Thank you for the review of our manuscript. In this final authors' comments, we give a few immediate/early replies to selected comments. All comments will be given full attention and point-by-point replies will be provided with the revised manuscript.

Reviewer 2.

General comments.

1. 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

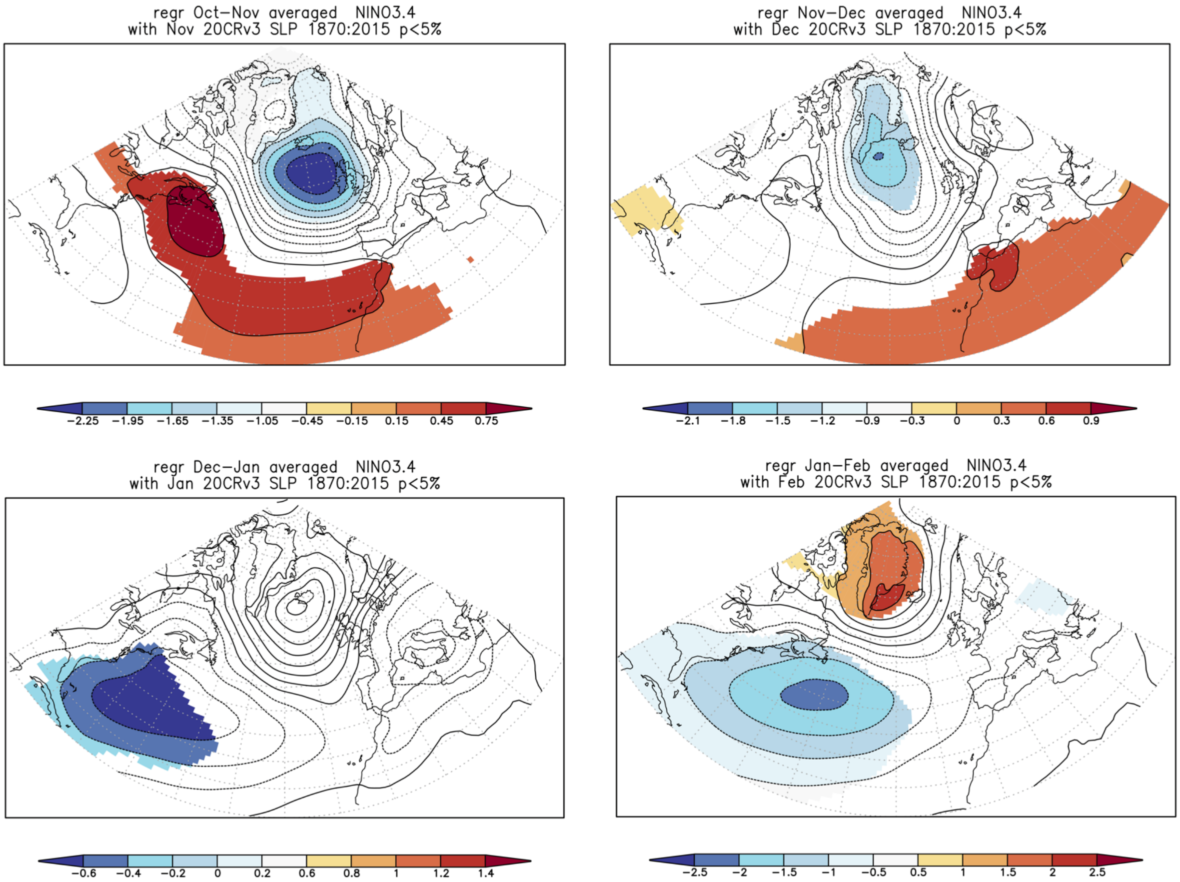
Weinberger, I., Garfinkel, C. I., White, I. P., & Oman, L. D. (2019). The salience of nonlinearities in the boreal winter response to ENSO: Arctic stratosphere and Europe. *Climate Dynamics*, *53*(7–8), 4591–4610. https://doi.org/10.1007/s00382-019-04805-1

Thank you for the additional references. We will add discussion of these papers in the appropriate place in the revised manuscript. While our focus was not on asymmetry or non-linearity in ENSO teleconnection, this is of course an important issue. We report only that we have not found asymmetry in terms of the signs of the SLP anomaly with analyses based on Nino3.4. This finding agrees with that of Deser et al (2017) which used the same general approach as ours. In a future study, we plan to perform further hypothesis tests on nonlinearities in amplitudes of the teleconnection as well as anomalies related to different ENSO types. This could be challenging because non-linearities in the signals could be smaller (because one is taking differences of differences/anomalies), and stratifying the ENSO types using reanalysis/observation reduces the sample sizes.

We would like to keep the focus and key message for current manuscript. Therefore, while we will include the additional discussion, we do not plan to address the more complex issue of nonlinearities and effects of ENSO types.

2. I agree with reviewer 1, that it would be good to show the individual monthly mean evolution of the teleconnection from Nov to March.

(Same reply is also given to Reviewer 1) We show below the regressions (not composites) of SLP in Oct, Nov, Dec, and Jan separately on HadISST Nino3.4 plotted quickly using the KNMI Climate Explorer web tool (https://climexp.knmi.nl/start.cgi).



It is seen here that there is an evolution through these months. In particular, the signs in the two major centres of action change from November/December to January/February.

In the revision, we will carry out additional analysis for these months using the method consistent with the rest of the manuscript and document the result appropriately.

3. 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.

We can carry out the same analysis and test based on this separation you suggest. It will be interesting to see how the previous finding on extreme ENSO teleconnection stand under the same bootstrap tests. For the case of strong/extreme events, we have some concerns on the potentially much smaller sample, and therefore the robustness of the conclusion that can be made. However, we will carry out the analyses and report the results in the revised manuscript.

4. 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.

Indeed, we carried out the analyses/tests in sect 3.3 to check what these different methods produce. The result is somewhat "boring" in the end, but we did not know this beforehand. In fact, the "boring-ness" could be considered the important part of the result. The additional tests are not normally employed by the wider climate teleconnection research community, and they might produce different results for other regions and teleconnection drivers.

However, we understand your point about the figures. We think it could work to use only selected panels in the main text body and move the full sets of panels to the appendix.