Response to reviews

General comments

We thank all reviewers for their recommendations.

The revised manuscript now includes five supplementary figures which are provided in a supplementary material. Some additional text was added to discuss the new results.

In addition to the specific response to reviewers, we would like to acknowledge:

- the text was revised to provide details regarding the calculation of the snow cover from the daily snow water equivalent of CanSISE,

- we found an error in the previous manuscript regarding the snow mass trend from CanSISE illustrated in panel (a) of Fig. 5 (grey cross and circles). In previous version, the trend in snow cover was reported instead of the snow mass trend. This was corrected in the revised manuscript,

- the ensemble mean from ALL and NoSICvar (new name of NoSIC) was removed before calculation of the regression in Fig. 15a. This modifies slightly the Fig. 15a in the revised manuscript,

- some figures captions were completed and corrected. In particular, in the revised manuscript Figs. 6a-c is described as ''Variance fraction (in %) of the interannual snow cover anomalies'' instead of '' Variance fraction (in %) of the interannual snow water equivalent anomalies'',

- some additional minor changes to further improve the text (see track-change version of the manuscript).

Anonymous Referee #1

We thank the reviewer for the useful recommendations.

1) My first comment is about the observed trend in Eurasia/North America snow cover in November. The CanSISE observations you use stop in 2010, but it is notorious that snow cover in fall has had a tendency to increase since 2010, roughly. See the timeserie of snow cover from the Rutgers University Global Snow Lab for November, that shows the Eurasia and North America snow cover extent for the 1966-2022 period. There is a clear positive trend in both domains. This is for 1966-2022, but even over the 1979-2010 period that you use in your study, I do not see a decreasing trend as shown in your Fig. 5. Could you elaborate on this discrepancy? Is this due to uncertainties in observations, periods, both?

Link to the Rutgers Univ. snow extent timeseries:

https://climate.rutgers.edu/snowcover/chart_anom.php?ui_set=1&ui_region=nhland&ui_month=112)

We also find this discrepancy between the NOAA-CDR snow cover extent and the snow extent found from CanSISE or other datasets. We interpret it as an important uncertainty in observation. Brown and Derksen (2013) already pointed out this discrepancy between NOAA-CDR and other datasets in October. The singular trend of NOAA-CDR was also described in October and November by Mudryck et al. (2017), and reported in the IPCC 6th assessment report (Chapter 2 and 9; Gulev et al., 2021; Fox Kemper et al., 2021). To the best of our knowledge, we do not find any explanation for this discrepancy.

We included more discussion on the observational trend in the revised paper. In particular, the spread in the observational dataset is now shown in Figs. S2 and S3.

L335-341: ‘’ A comparison of the trends obtained in other observational datasets (Figs. S2 and S3) shows a large spread in trend estimates, with increasing snow extent in fall and early winter in the NOAA-CDR observations, while ERA5-Land, MERRA2 and CanSISE show a decreasing snow extent, as found previously (Brown and Derksen, 2013; Mudryck et al., 2017). The reason for this difference between the NOAA-CDR and multi-observation products is unknown (Mudryck et al., 2017). This suggests that observational uncertainties are important, especially in fall, as reported by Fox-Kemper et al. (2021).”

L648-650: ‘’ Although models simulate different mean states for snow cover, the observations show an important spread as found by Mudryck et al. (2017), and both simulated trend and mean state are within the observational spread.’’

2) In link to my previous comment, section 3.4 shows there are large differences between the observed datasets in November. This is something that could be discussed further. Which dataset is more reliable in fall? NOAA-CDR, because it consists of direct satellite measurement of snow cover?

See response to comment 1). The spread obtained for the trend calculated in the four observed dataset is now shown (Figs. S2 and S3) and discussed L335-341 and L648-650.

The spread provided by CanSISE is also indicated in the revised version of Fig. 1.

We do not know which dataset is more reliable. In IPCC AR6 (Fox-Kemper et al., 2021), it is noted that ‘’The positive trends from the NOAA CDR are also inconsistent with later autumn snow-on dates since 1980 (–0.6 to –1.4 days per decade), based on historical surface observations, model-derived analyses and independent satellite datasets (updated from Derksen et al., 2017). The SCE trend sensitivity to surface temperature forcing in the NOAA CDR is anomalous compared to other datasets during October and November (Mudryk et al., 2017). There is therefore *medium confidence* that the NH SCE trend for the 1981–2016 period was also negative during these two months ''.

This suggests that many snow datasets are needed to sample some of the observational uncertainty. This is why we include the snow cover and snow depth from four different datasets in this manuscript.

3) The most interesting result of the study, other than highlighting the limited impact of SIC on snow cover variability, is the potential feedback of January snow cover anomalies on polar vortex warming events (section 4.3). You find that snow cover $EOF1_{int}$ is preceded then followed by a significant weakening of the polar vortex, and you hypothesize that snow may act as a feedback in increasing the persistence, possibly amplitude, of the anomaly in the stratosphere. This is interesting but only speculation since no analyses are shown to demonstrate it. I think this is where the paper can be improved, and I have a few suggestions. In the analyses of LMDZOR6, you could select winters that exhibit persistent polar vortex weakening (similar to Fig. 15), and differentiate these events between those that also exhibit high snow cover $EOF1_{int}$ anomalies, and those that do not (composite analyses). This would be a way to verify whether snow cover $EOF1_{int}$ anomalies are indeed necessary to enhance the persistence and amplitude of the polar vortex warming. If possible, it would also be nice to see how snow cover $EOF1_{int}$ affects the stationary wave structure over Eurasia, and wave activity, that could potentially cause a higher persistence of polar warming anomalies. This section needs improvement to be more convincing.

