Final author comments

Anonymous Referee #2

We thank the reviewer for the 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.

Agreed. We will reformulate the statement in the conclusion.

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

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.

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"

We will reformulate the abstract according to these three comments.

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. This would indeed correct the estimated snow mass in these two models. We will correct all figures if such a lower value of snow density is appropriate for the model data used here.

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.

We will modify all plots and text accordingly.

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).

Please note that 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 calculated using an average of binary and full field SCF.

To answer the comment separate EOF analysis will be conducted on the ensemble mean from each model. The similarity between the results of each model and the MMM will be assessed. The text will be amended if results show some sensitivity to the nature of the field used.

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.

Indeed, there is a convention to assign the sign of the EOF pattern. This convention will be added in the text. To ease the comparison between Fig. 7 and Fig. 10, two solutions will be explored. We might add a grid with the corresponding values of latitude and longitude. Otherwise, we might also change the projection to increase the similarity between Fig. 7 and Fig. 10, and ease the interpretations.

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

Agreed.

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 will be corrected.

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

Thank you. Text and figure caption will be modified 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. We might use transparency or change the order as suggested to ensure that all 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. The meaning of the * symbol in Figure 6 will be explained.

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.

As mentioned above, to ease the comparison between Fig. 7 and Fig. 10, two solutions will be explored. We might add a grid with the corresponding values of latitude and longitude. Otherwise, we might also change the projection to increase the similarity between Fig. 7 and Fig. 10, and ease the interpretations.

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.

We thank the reviewer for pointing this out. This will be mentioned.

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 will be modified accordingly.

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…"

The text will be modified accordingly.

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 will be explored. If any link is found with the model variability, this will be discussed in the modified version of the manuscript.

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 proposed is more convincing. The text will be modified accordingly.

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

Agreed.

L572: Fig 16b?

Agreed.

L578: Fig 16a,b?

Agreed.

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.

Agreed. The abstract will be modified accordingly.

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 the line 592 discusses the snow cover variability (and not the snow mass). The snow cover variability related to sea ice is discussed in Figs. 5 and 6, so we believe that the results do support our statement on snow cover variability.

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

Agreed. This will be removed.

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…."

Agreed.

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

Agreed.

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..."

Agreed.

Figure 6: separaty -> separately

Agreed.

L619: In observations…

Agreed.