

## Final author comments

### Anonymous Referee #3

We thank the reviewer for the useful recommendations.

#### Major Comments

- That the Eurasian snow cover in autumn leads a negative phase of the (N)AO in winter was originally been proposed by Cohen and co-authors (cited). The idea that the snow cover exerts a (weak) feedback that reinforces a pre-existing negative (N)AO phase during winter, was proposed in Orsolini et al (2015), based on a case study contrasting a pair of coupled forecast ensembles where the snow-atmosphere feedback could be switched off (or at least scrambled at the initial time). This idea was further explored by Garfinkel et al (2020), using a suite of coupled (S2S) forecast models, who showed some transient feedback from snow cover onto the atmospheric circulation in the models with a better stratosphere.

It seems to me that one of the main findings of the current study is along the same lines, albeit using a different set of atmosphere-only ensemble of simulations. Namely, that a negative AO is not forced by snow as it arises from internal variability but, yet, is re-inforced or prolonged by the snow feedback (Fig 13), especially in January: is this correct interpretation? This could be stressed more clearly in the Abstract, and the appropriate references included.

Thank you for this comment. We agree that the abstract should be modified so that it better reflects the results of the paper. This comment is in the same line as the first comment of reviewer #2.

Orsolini (2016) is indeed relevant. The results of Garfinkel et al (2020) will be compared and discussed in the conclusion.

- In their observational case study of the 2018 SSW, Lü Z. et al highlighted the potential role of the Siberian snow cover fluctuations in forcing planetary waves into the stratosphere, with the pulses of upward wave propagation preceded by snow increases in January-February by about a week (see their Figs 10-12), which modulate land-sea longitudinal temperature contrast over the Eurasian continent. Although such a lag is not a proof of causality, I wonder if this is consistent with the lagged effect on surface temperature/SLP highlighted here (Figs 13,15). A map of geopotential height in the stratosphere might be useful to complement Fig 15.

The stationary wave pattern and the geopotential height in the stratosphere related to the snow cover will be further assessed. We will also consider adding an analysis of the wave activity flux. The results will be compared to Lü et al. (2020) and discussed in the paper.

- Earlier studies of the snow-NAO linkage argued that the observed snow cover variability in the fall is underestimated by climate models. Here, model ensemble means are used, which damp the variability, but it would be of interest to document of actual range of snow variability in each model using all members, across the snow season.

The seasonal variability of the snow cover extent and mass will be investigated by their standard deviation for each month. The results will be discussed in the revision. We will consider adding the figure in an appendix, keeping in mind that the paper is already quite long.

The role of the sea ice change on the continental-scale snow cover trend indeed appears small. Yet, it is interesting that there appears to be a regional effect in Western Russia during November (Fig 5) where there is some decrease downstream and south of the Barents-Kara seas. Could the authors comment on that?

We agree that this can be commented. This might be related to the important sea ice decline observed over the Barents-Kara seas. A sentence will be added in the revised text.

- I believe that the CanCISE snow product is a multi-instrument/model product which comes with a measure of uncertainty. Would it be of interest to incorporate that “observational” uncertainty in some of the Figures (e.g. Figs 4)?

The CanCISE snow product has a spread, which is the difference between the maximum and minimum snow fraction estimated in the 5 products averaged. We will add such a spread if it does not degrade too much the visibility of the time series in Fig. 4.

- The role of spring snow cover over the Tibetan Plateau Mongolian Plateau and its impact on the monsoons is alluded to on several occasions, with a reference to Barnett et al (1989). There has been a large body of literature on this topic since 1989, which is not mentioned. Since the paper focuses on continental Eurasia and North American snow cover in autumn and winter, and this precipitation and snow biases in models and re-analyses over this Tibet region are well documented elsewhere, the authors could skip this issue and keep the paper more focused.

We agree that the text related to the Tibetan/Mongolian Plateau snow and its impact on monsoon is maybe out of the scope. The related sentences will be removed.

- Garfinkel C.I, C. Schwartz, I. White and J. Rao (2020), Predictability of the early winter Arctic Oscillation from autumn Eurasian snowcover in subseasonal forecast models, *Clim. Dyn.*, 5:961-974
- Orsolini, Y.J., Senan, R., Vitart, F., Weisheimer, A., Balsamo, G., Doblas-Reyes F., Influence of the Eurasian snow on the negative North Atlantic Oscillation in subseasonal forecasts of the cold winter 2009/10, *Clim. Dyn.*, DOI: 10.1007/s00382-015-2903-8 (2015)
- Lü, Z., Li, F., Orsolini, Y. J., Gao, Y., & He, S. (2020). Understanding of European Cold Extremes, Sudden Stratospheric Warming, and Siberian Snow Accumulation in the Winter of 2017/18, *Journal of Climate*, 33(2), 527-545.

Minor comment:

- I find it confusing that, in Fig 4, the anomaly (ALL minus SIC, hence a small quantity), representing the potential role of the sea ice, is correlated with the full-field snow from ERA5-land. Wouldn't it be clearer to show the relation to the snow from ERA5-land for each simulation ensemble separately, next to one another?

We will consider modifying Fig. 4 showing ALL and NoSIC as two separate lines. We will modify Fig. 4 if the modifications do not degrade too much the visibility of the time series in Fig. 4.

- The inset in Fig 4 should specifically mention ERA5-land, not be confused with ERA5 re-analyses, which assimilate snow observations.

Thank you. Text and figure caption will be modified to only mention ERA5-Land.

**Wording:**

L23: The first sentence of the Abstract is a bit unclear.

The first sentence will be reformulated.

L42: which aspect of “ecosystems”: management? Understanding the inner working of ecosystems?

The role of snow in the understanding the ecosystems will be mentioned in a separate sentence.