As suggested, the wave activity flux is calculated using daily outputs from LMDZOR6. The regression on the PC_{Int} is shown as Fig. S5. The atmospheric circulation anomaly is also investigated with regressions of the 500-hPa and 50-hPa geopotential height (Fig. S4).

L599-604: 'The 50-hPa geopotential height anomalies associated to the snow cover show that the polar vortex weakening is widespread and affects the whole polar cap (Fig. S4). The wave activity flux was calculated to investigate the propagation of the stationary waves (see Section 2.2). The regression of the wave activity flux on $PC1_{Int}$ shows an amplified upward component of the wave activity flux over eastern Eurasia and the western North Pacific (Fig. S5).''

The concomitant role of the polar vortex for the snow cover influence is now described in the new Fig. 15b. It is interpreted as the respective role of the polar vortex and snow cover forcing. We also complemented this analysis with a composite analysis following the recommendations in Fig. S6.

L610-620: '' To do so, we consider in the same model (e.g. LMDZOR6) an index of the stratospheric polar vortex, defined as the standardized January polar cap (north of 60°N) temperature anomalies at 50-hPa, hereinafter called PCT_{50}. We also define an February AO index with the first PC of the SLP north of 20°N. The ensemble mean of that model is then removed from all time series. The correlation between PCT_{50} and $PC1_{Int}$ is weak (0.09) but significant. Both the January PCT_{50} and $PC1_{Int}$ are significantly correlated to the February AO, but the variance of the AO that is explained by PCT_{50} is larger than the one associated with the snow cover index, $PC1_{Int}$ (Fig. 15b). Both polar vortex and snow cover remain significantly related to the AO when using a multi-variate regression (not shown). A similar analysis using composites confirms this result and shows that the relationships between both indices and the AO are mostly linear (Fig. S6). Therefore, the snow cover has a significant influence on the AO, but this influence is smaller than the one associated with the polar vortex. ''

The summary at the end of section 4.2 was also improved.

L 634-642 : ‘’ In summary, the snow cover and the polar vortex have a common driver, namely Ural tropospheric blocking. The snow cover and the polar vortex also have a similar influence on the AO one month later. Therefore, the lag relationship between January snow cover and the troposphere in February or March must be interpreted with caution, as causality cannot be firmly established. However, the polar vortex anomalies in Fig. 15 show a clear amplification in February, following the January snow cover anomalies. Although the snow cover influence is smaller than the one of the polar vortex, it remains significant when removing the concomitant effect of the polar vortex with a multivariate regression using snow cover and polar vortex indices. This suggests that snow cover anomalies act as a positive feedback for the AO variability, as they amplify the combined negative AO and Ural blocking pattern. ’’

The abstract and discussion and conclusion were also modified according to these new analyses.

References

Brown, R. D., & Derksen, C. (2013). Is Eurasian October snow cover extent increasing?. *Environmental Research Letters*, *8*(2), 024006. https://doi.org/10.1088/1748-9326/8/2/024006

Fox-Kemper, B., H.T. Hewitt, C. Xiao, G. Aðalgeirsdóttir, S.S. Drijfhout, T.L. Edwards, N.R. Golledge, M. Hemer, R.E. Kopp, G. Krinner, A. Mix, D. Notz, S. Nowicki, I.S. Nurhati, L. Ruiz, J.-B. Sallée, A.B.A. Slangen, and Y. Yu, 2021: Ocean, Cryosphere and Sea Level Change. In *Climate Change 2021: The Physical Science Basis. Contribution of Working Group I to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change*. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 1211–1362, https://doi.rog/10.1017/9781009157896.011 , 2021

Gulev, S.K., P.W. Thorne, J. Ahn, F.J. Dentener, C.M. Domingues, S. Gerland, D. Gong, D.S. Kaufman, H.C. Nnamchi, J. Quaas, J.A. Rivera, S. Sathyendranath, S.L. Smith, B. Trewin, K. von Schuckmann, and R.S. Vose: Changing State of the Climate System. In *Climate Change 2021: The Physical Science Basis. Contribution of Working Group I to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change*. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, pp. 287–422, doi:10.1017/9781009157896.004, 2021.

Mudryk, L. R., Kushner, P. J., Derksen, C., and Thackeray, C. (2017), Snow cover response to temperature in observational and climate model ensembles, *Geophys. Res. Lett.*, 44, 919– 926, https://doi.org/10.1002/2016GL071789.

Anonymous Referee #2

We thank the reviewer for his useful recommendations.

Abstract: Please review the entirety of the abstract to make sure it's consistent with the results in the paper and as stated in the conclusions. E.g. It’s stated here that snow cover/PV anomalies act to *generate* the AO, which isn't consistent with the paper results or interpretations discussed within. The statement in your conclusions, that Ural blocking, which itself projects onto the AO, may drive important elements of both snow cover and PV variability, is consistent with my interpretation of your results.

We agree. The abstract was rewritten to be consistent with the main conclusion.

