBO when ENSO is included (Figure S5), we see that much of the shading east of 0\degree is not apparent. The presence of skill east of 0\degree when ENSO is not included may be due to small sample sizes of the NQBO events. Thus, when comparing MJO impacts between strong and neutral QBOs, it is important to keep sample size in mind. That being said, the statistical analysis we have applied here for requirements 1-3 account for the small sample sizes in the analysis."

3. I found section 3.2.5 extraneous and hard to understand without first skimming Tseng et al 2018. Consider deleting. Response: Thank you for this comment. The other reviewer had similar concerns and so we have decided to remove Section 3.2.5: Northern Hemisphere Prediction Skill and Sensitivity.

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? Response: Thank you for pointing this out. It was an oversight, and we have updated it to say "7-28 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 Response: We have added QBO effects on ENSO teleconnections to the ENSO discussion in the methods. "Some earlier research indicates that ENSO has a limited impact on the QBO-MJO interaction (e.g. \citealt{Yoo2016, Nishimoto2017}); however, recent work on QBO-MJO teleconnections has shown a possible dependency of results on ENSO \citep{Son2017, Wang2018, Sun2019}. In addition, other research suggests that the QBO affects ENSO teleconnections \citep{Garfinkel2010, Richter2015, Hansen2016}, which may consequently impact the MJO and its teleconnections."

Technical comments: Line 2 stationary Rossby wave **and** tropical-extratropical teleconnections Response: The quasi-stationary rossby waves are tropical-extratropical teleconnections, which is why we have a comma instead of 'and'.

Line 19 excitation of **quasi**stationary Rossby waves (the MJO can't force stationary waves on monthly mean or seasonal mean timescales) Response: Fixed. Thank you.

Line 126 the reference to figure 3 seems incorrect. Figure 3 shows something else entirely. Response: This should say Supplemental Figure 3. This has been corrected. Thank you.

Figure 1, title of bottom-right panel is incorrect (It probably should be WQBO-MJO) Response: Fixed. Thank you.

Please also note the supplement to this comment:
http://www.weather-clim-dynam-discuss.net/wcd-2019-13/wcd-2019-13-AC1-supplement.pdf

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Interactive comment on Weather Clim. Dynam. Discuss., https://doi.org/10.5194/wcd-2019-13, 2019.

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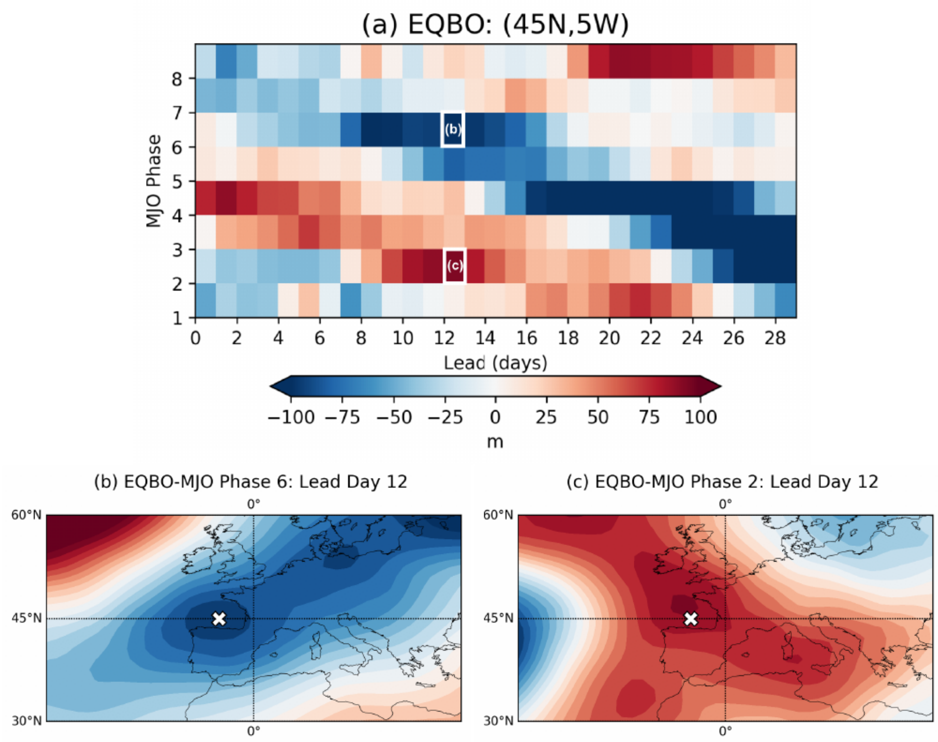

Figure 1. (a) Boreal winter (DJF) composite ERA-I z500 anomalies subsampled to ECMWF initialization dates (1995-2016) for each MJO phase during EQBO vs lead at 45N and 5W. White boxes and text denote corresponding figure in bottom panel. The bottom panel includes composite ERA-I z500 anomalies subsampled to ECMWF initialization dates (DJF, 1995-2016) over Europe for (b) Phase 6 and (c) Phase 2 at lead day 12. The white X denotes 45°N and 5°W.

Fig. 1.

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