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Comment on wcd-2021-15

Anonymous Referee #2

Referee comment on "Resampling of ENSO teleconnections: accounting for cold season evolution reduces uncertainty in the North Atlantic" by Martin P. King et al., Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2021-15-RC2>, 2021

Review of "Resampling the ENSO teleconnections: accounting for the cold season evolution uncertainty in the North Atlantic"

The manuscript by M. P. King, Camille Li, and Stefan Sobolowski focuses on the quantifying uncertainty of the winter ENSO teleconnection to the North Atlantic sector. The main message that arises from this study is that accounting for the winter evolution of this teleconnection and thus separating the sea level pressure (SLP) response into early and late winter reduces the uncertainty of this teleconnection. In the manuscript, the uncertainty of the teleconnection is measured using different bootstrapping methodologies and displayed using Taylor diagrams, which displays information of the pattern-correlation and the amplitude of the teleconnection. The authors do not attempt to better understand the underlying mechanism behind the intraseasonal variability of the teleconnection but just focus on pure statistical aspects of the SLP response to ENSO. The paper briefly touches on the topic of asymmetry between La Niña (LN) and El Niño (EN) and the nonlinearity associated with the location of the maximum SSTs in the tropical Pacific, i.e. East and Central Pacific ENSO, events and find that considering EP and CP events separately leads to larger confidence intervals. The paper finishes with a detailed discussion on the confidence intervals and t-tests and concludes that for the present application using a simple t-test is as good as using a more sophisticated bootstrapping technique, which agrees with the common practice in climate science to use t-test to test the statistical significance in seasonal teleconnections.

Overall, I think this paper reexamines an interesting and relevant topic for the climate community. Although many of the findings presented in the paper are not novel and mostly expected, I think it provides an exhaustive and detailed quantification of the uncertainty in ENSO-North Atlantic teleconnection. Therefore, I just have a few general minor comments that should be included in a revised version of the manuscript.

General comments.

- The authors seem to have missed a few recent and relevant studies examining the

asymmetry and non-linearity in the ENSO teleconnection to the North Atlantic. It would be nice that the papers cited below are included in the new version of the manuscript. At least when discussing that these model results disagree in some points when trying to identify nonlinearities in the ENSO-North Atlantic teleconnection (e.g. lines 194-201)

Hardiman, S. C., Dunstone, N. J., Scaife, A. A., Smith, D. M., Ineson, S., Lim, J., & Fereday, D. (2019). The Impact of Strong El Niño and La Niña Events on the North Atlantic. *Geophysical Research Letters*, 46(5), 2874–2883. <https://doi.org/10.1029/2018GL081776>

Jiménez-Esteve, B., & Domeisen, D. I. V. (2020). Nonlinearity in the tropospheric pathway of ENSO to the North Atlantic. *Weather and Climate Dynamics*, 1(1), 225–245. <https://doi.org/10.5194/wcd-1-225-2020>

Trascasa-Castro, P., Maycock, A. C., Scott Yiu, Y. Y., & Fletcher, J. K. (2019). On the Linearity of the Stratospheric and Euro-Atlantic Sector Response to ENSO. *Journal of Climate*, 32(19), 6607–6626. <https://doi.org/10.1175/JCLI-D-18-0746.1>

Weinberger, I., Garfinkel, C. I., White, I. P., & Oman, L. D. (2019). The salience of nonlinearities in the boreal winter response to ENSO: Arctic stratosphere and Europe. *Climate Dynamics*, 53(7–8), 4591–4610. <https://doi.org/10.1007/s00382-019-04805-1>

- I agree with reviewer 1, that it would be good to show the individual monthly mean evolution of the teleconnection from Nov to March.
- The authors analyze the effect of separating the ENSO events into CP and EP events. I think, given the nature of the paper, it would be important to analyze what is the effect of the ENSO magnitude, i.e. consider strong ($Nino3.4 > 1.5/2SD$) and moderate ($1SD > Nino3.4 > 1.5/2SD$) ENSO events separately. I think the current length of the paper would allow for the addition of this analysis.
- Section 3.3 discusses different methods to compute confidence intervals. However, the conclusion of this section is rather “boring”. Figures 7 and 8 look almost the same for all the different methods employed. I understand that the reason why the authors have decided to put these figures in the main text is to show that they are actually very similar. Nevertheless, I would recommend putting these two figures as an appendix as they do not provide a lot of quantitative information, but they are a justification for the methods employed in the first part of the study.

Specific comments (line by line):

Line 10: What does “some confidence in the signs” means here? Is there a more objective way to state this?

Lines 11-12: Is the lower confidence in the amplitude due to internal North Atlantic variability or due to ENSO diversity (asymmetry and nonlinearity)?

Line 20: All teleconnections are uncertain in amplitude due to internal variability no? Maybe one should say if this amplitude uncertainty is similar or smaller than in the North Atlantic?

Line 22-23: What do you mean by “detectable response”? Do you mean that the sign of the teleconnection is uncertain?

Line 25: In my opinion, the ND pattern is not similar to the NAO pattern, as one center is much stronger than the other, so it is not just a shift, but more similar to a monopole that resembles the EA pattern (e.g. Woollings et al., 2010)

Woollings, T., Hannachi, A., & Hoskins, B. (2010). Variability of the North Atlantic eddy-driven jet stream. *Quarterly Journal of the Royal Meteorological Society*, 136(649), 856–868. <https://doi.org/10.1002/qj.625>

Line 26: I think we cannot say that the teleconnection is “the NAO pattern” or NAO-like pattern, but that it projects onto the NAO pattern. Related to this point, the authors might refer to the paper by Mezzina et al. (2020a,b).