L31-41:’’[…] On the other hand, the first mode of Eurasian snow cover variability in January, with more extended snow over western Eurasia, is found to precede by one month an atmospheric circulation pattern similar to a negative Arctic Oscillation (AO). A decomposition of the variability in the model simulations shows that this relationship is mainly due to internal climate variability. Detailed outputs from one of the models indicate that the Western Eurasia snow cover anomalies are preceded by a negative AO phase accompanied by a Ural blocking pattern and a stratospheric polar vortex weakening. The link between the AO and the snow cover variability is strongly related to the concomitant role of the stratospheric polar vortex, with the Eurasian snow cover acting as a positive feedback for the AO variability in winter. No robust influence of the SIC variability is found, as the sea ice loss in these simulations only drives an insignificant fraction of the snow cover anomalies, with few agreements among models.’’

L23: You analyze more than just the role of SST anomalies.

L23 : we replaced ‘’ The role of surface ocean anomalies for the […] snow cover ‘’ by ‘’The main drivers of the […] snow cover ‘’.

L30: I agree that topical/North Pacific SSTs are a dominant influence over the 1981-2014 period, but given the strong change in IPO signals over the period the statement may not apply more generally. Please restate to include the period of analysis.

We added at L29 ‘’during this period.’’ To be more specific.

L37: rephrase to be more specific. E.g. “snow cover *variability* across western Eurasia and an important contribution to polar vortex *variability* are both generated by Ural blocking”

This part of the abstract was rewritten.

L37-39: ‘’[…] The link between the AO and the snow cover variability is strongly related to the concomitant role of the stratospheric polar vortex, with the Eurasian snow cover acting as a positive feedback for the AO variability in winter.’’

L154: A snow density of 330 kg/m^3 is unreasonably high for a spatial and seasonal mean. A seasonal mean of 220-240 kg/m^3 would be more appropriate and would correct some of the over-estimated snow mass for the LMDZ6 and CMCC models shown in your Figure 1 and mentioned at line 249. Sturm et al (https://doi.org/10.1175/2010JHM1202.1) is the classic reference and the "test" data described there (Fig 1-2, Table 3) is consistent with a more recent analysis (https://doi.org/10.5194/tc-2022-227; in fact, Fig 3 of this recent analysis plots the spatially averaged snow density as a function of day of the year).

We thank the reviewer for this useful comment. A snow density of 240 km/m^3 is now used to convert snow depth into snow water equivalent. The reference Sturm et al. (2010) is given in the revised manuscript. This corrects the estimated snow mass in LMDZ6 and CMCC, which remains overestimated and larger than the observational spread (see Fig. 1). The Figs. 3, 4 and 5 were corrected accordingly, but the main conclusions are hardly modified.

L141: This is a small point but changing the name of the second ensemble to ‘NoSICvar’ in the text and plots would read more accurately to me.

NoSIC was replaced by NoSICvar in all plots and text.

L155: The monthly binary fields resulting from this procedure would produce reasonable time series, but I don't know that the EOF patterns from these 4 models would have the same spatial variability as those based on monthly mean snow cover fraction (which would average over sub-monthly changes in the snow line). It might not matter in this analysis if the important part of the signal is just snow vs no-snow, but please check that the EOF_BC and EOF_SIC patterns (used in Fig 8) for models 1-4 (which use binary fields calculated from SWE output) are the same as those from models 5-8 (which use model-derived SCF).

The EOF_BC and EOF_SIC are built from the multi-model ensemble mean (MMM), averaging data from models 1_4 (binary fields) and models 5-8 (model-derived snow cover). Therefore, the EOF is not calculated using binary data. Nevertheless, the EOF_{Int} patterns are built using each model separately. This comment applies for Fig. 11. We illustrate in Figs. R1, R2 and R3, the first EOF of the snow cover in November, January and April in the 8 models separately. The patterns given by models 1-4 are similar to that of models 5-8, but they have perhaps more intense loadings. We believe the main conclusion of the manuscript should not be affected by this difference.

