

Weather Clim. Dynam. Discuss., referee comment RC1
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Comment on wcd-2021-52

Anonymous Referee #1

Referee 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-RC1>, 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.

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).

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.

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

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

Line 153: "identically" should be "identical"

Line 332: Delete "(?)"

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

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

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