

Weather Clim. Dynam. Discuss., author comment AC1
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Reply on RC1

Martin Wegmann et al.

Author comment on "Impact of Eurasian autumn snow on the winter North Atlantic Oscillation in seasonal forecasts of the 20th century" by Martin Wegmann et al., Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2021-52-AC1>, 2021

This paper uses a large ensemble of seasonal forecasts to address open questions about the potential link between Autumnal Eurasian snow cover and the winter North Atlantic Oscillation (NAO).

I find this to be a convincing, carefully thought out, and well written paper, with important conclusions. I would recommend publication subject to addressing the two minor comments below.

MINOR COMMENTS

1)

If the mechanism of Autumn Eurasian snow influencing the winter NAO were not real, I'm not sure I'd suggest that the signal seen in reanalysis was "random co-variability between snow cover and DJF NAO", as is suggested in this paper a few times. Much more likely, I'd have thought, is that the same external driver is influencing both the Autumnal Eurasian snow cover and the winter NAO.

For example Arctic sea ice can influence Eurasian snow cover

<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019JD030339>

and also the NAO

<https://www.nature.com/articles/s41598-017-00353-y>

Tropical rainfall has also been shown to strongly influence the extratropics on seasonal time scales

<https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/qj.2910>

Can you demonstrate that both sea ice and tropical rainfall are near-enough identical in your high snow and low snow composites? If you can then that would be a good result to report too. It would arguably rule out the above suggestion of either sea ice or tropical rainfall influencing both snow and the NAO, and would strengthen your conclusion that it is snow influencing the NAO.

If, however, you find that sea ice or tropical rainfall are different in your high and low snow composites (due, presumably, to different land surface conditions outside of Eurasia), I think you need to demonstrate somehow that neither sea ice nor tropical rainfall plays an important role here.

REPLY: Thank you very much for your comment. In our experiment, sea ice and SSTs are not dynamical. As such, sea ice is the same for each of the EXP members and is subtracted out as forcing. We checked the tropical precipitation response in the EXP members and found no significant anomalies for November and only sparse significant precipitation spots in December. As such we rule out the impact of tropical rainfall on the winter NAO in our experiments and added that information to Line 425–430. We changed the wording of “random co-variability” throughout the paper and added the information about sea ice and tropical precipitation to the discussion section.

2)

A caveat of previous modelling experiments that prescribe snowpack anomalies, is that the snow anomalies required to produce a significant NAO signal are unrealistically huge. I think you do much better with your experiments here, and indeed think that this is a key strength of the paper (perhaps you could emphasize this point). Therefore, I'd just like it clarified that the anomalies in your high and low snow composites are indeed realistic.

You state that "snow removal to the west of Russia appears in regions with no to rare snow cover". Does that leave you with negative snow anomalies in your low snow composites, which would be unrealistic, or do you set the snow to have a minimum value of zero? I think this needs to be clarified. [Admittedly, it shouldn't impact your conclusions since you show in section 3e that it is the negative dipole (with additional snow to the west of Russia) that leads to most of the response seen.]

Also in the last sentence of the paper you talk about "allowing more extreme snow forcing" but I think you should clarify that this forcing needs to remain realistic (otherwise any conclusions would not add to previous modelling experiments).

REPLY: Thank you very much for your comment. We adjusted the “unrealistic” point throughout the document. Snow depth can never go below 0 in our composites and experiments. Our wording was not precise enough. We subsample members with low snow depth in the west and compare them with members with high snow in the west. We made sure that this wording is improved throughout the document.

In previous experiments with the ECMWF seasonal forecast model (Orsolini et al., 2013), the snow anomalies were not unrealistic but rather taken from different times in the seasonal cycle (e.g, October 1st forecast could have been initialized using December 1st snow for some members). Here, the snow initialization is more consistent as it is the same date for all members (but corresponding to different years)

VERY MINOR COMMENTS AND TYPOGRAPHICAL ERRORS

Line 110: "latter multi-model study". It is not clear to me which study you are referring to here.

REPLY: Fixed

Line 134: "We use the Centre" should be "We use the European Centre"

REPLY: Fixed

Line 142: "as predictor" should be "as a predictor"

REPLY: Fixed

Line 153: "identically" should be "identical"

REPLY: Fixed

Line 332: Delete "(?)"

REPLY: Fixed

Line 383: "positive the" should be "positive"

REPLY: Fixed

Figure 2 caption: "As example" should be "As an example"

REPLY: Fixed

Figure 4 caption: "individual member" should be "individual members"

REPLY: Fixed