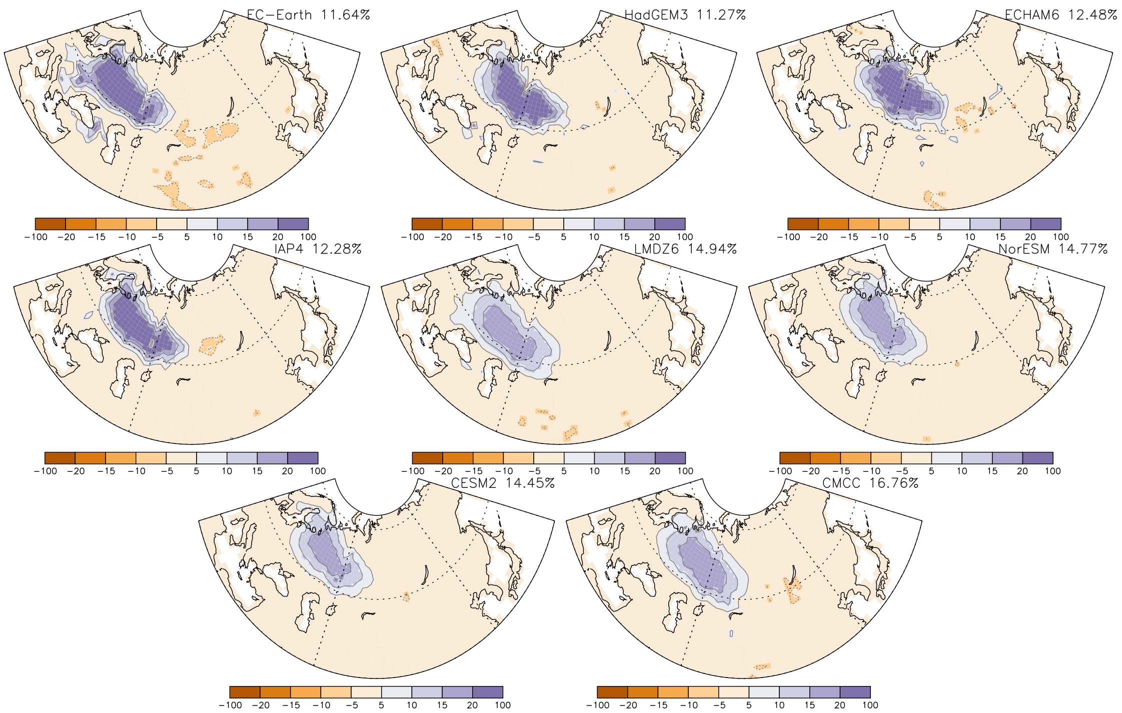

Fig. R1. First EOF_{Int} of the November Eurasian snow cover, in %, associated with internal atmospheric variability in the simulations ALL and NoSICvar. The variance explained is given on top.

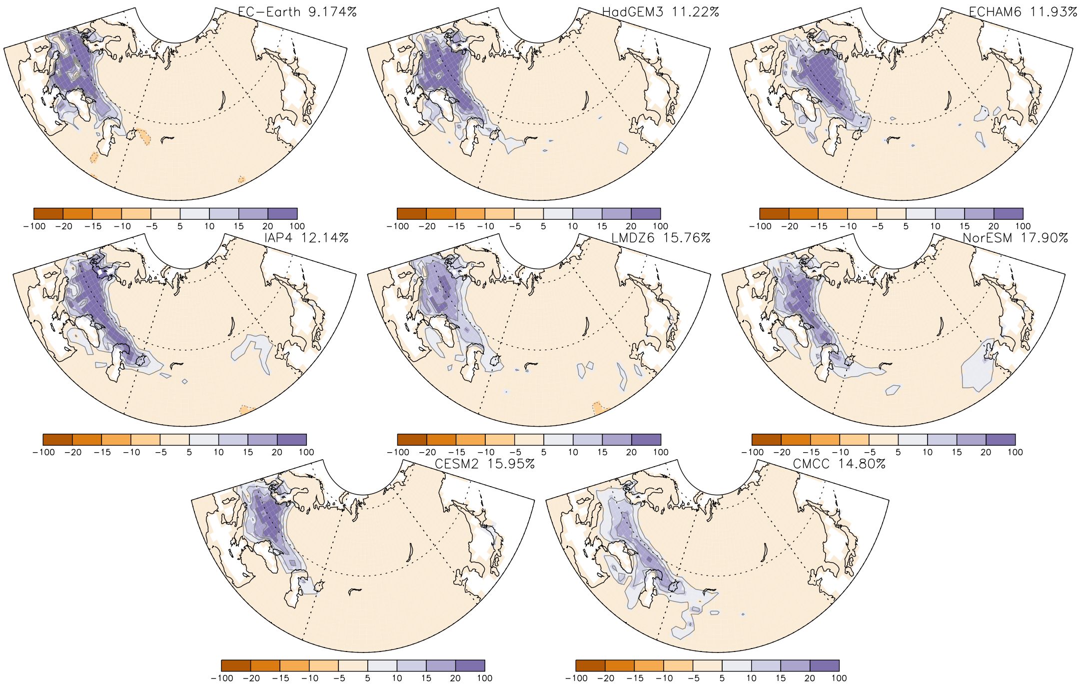

Fig. R2. Same as Fig. R1, but for January.

