

Weather Clim. Dynam. Discuss., referee comment RC1
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Comment on wcd-2021-82

Anonymous Referee #1

Referee comment on "Modulation of the El Niño Teleconnection to the North Atlantic by the Tropical North Atlantic during Boreal Spring and Summer" by Jake W. Casselman et al., Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2021-82-RC1>, 2022

SUMMARY:

The study is about inter-basin interaction of the Walker Cells and Gill responses forced by ENSO-related SST in the Pacific and Tropical North Atlantic during boreal spring and summer, as well as their teleconnection to the North Atlantic. Analyses from reanalyses and atmospheric model experiments are performed.

I think this is an interesting and important topic. However, currently, I find that the main problem is the presentation/writing lacks connections among the every smaller piece of results described. This is perhaps due to the reason that there is a lack of explanations on how the different pieces fit together. The paper in some parts reads like a mere listing of many features found, and it is easy to become lost for the readers. I describe this issue in a number of specific subsections below.

I also have some questions about clarifying what relationships are being modulated and what are the modulators.

MAJOR COMMENTS:

Sects. 3.1, 3.2: There are various features and properties identified and described. I find it somewhat difficult to digest because I am not quite clear about what are the reasons for highlighting those specific features. I suggest to consider the main aims of the study when selecting features that are essential to focus on, and tell us also exactly why we should look at them in relations to the central questions of the study and how they link to the

next subsections.

Sect. 3.3: This is again a rather long catalogue of different combinations, comparisons and observations of the model results. It is not described clearly why all the different results should be of interest in connection with the central themes. Yes, ok, there is a linear constructive/destructive effect, there is also some nonlinear effect in certain features, and there are some agreements and disagreements with the reanalysis data. But try to tell us why they are important in their own right and/or for the central themes.

Fig. 8: You are making comparisons between the correlation coefficients r . I have doubt that some of the differences you describe are statistically significant. I think you should at least check with Fisher z-transformation, find the confidence interval, then transform it back to r . Then you can present each correlation coefficient in a range, based on the significance level you decide. If the uncertainty range overlap between two experiments then the difference is not statistically significant. Here is an example that presents such information in their results:

Revisiting the relationship between jet position, forced response, and annular mode variability in the southern midlatitudes

<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016GL067989>

(see their Figure 2).

Fig. 8: Are the scatters (dots) in each experiment due to internal variability in the same experiment? Or each dot represents one experiment? The SST forcing is the same for a same season year by year, right? I am a little surprised by the 'large' variability of the Walker Index. I thought tropical circulations are much less 'noisy' under the same SST forcing. Would it be useful to add the reanalysis data (under El Nino condition if you like) in these plots for comparison? I understand they won't be then comparing the same things, but it could give us an idea both on what the variability and the relationships between these indices are like in the reanalysis data.

Lines 440-448: I am not sure this argument works nicey. You are using the observation that there is near-zero RWS from non-zero Walker Index anomaly for P (from Fig. 8a?) and that RWS is related to the Walker Index, to argue that the modulation from the tropical Atlantic in the AP is important to improve relationship of RWS and the Walker Index. But the AP experiment also has a problem of having average near-zero Walker Index anomaly, so how does that result in a non-zero RWS anomaly that is obtained? Maybe there is another factor not considered here and the Walker cell is not suitable factor. Also, you might have mixed up two things in your descriptions here: the average response of the experiments (compared to Control experiment) and the modulations by the SST forcings on the internally-generated RWS and Walker Index relationships. Or it's possible I have misunderstood Fig. 8, see my question about Fig. 8 above. In any case, I think further improvement in the descriptions and making the arguments is required.

Paragraph in Line 464, Fig. 8b, c, and the title of the paper: If I have understood Fig. 8 correctly, I think Fig. 8 is mostly presenting something fundamentally different from Figs. 6, 7 (and 9, 10). For eg, Fig. 8b, c is about how both ENSO and the related tropical North

Atlantic SST modulate the relationships between the Walker Index and EA (and also NAO), whereas Figs. 6, 7 instead are mainly about the point of how ENSO teleconnection is modulated by the tropical North Atlantic (like the paper title). In the former, ENSO is one of the modulators; in the latter, ENSO teleconnection is being modulated. Therefore, Fig. 8 is also not directly related to the title of the paper. (See also the previous comment).

MINOR COMMENTS:

Title of paper: Most papers on similar subject would be on the cold seasons, if this is about boreal spring and summer, I think it should be reflected in the title.

Paragraphs in Lines 67-82: Shorten these substantially. They read like an information dump and are not easy to follow. They also appear to disconnect the subsequent and preceding paragraphs, where you are already building up to the aims of the study. So, they might be relocated to an earlier part of Intro.

Sentences in Lines 88-92: Which seasons/months do you focus on?

Sect 2.2 heading: Change to "Model description and experiments".

Line 122 "We chose to multiply the forcing by 4": Is this a realistic forcing? Would this be an obstacle for using the model results to interpret observed relationships or to understand the real world, considering also there might be the potential issue of nonlinear teleconnections due to forcing amplitudes?

Paragraph in Line 126 and last ever: WHY do you want to "remove the indirect influence from stratospheric variability"? Again, what does this mean in the limitations of using the models results to understand the real world? Isn't it easier to not apply the relaxation in the first place? Perhaps you should also perform experiments without the relaxation now? Why wait for another future study? Are the experiments very expensive to run?

Line 123 "we aim to determine the importance of the TNA following an El Niño only": Is there a reason choosing to only look at El Niño?

Line 261 and other places: "Sensitivity experiments": In my view, these are not sensitivity experiments because we don't get a sense of how sensitive the model response is to the change of an input strength.

Line 334 and other places, "sensitivity runs": Same as above, I would not call these sensitivity runs.

Figs. 6, 7 and Figs. 9, 10: Is it really essential show both pairs of figures (psi and Z200)? Maybe it's enough to only show results in psi, and reuse the same figures in the description in Sect. 3.4. Extratropical (for a geostrophic argument) geopotential height and psi at 200hPa are related with a factor of the Coriolis parameter.

Fig. 8: Have you specifically refer to or describe the dark straight line and the legend for R written in black?

Fig. 8: Sorry to be pedantic. The correct way to describe these plots is "Y vs. X", not "X vs. Y" as you write.

Line 382: This description is not precise or not correct. Fig. 9e obviously shows an EA pattern, yet you say it is "less present". Perhaps what you try to say is simply the negative EA pattern in AP is not as strong as the negative EA pattern from A+P (?).