Weather Clim. Dynam. Discuss., author comment AC2
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Reply on RC2

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-AC2, 2021

Summmary

This study uses a novel experimental setup that allows the authors to test the impact of different Eurasion snow states on the seasonal evolution of the polar vortex and NAO. Regression analysis and compositing high and low snow dipole scenarios are used to show a relationship between 1st November Eurasion snow and the NAO.

Overall I think this is a well written paper, and an interesting study with a novel use of the exerimental setup. My only concern is that a discussion of non-linearities in the results seems to imply a large difference between the impact of high and low snow, and this difference may be hidden by the use of linear regressions. I believe this should be explained, or investigated briefly, before publishing.

Major Points

Section 4 e. Non-linearities, Fig. 11. This was very interesting to separate the effects of a high and low snow dipole, but then it seems to call into question the core result of the paper, i.e. the statement from the abstract: "Subsampling the perturbed forecast ensemble and contrasting members with high and low initial snow dipole conditions, we found that their composite difference indicates more negative NAO states in the following winter (DJF) after positive west to east snow cover gradients at the beginning of November." this statment is still true, but it seems to imply that high snow leads to a negative NAO (i.e. the canonical view of the snow-polar-vortex-NAO connection). But then Fig. 11b doesn't show a signal for high snow. Does this imply that the regressions, e.g. in Fig. 6 would also include this strong non-linearity (which, of course, would not be picked up the a linear regression). Perhaps this could be explored with a scatter plot, or something similar (I trust the specific details to the authors discetion), showing the snow dipole index plotted against indices of the NAO or an appropriate polar vortex index. If these plots showed a fairly linear response of NAO/polar vortex to a high/low snow dipole, then I think the conclusions are valid. If there is a strong non-linearity and the NAO response is mostly driven by low-snow, then perhaps the conclusions need to be modified somewhat, but this it still a very interesting result.

REPLY: Thank you very much for your comment. We modified Figure 11 (now Figure 10) now and added two additional supplementary figures plus changed the wording about this feature substantially throughout the document. That said,

here are possible explanations.

As you rightly said, our anomalies are still true in the model world, just as we described them. Now we should have been more precise with Figure 11, which we are now. In Figure 10a we basically compare a highly modified snow depth distribution with "reality" and in Figure 10b we compare a "realistic" snow depth distribution with reality. It follows naturally, that the stronger change of the earth system gives us a stronger signal. New information in Figure 10 now goes more in depth and shows that the high snow state is indeed able to generate increased sea level pressure over the Atlantic albeit with much lesser impact than the low snow state does.

This makes sense in the physical world. Adding a bit of snow on a snow free surface has a much higher impact than removing a bit of snow from a thick snowpack. Physically, the impact of added snow on the western domain has to be bigger than added snow on the eastern domain. All in all, we would argue for 1) a strong longitudinal gradient has a chance of modifying the NAO towards a more negative state and 2) a weak longitudinal gradient always prefers a positive NAO, which is the preferred state by the Earth System.

Now that is the model world. In the regression we try to model this gradient by defining an index. What we see is indeed a similar behaviour. Weak gradients favours positive NAOs. We also find that a linear regression model using only the eastern domain snow depth variability in explaining DJF NAO shows less significance than a model only using the western domain snow depth variability. The west-east gradient shows the highest significance for predicting wintertime NAO, no matter if we use ERA20C derived NAO or station-based NAO. For a new and more in depth discussion of this topic see Chapter 3f.

Minor points

Would it be practical to show Figure 1 and 4b together? Since it is interesting to compare these.

REPLY: We combine Figure 1 and Figure 4 in the new manuscript.

Line 64: "memory" (quotation marks in english are always up top)

REPLY: Fixed

Line 94 and 141: Is there a reason, or proposed mechanism, for why a snow dipole is a better index than a snow extent index used in many earlier studies, and can this be briefly mentioned in the introduction?

REPLY: Thank you very much for your comment. Just below Line 94 we refer to previous studies that showed that the November snow gradient or dipole is the strongest snow co-variate with DJF NAO, rather than a uniform large scale snow depth field or an October snow depth metric. Studies cited are Gastineau et al., 2017; Han and Sun 2018; Santolaria-Otín et al., 2021. Based on their findings, we decided that it makes most sense to use this November snow depth longitudinal gradient for our experiment setup.

Line 171: For land surface perturbed runs I assume the SSTs, sea ice, etc, are not changed, can this be stated.

REPLY: Thank you very much for your comment. Indeed, SSTs and sea ice are not

changed. We added that information to the text, see also Line 425–430.

Line 244: "positive snow dipole, as depicted in Figure 4," Do you mean Figure 3?

REPLY: Fixed

Line 248. Do the numbers in the brackets mean the values for plus/minus one standard deviation? Please write as a separate sentence if it's important.

REPLY: Fixed

Figure 6. Rather than Dec and DJF, perhaps showing Dec, Jan, Feb would be better so we can see the seasonal progression of Z and slp anomalies. If Nov was included too then we couls also see the SLP associated with the snow anomalies.

REPLY: Thank you very much for your comment. Our first initial concept included to show the individual months but we decided to focus on the most important information. Since the two other reviewers seem to be okay with that concept, we will stay with it for the time being. However, we added the monthly information for Figure 6 (new 5) and Figure 7 (new 6) now in the Supplement, so that the interested reader has access to the more detailed information. We do not show the November response, since as shown in Figure 9 (new 8) , significant anomalies really only emerge at the beginning of December. This is true for almost all variables (with snow being one exception of course).

Line 276: Why is the initial response shown for December, and not November?

REPLY: We do not show the November response, since as shown in Figure 9, significant anomalies really only emerge at the beginning of December. This is true for almost all variables. We added that information to the text.

Figure 6, 7, 10, Supp Fig 4: Short titles above. or on the left side of each plot with the variable, e.g. 500hPa Z, SLP etc, makes it easier to read, compared to relying on the captions.

REPLY: Thank you very much for your comment. We added the variable information to the plots.

Line 303: "ca." I think this abbreviation is usually used for historical dates. It's not incorrect, but perhaps just using "about" would aid coprehension for most audiences.

REPLY: Fixed

Line 319, Fig 8: Is the tropospheric jet shift related to the snow anomaly?

REPLY: All the anomalies in Figure 8 are based on the AFS-20C EXP subsampling and compositing and, as such, result from the snow gradient difference.

Line 336/413: "preceding", "precedes"

REPLY: Fixed

Figure 11: missing 'a' and 'b'

REPLY: Fixed

Line 448: Using parentheses to describe opposite effects can make sentences very difficult to understand, see article: Robock, A. (2010), Parentheses are (are not) for references and clarification (saving Space), Eos Trans. AGU, 91(45), 419–419, doi:10.1029/2010EO450004. https://eos.org/opinions/parentheses-are-are-not-for-references-and-clarification-saving-space

REPLY: Fixed