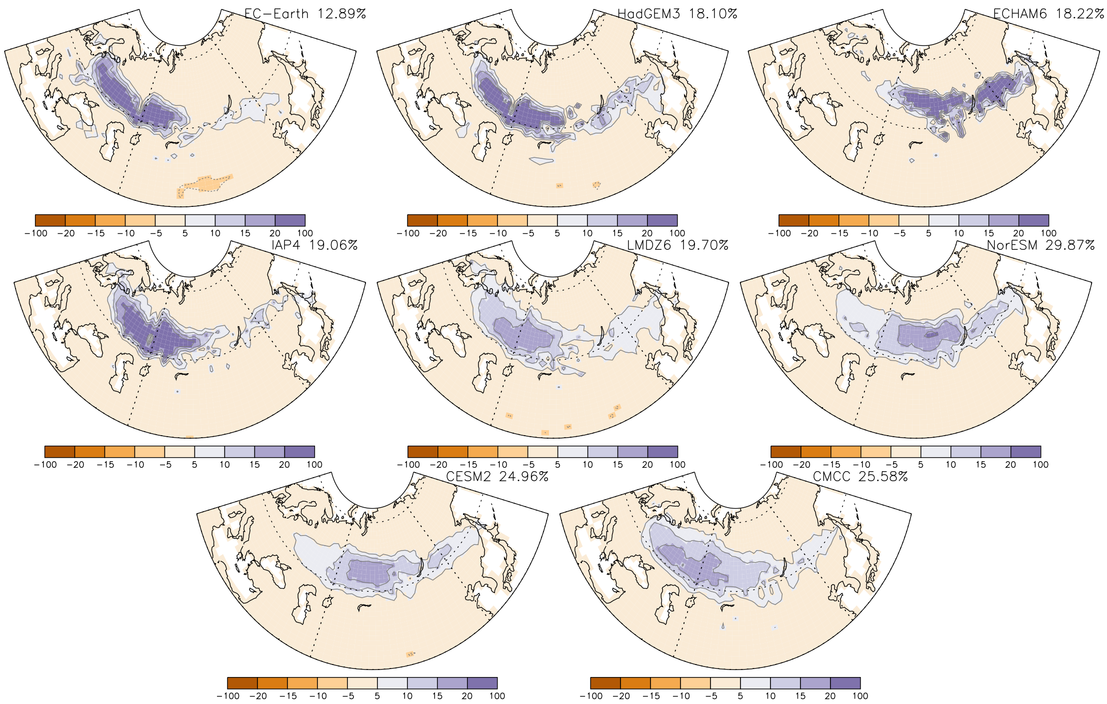

Fig. R3. Same as Fig. R1, but for April.

L198: Do you use a convention to assign positive/negative values to the EOF patterns? For the patterns with multiple centers of action it's not always clear how the EOFs relate to one another among the different plots. E.g. Fig 7d and Fig 10e.

The convention to assign the sign of the EOF pattern is now provided in the Method section.

L231-232:'' The sign convention is that a positive PC corresponds to an EOF with positive loading over eastern Europe (20°E-70°E 55°N-70°N).''

To ease the comparison between Fig. 7 and Figs. 10 or 11, we modified the projection in Figs. 10 and 11, which are now using a stereographic North projection. A dashed grid every 60° for longitudes and at 50°N and 70°N is provided in all plots (Figs. 2, 3, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14 and 16) so that the reader can establish the links between the patterns.

L260: "simulate more snow cover… and too little snow cover over…"

Done

L268-269: I think you meant to write that ECHAM6 and IAP4.1 both under-estimate snow water equivalent over Eurasia rather than overestimate.

Thank you. This was corrected.

L290-292: '' The bias is negative in ECHAM6 and IAP4.1, with an underestimation of the SWE over the East European Plain and North-Eastern Siberia, respectively. ''

L290: Please confirm you are using snow cover output from ERA5-Land, not ERA5 and correct in the text and figure captions.

The text and figure caption (Fig. 1 and 4) were corrected to only mention ERA5-Land.

Figure 5: The figure caption which specifies snow cover vs snow mass does not reflect the figure labels. Please confirm the labels are placed on the correct plot and correct the description if necessary. Also please change the plot so that the small grey circles in Jan and Apr are plotted on top of the other symbols and can be seen. The one in Apr is hard to see and I don't see it at all in Jan assuming it is underneath one of the other symbols. Same for the dark blue cross in NDJFM for plot c.

Thank you. The revised Figs. 5a-d uses transparency to ensure that all superposed symbols are visible.

Figure 6: This is the first time they appear and it's not clear what distinguishes the models marked with the * symbol (specified on Figure 11).

Thank you for this comment. We added a sentence to the legend of Fig. 6 :

''The symbol star indicates when 30 members are available for both ALL and NoSICvar.''

Figure 10: Please adjust the lower latitude limit of Figures 10b,c,f,g,j,k to match those in Figures 7,8,12,13 etc.

The lower latitude limit of Fig. 10b,c,f,g,j,k was modified, as recommended.

L 459: The similarity of the January snow cover variability patterns over Eurasia between Figs 7 and 10 suggests the NAO is the dominant source of variability over Eurasia in January rather than external forcings (as opposed to Nov and Apr where snow loss trends from external forcings are an important source of variability and alter the observed EOF1 patterns). This might be worth pointing out and commenting on here and in the discussion since it's consistent with the strong influence of the NAO analyzed in the model simulations during January.

A sentence was added as recommended:

L491-493: ''The similarity in the EOF1 pattern obtained in January in models and observations (Figs. 7d and 10e) suggests that internal variability is dominant during that month, as opposed to November and April where forcings are important.''

L480: I suggest being more nuanced about the maximum loading locations: in Nov it is in western Russia, in January it shifts towards eastern Europe, in Apr it shifts back eastwards to central Siberia. The dipoles of the EOF2 patterns seem to be positioned on the northwestern and southeastern ends of these EOF1 patterns.

The description of Fig. 11 was modified accordingly.

L515-519: 'The maximum loading of the monopole is located in Western Eurasia in November. It shifts to Eastern Europe in January, and moves eastward toward Central Siberia in April. The second mode is a dipole, with anomalies positioned on the northwestern and southeastern ends of the EOF1 patterns; its explained variance ranges between 6% and 18%. ''

L500: A little more summary/guidance for the reader would be helpful here. Maybe something like "The comparison between Fig. 10 for observations and Figs. 11-12 for models suggests that the models reproduce fairly well the main mode of variability found in observations. The NAO is the dominant mode of variability during January in both models and observations. During November and April, the dominant mode of variability found in the observations is a blocking pattern with a trough over the Ural region. This pattern also occurs in the model simulations but with less associated variance (it is reproduced in the EOF2 patterns rather than the EOF1 patterns). However the analysis of observations is based on…"

A summary at the end of section 4.1 was added for the reader, as recommended.