Mezzina, B., García-Serrano, J., Bladé, I., & Kucharski, F. (2020). Dynamics of the ENSO Teleconnection and NAO Variability in the North Atlantic–European Late Winter. *Journal of Climate*, 33(3), 907–923. <https://doi.org/10.1175/JCLI-D-19-0192.1>

Mezzina, B., García-Serrano, J., Bladé, I., Palmeiro, F. M., Batté, L., Ardilouze, C., ... Gualdi, S. (2020). Multi-model assessment of the late-winter extra-tropical response to El Niño and La Niña. *Climate Dynamics*, 1, 3. <https://doi.org/10.1007/s00382-020-05415-y>

Line 30: Maybe you could add the paper by Jimenez-Esteve and Domeisen (2018) where the tropospheric mechanisms in late winter are analyzed together under the influence of the stratospheric pathway.

Jiménez-Esteve, B., & Domeisen, D. I. V. (2018). The tropospheric pathway of the ENSO–North Atlantic teleconnection. *Journal of Climate*, 31(11), 4563–4584. <https://doi.org/10.1175/JCLI-D-17-0716.1>

Line 34: Actually, Hardimann et al. (2019) also find that this Rossby wave train is important throughout the season, mainly for strong EN events, but it is more muted for moderate EN or LN events when the stratospheric pathway is active in late winter.

Hardiman, S. C., Dunstone, N. J., Scaife, A. A., Smith, D. M., Ineson, S., Lim, J., & Fereday, D. (2019). The Impact of Strong El Niño and La Niña events on the North Atlantic. *Geophysical Research Letters*, 46(5), 2874–2883. <https://doi.org/10.1029/2018GL081776>

Line 56: “larger” than what?

Line 58: A “more optimistic” perspective compared to? DESER17? I am not sure if “optimistic” is the right term to use here as optimism is subjective and not objective science.

Line 66: Why are the two citations after a full stop?

Line 68-69: For what months or running mean is the Nino3.4 averaged?

Line 72: How many years do you draw every time from the E_0 and L_0 sets every time (2000 times)? In your expression of C*, E* and L* are averaged before computing the difference, is this correct? Please clarify.

Lines 89-98: After reading this subsection, I understand the meaning of what is represented in the Taylor diagram, i.e the amplitude and spatial correlations, but I struggle to understand what exactly each point in the Taylor diagram represents. For example, what do the $||$ $||_2$ operators symbolize? A better explanation of how the spatial information is averaged in this diagram would be desired.

Line 103: Why using different definitions of ENSO depending on the period analyzed? I think the common approach is defining ENSO events based on the NDJ or ONDJF Nino3.4 mean and then use the same years for the 2-months composites (ND or JF). I am aware this will not lead to different results, but I think it complicates the methodology more than necessary.

Lines 116-117: It is inaccurate to say that the meridional gradient is opposite for ND than JF, and just say that the centers are shifted, while as you mention in the intro, this pattern, although maybe projecting weakly on the positive NAO, is more closely related to the EA pattern, which usually leads to a strengthening of the North Atlantic jet stream (Woollings et al., 2010).

Figure 3: Maybe for consistency with Figure 2 you could also show the 5th and 95th percentile maps when using only the Icelandic low box.

Lines 143-147: Including a larger area does not decrease the uncertainty in the polar regions (>60degN), but because the uncertainty is much lower over the Azores anticyclone, this compensates the uncertainty of the Icelandic low center when both are averaged in your spatial correlation number.

Lines 161-170: I am not very convinced why the authors need to prove that EN is different than LN? Isn't this obvious? What one could test here is if the teleconnection pattern is different (asymmetry). This could be done by multiplying the SLP pattern for LN years by -1, and then perform the analysis that the authors propose here.

Lines 171-173: I do not understand where the 30/2000, 16/2000 and 11/2000 values come from? I also assume in Figure 5 you are using the 1870-2014 period as you refer to figure A1, but this is not mentioned in the text. Could you please clarify?

Line 187: I do not agree with the statement that there is no sign of asymmetry between EN and LN response. I can see a more zonally extended NAO-like dipole response for JF LN or a much stronger deepening of the Aleutian low in the North Pacific for EN. The asymmetry seems to be more evident in late winter. This is asymmetry from my understanding. Alternatively, one could do bootstrapping to analyze the asymmetry similar as in Figures 1 and 2 but using the EN+LN composite instead.

Line 209: If the purpose of this paper is uncertainty, I think the authors should try to see if the uncertainties in the nonlinearity/asymmetry are significant or not (see my previous comment). Previous studies identifying nonlinearities (e.g., Trascasa-Castro 2019; and Jimenez-Estevé and Domeisen, 2020) have shown that those are only statistically significant in model simulations where a large sample of strong ENSO events can be simulated.

Section 3.3: See general comments

Lines 259-260: As I mentioned before I wouldn't call the early-winter response a dipole response.

Lines 264-265: What is the thing that makes your view more optimistic than previous studies? This optimistic view that you claim needs a more clear justification. Most previous studies acknowledge that internal variability in the North Atlantic is large and those signals are not always as robust as one would like, but that does not mean that other papers are less optimistic. Nevertheless, optimism is a subjective vision and I think science should try to be objective