L535-544: 'The comparison between Fig. 10 for observations and Figs. 11-12 for models suggests that the models reproduce fairly well the main mode of the observed variability. In both cases, the NAO is the dominant mode of variability during January. In January, the analysis of the second EOF from detrended observations (not shown) also shows that it is associated with patterns similar with that found in models (Figs. 11e and 12e). During November and April, the observed dominant mode of variability is a blocking pattern with a trough over the Ural region. This pattern also occurs in the models, but with less variance, as it is reproduced by EOF2 rather than EOF1. However, there is an important spread among observations in November and April, and the analysis of observation is based on detrended time series that still include the contribution from interannual SST variability and short-term fluctuations in the external forcing. ''

L500: I know the paper already includes a lot of analysis, but I presume the projections of SLP and temperature onto the observed PC2 time series either isn't very interesting or doesn't relate to the other modes of variability seen in the models?

The SLP and temperature associated with the observed PC2 are provided in Fig. R4. The second EOF in the observations shows patterns with loading at a location different from the models (compare Fig. R4 with Figs. 11def). However, in January and April, some similarities are noticed between the EOF2 patterns obtained from observations and models. In observations and models, the maximum loading is located over south-western Eurasia in January, and south-eastern Siberia in April. The observed SLP anomalies at lag 0 associated with $PC_{Int}2$ (compare Fig. 12def to second column of Fig. R4) show patterns with some similarities between observations and models. In November, a large anticyclone is located over Southern Scandinavia, which is similar to the model EOF2 but with the opposite sign. In January, there are negative SLP anomalies over western Europe and positive anomalies over the Arctic. In April, a depression is located over northern Russia. The EOF2 pattern in January also shows some persistence, with a pattern somehow similar obtained at lag 0 and 1 in observation.

The similarity in the EOF2 pattern for observations and models is briefly mentioned in the text.

L537-539 : '' In January, the analysis of the second EOF from detrended observations (not shown) also shows that it is associated with patterns similar with that found in models (Figs. 11e and 12e).''

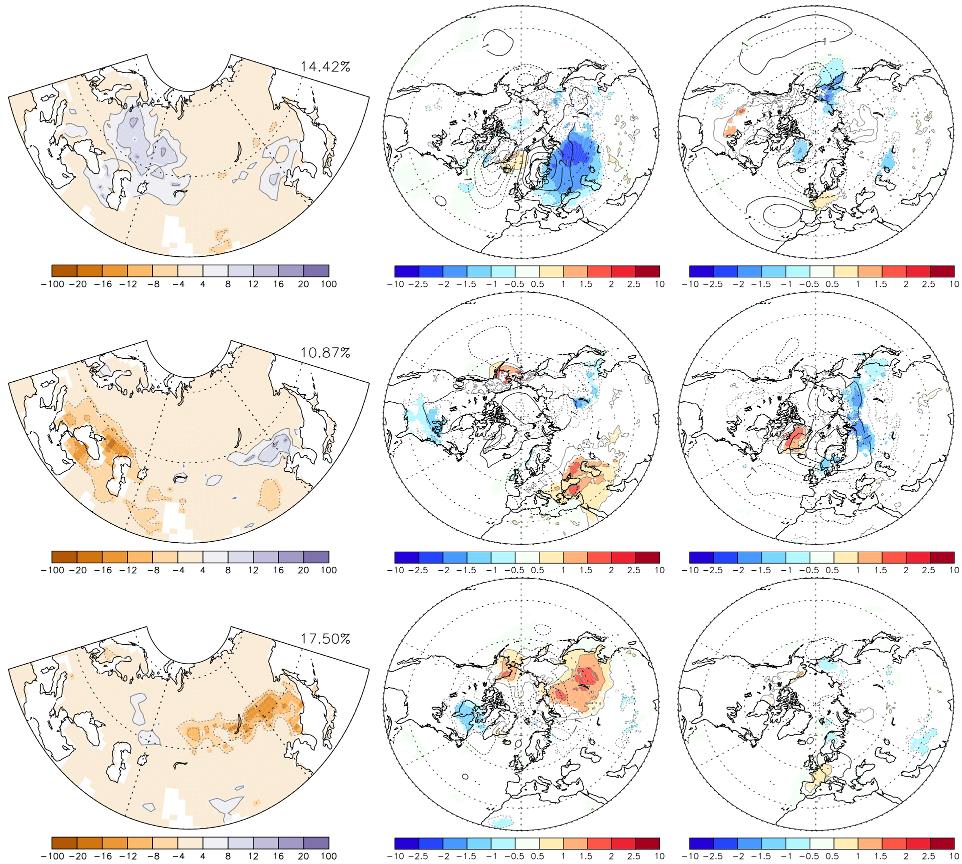

Fig. R4. Same as Figs. 10a-c, 10e-g and 10i-k, but for the second EOF of the detrended Eurasian snow cover in ERA5-Land. The first column shows the second EOF, with the variance explained. The second column shows the (color) 2m air temperature and (contour) sea level pressure regression onto the $PC2_{Int}$ time series, at lag 0. The third column is the same as second column but at lag = 1, the $PC2_{Int}$ leading by one month. The first, second and third line is for November, January and April $PC2_{Int}$, respectively.

L509: This claim that the SLP pattern in Fig 10c resembles the AO is *not* convincing. Nor do the models suggest anything of this sort in November in Fig 13a. I would accept that as for January the Nov-lagged pattern shares some similarities with the original Nov pattern and hence has somewhat persisted into the following month.

We agree. We do not see an AO pattern. The description was modified.

L547-550 : '' In observations, the November EOF1 is followed by negative SLP anomalies over the polar cap and some weak positive anomalies over Western Europe and the Bering Sea. This SLP pattern shares some similarities with that found at no lag, but with a smaller amplitude.''

L539: Should this read "The comparison of Fig 10 with Figures 12 and 13…."?

Yes. This is corrected.

L579: ''The comparison of Fig. 10 with Figs. 12 and 13''

L572: Fig 16b?

Yes. This is corrected.

L622: '' The same regression using SLP (Fig. 16b,e) ''

L578: Fig 16a,b?

We believe that the reference to Fig. 16b,e and Fig. 16c,f were correct and we did not modify the original text.

L630-632: '' The regressions obtained using PCT_{50} (Fig. 16b,e) resemble the one obtained with $PC1_{Int}$ (Fig. 16c,f), even if the later anomalies are shifted toward the North Atlantic. ''

L591: In the abstract, consider adding your conclusion that in uncoupled models sea ice loss drives a detectable but insignificant fraction of snow cover anomalies. I know it's effectively a null result, but I think it's still important to highlight.

A last sentence was added to the abstract to emphasize this result:

L39-41: ''No robust influence of the SIC variability is found, as the sea ice loss in these simulations only drives an insignificant fraction of the snow cover anomalies, with few agreements among models. ''

L592: In the results you also state sea ice variability drives a small and insignificant fraction of snow mass anomalies, but don't explicitly show the results.

We do not understand the comment, as line 592 (in previous manuscript) discusses the snow cover variability (and not the snow mass). We looked for misleading references to snow mass elsewhere in the conclusion, but we did not find any.

We did correct an error in the legend of Figs. 6a-c which show the variance fraction of the snow cover (and not of the snow water equivalent) associated to SST, external forcings and sea ice concentration.

Regarding snow mass, the results of the ANOVA are shortly discussed (L345-346 in original manuscript and L380-381 in revised manuscript), but are not shown.

L380-381: ''Using SWE instead of snow cover yields similar results (not shown).''

L620: Please remove this claim unless further justified (see comment at line 509).

We agree. This claim was removed.

L680-682: '' In mid-winter, the NAO has the dominant influence, increasing the snow cover over Europe and inducing a widespread Eurasian cooling for negative NAO phases. In both observations and models, we found no robust circulation pattern following November snow anomalies.''

Technical Comments:

L199: "The first EOF analysis performed is based on the MMM calculated from the ALL experiment. The EOF pattern is denoted as EOF_BC, where…."

Done L218-219.

L218-219: ''The first EOF analysis performed is based on the MMM calculated from the ALL experiments. The EOF pattern is denoted as EOF_{BC}, where…''

L208: "to highlight the effect of the SIC variability."

Done L228-229.

L296: For clarity I suggest: "The overall impact of the sea-ice variations on the snow cover area and snow mass is limited, as shown by the differences between the MMM of ALL and NoSIC (Fig. 4e-h). Figures 4f-h have no clear trend and are not significantly related to observations..."

We modified the text as suggested L319-321.

L319-321: '' The overall impact of the sea-ice variations on the snow cover area and snow mass is limited, as shown by the differences between the MMM of ALL and NoSICvar over North America (Fig. 4f-h). The time series in Fig. 4f-h show no clear trend and are not significantly related to observations…''

Figure 6: separaty -> separately

Done

L619: In observations…

Done

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 modified the abstract.

L31-39:[…] ‘’On the other hand, the first mode of Eurasian snow cover variability in January, with more extended snow over western Eurasia, is found to precede by one month an atmospheric circulation pattern similar to a negative Arctic Oscillation (AO). A decomposition of the variability in the model simulations shows that this relationship is mainly due to internal climate variability. Detailed outputs from one of the models indicate that the Western Eurasia snow cover anomalies are preceded by a negative AO phase accompanied by a Ural blocking pattern and a stratospheric polar vortex weakening. The link between the AO and the snow cover variability is strongly related to the concomitant role of the stratospheric polar vortex, with the Eurasian snow cover acting as a positive feedback for the AO variability in winter.’’

The snow cover influence on the forecast skill is discussed with these references in the revised manuscript. In the introduction:

L100-103 : ‘’ Such influence is consistent with changes in subseasonal forecast skill when modifying the initialization of the snow cover in 2004-2009 (Orsolini et al., 2013) or in 2009-2010 (Orsolini et al. 2016), even if this influence is not found systematically from other periods and different models (Garfinkel et al., 2020). ‘’

The comparison between our results and Orsolini (2016) and Garfinkel et al (2020) is also discussed in the conclusion:

L702-705: ‘’ This suggests a two-way coupling between the snow cover and the internal atmospheric variability, where the snow cover anomalies amplify the AO-Ural blocking anomalies that generated them, acting as a positive feedback in the land-troposphere-stratosphere system.’’

- 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.

We included in the supplementary material two additional figures (Fig. S4 and S5) to include the new analyses suggested. Fig. S4 shows the regression of the 500-hPa and 50-hPa on January $PC1_{Int}$ in LMDZOR6. Fig. S5 shows the same regressions, but for the vertical component of the wave activity flux at 500-hPa and for the horizontal component of the wave activity flux at 250-hPa. We do see an amplification of the vertical component of the wave activity flux at lag 0 and 1.

The text was modified to describe and discuss these results:

L178-180: ''The detailed outputs from LMDZOR6 are also used with the monthly geopotential height at 500-hPa and 50-hPa, and the daily air temperature. For this model, the wave activity flux from Plumb (1985) is also calculated from daily geopotential height, zonal wind and meridional wind at 500-hPa and 250-hPa.''

L602-604 : ''The wave activity flux was calculated to investigate the propagation of the stationary waves (see Section 2.2). The regression of the wave activity flux on $PC1_{Int}$ shows an amplified upward component of the wave activity flux over eastern Eurasia and the western North Pacific (Fig. S5).''

The results of Lü et al. (2020) are now discussed.

L94-96: '' The pathway of the snow influence involves an amplification of the climatological tropospheric stationary wave associated to a lower-troposphere cooling as the snow cover increases (Cohen et al., 2014), as found for the 2017/2018 winter (Lü et al., 2020).''

L701-702: ''This mechanism is supported by the intensification of the upward propagating planetary waves following a larger snow cover extent in January, as found by Lü et al. (2020).''

- 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 year-to-year variability of the snow cover is now described for each calendar month with Fig. S1 in the supplementary material. The year-to-year standard deviation is shown in models and observations. We did not notice an underestimation in models with this simple analysis. We added a few lines in the new manuscript to describe this result.

L269-271: ''We also calculate the standard deviation obtained from year-to-year time series for each month. The interannual variability in models also agrees with that found in observations (Fig. S1).''

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?

This is now commented in the revised manuscript.

L350-353: ʺ In November, a decreasing trend is located east of Scandinavia, downstream of the Barents Sea. Such a location is in agreement with the large oceanic heat release expected from the observed Barents-Kara sea ice loss (Deser et al., 2015).ʼʼ

We also added a sentence in the conclusion:

L651-654: ʺ The sea ice loss only drives a small and insignificant fraction of snow cover trends. The sea ice loss only produces a decreasing snow cover trend located south and downstream of Scandinavia in January and April, and east of Scandinavia downstream of the Barents Sea in November.ʼʼ

- 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 provides a spread, which gives the range between the maximum and minimum daily SWE from the five products used. This spread was used to evaluate the mean snow cover and snow mass in Fig. 1 of the new manuscript (see grey shade).

L134-142: ʺ . Lastly, we use the daily CanSISE SWE in 1981-2010 (Mudryk et al., 2015; Mudryk and Derksen, 2017), which is based on five products: GlobSnow v2, ERA-Interim/Land reanalysis, MERRA reanalysis, Crocus (Brun et al., 2011) and GLDAS version 2 (Rodell et al., 2004). The CanSISE product also provides a spread based on the range (maximum minus minimum) of these five products. A snow cover from CanSICE is then estimated from the SWE using a threshold of 7 mm. If the daily SWE depth is lower (larger) than 7 mm, then it is assumed that the snow cover is zero (1). A minimum and maximum snow cover is also estimated with the same procedure using the SWE and its spreads, assuming the spread is centered on the mean SWE. The SWE and snow cover from CanSISE are then aggregated into monthly means.ʺ

The observational range is large, much larger than the spread between models in Fig. 4 (not shown). We did not evaluate how this uncertainty would affect the trend in Fig. 5. Instead, we improved the discussion regarding the trends of observational products and their uncertainty (as suggested by reviewer #1).

L71-73: ʻʻalthough observational data shows a large spread in fall and early winter (Brown and Derksen, 2013; Mudryck et al., 2017)ʼʼ

A comparison of the trends obtained from the different observational data is included in Figs. S2 and S3:

L335-341: ʻʻA comparison of the trends obtained in other observational datasets (Figs. S2 and S3) shows a large spread in trend estimates, with increasing snow extent in fall and early winter in the NOAA-CDR observations, while ERA5-Land, MERRA2 and CanSISE show a decreasing snow extent, as found previously (Brown and Derksen, 2013; Mudryck et al., 2017). The reason for this difference between the NOAA-CDR and multi-observation products is unknown (Mudryck et al., 2017). This suggests that observational uncertainties are important, especially in fall, as reported by Fox-Kemper et al. (2021).ʼʼ

- 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. The related sentences were 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 believe that showing the difference between ALL and NoSICvar is appropriate to illustrate the climate influence of the sea ice concentration variability. Therefore, we did not modify the figure as suggested.

- 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 were modified to only mention ERA5-Land.

Wording:

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

The first sentence was reformulated.

L23-24: '' The main drivers of the continental Northern Hemisphere snow cover are investigated in the 1979-2014 period.''

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

We reformulated the sentence:

L44-45 : '' It is also essential for the evolution and understanding of midlatitude and subarctic ecosystems.''