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Reply on RC3

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-AC3, 2021

This paper examines relationships between autumn Eurasian snow conditions and subsequent winter NAO development and associated climatic conditions over a 110-year span in the ERA-20C reanalysis, the ECMWF ASF-20C seasonal hindcasts, and a tailored hindcast set in which land surface initial conditions, including snow, are sampled from 20 adjacent years. It is found that differences in the longitudinal gradient of Eurasian snow at the beginning of November have discernable influences on the subsequent winter NAO, but that this relationship is not stationary over the 110-year period and is weaker in the hindcasts than in the reanalysis. Anomalies composited on extreme values of the longitudinal snow gradient point to roles played by the Ural ridge and wave fluxes influencing the stratosphere.

Overall, the paper makes interesting contributions to efforts to unravel the hypothesized causal connection between autumn Eurasian snow distribution and winter climate in the Northern Hemisphere. Although the methodologies and conclusions drawn appear generally robust, the paper could benefit from improvements to the presentation and some additional discussion of certain points as recommended below.

Main comments:

1) In the first paragraph of the introduction at line 57, regarding the NAO and its impacts in the winter of 2020/21 the authors could cite https://doi.org/10.1175/2021BAMSStateoftheClimate.1 which touches on this on p. S73 and in Fig. 2.40.

REPLY: Added the reference

2) The following cited papers (may not be a complete list) are missing from the references: Meehl et al., 2021; Diro and Lin, 2020; Wegmann et al., 2017; Wegmann et al., 2016; Jones et al., 1997; Koster et al. 2011.

REPLY: Fixed

3) Near line 143, please say a few words about why these particular eastern and western domains were chosen so that the reader does not have to refer to Wegmann et al., 2017 (which is not in the reference list as commented above). Presumably their latitudinal range is intended to encompass variations in the November 1 snow line? (This is explained finally around line 209, so maybe could add "whose choices are motivated below" or something similar at line 145.) Also, is it possible to represent the domains in Fig. 3 accurately, with borders along latitude circles rather than inaccurately as line segments?

REPLY: Thank you so much for your comment. In the introduction 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 gradient for our experiment setup. However, we added your suggestion to the Methods section to highlight how our motivation is linked to previous studies. We fixed Figure 3.

4) Inconsistent terminology is used to describe the index for Eurasian snow distribution. For example, in section 2a the eastern minus western difference in Eurasian snow depths is described as the "west-east snow cover gradient". However, snow cover as conventionally defined refers to the presence or absence of snow (irrespective of depth), and snow cover extent to area blanketed by snow. To avoid potential confusion, and for consistency with the discussion on lines 150-154, I suggest referring here to the "west-east snow depth gradient" or simply "west-east snow gradient".

REPLY: We fixed the inconsistent wording to "west-east snow depth gradient" throughout the manuscript.

5) Although the derived index is consistently and appropriately called the Eurasian snow dipole index or simply dipole index, the sub-ensembles of high and low dipole index hindcasts are referred to the "high-snow ensemble" and "low-snow ensemble". However, it's not obvious what "high snow" and "low snow" refer to, and calling these the "high-dipole ensemble" and "low-dipole ensemble" would be clearer and more consistent.

REPLY: We fixed the inconsistent wording to "high-dipole" and "low-dipole" throughout the manuscript.

6) At lines 156-157, it's not entirely clear whether using the "same definition for the NAO DJF index in seasonal prediction runs" means that the predicted NAO index is obtained using the first EOF of ERA-20C SLP, or the first EOF of predicted SLP.

REPLY: Clarified this point. We use the first EOF of predicted SLP.

7) It should be clarified in section 2b that these are not, strictly speaking, seasonal retrospective predictions since the ocean and sea ice boundary conditions after initialization are based on observations (same as ERA-20C) rather than being predicted using damped anomaly persistence or some other means.

REPLY: Thanks for pointing this out, we added this information.

8) In my opinion Fig. 2 would be easier to process if the three "LSxx" labels in panel (a) and associated dashed lines were removed, since their meaning isn't explicitly defined and it's not obvious exactly what the dashed lines are connecting to. (Although their meaning can be distilled from the main text, I found these features to be more distracting than illuminating, e.g. one wonders why there are three and not some other number, why

those particular years, etc. all of which are irrelevant to the point of the figure.) More broadly, it might be commented that it may take most readers longer to understand the figure than the descriptions of the two experiments in section 2b which are straightforward, although the figure does nicely schematize what was done.

REPLY: Thanks for pointing this out. Since the two other reviewers didn't mention a possible removal of Figure 2, we keep it in the manuscript but removed the dashed lines and added a bit of explanation in the new version.

9) Considering that the western domain typically has little snow on November 1 (lines 212-213), is the dipole index time series, and by extension the overall results of this study, much different if only variability in the eastern domain is considered? (The discussion of Fig. 11 on page 12 also suggests this might be the case.)

REPLY: Thank you very much for your comment. We do not have the capacities to repeat the whole analysis with a different subsampling procedure, but there are certain hints we checked and can check. 1) Fig 11 (now Fig 10) rather suggests that the western domain carries most of the information. That does make sense, since the eastern domain will most likely remain snow covered, no matter the snow dipole index, whereas the western domain can easily change from no snow cover to fully snow covered from year to year. 2) We can check your question in the statistical sense in ERA20C and the real world. We checked a linear regression between DJF NAO and a) the eastern domain, b) the western domain and c) the difference (or gradient) and found that the eastern domain has consistently the lowest significance for predicting DJF NAO, no matter if we use ERA20C derived NAO or station-based NAO. The western domain carries more significance and finally the gradient showing the best model out of these three options. As such, I would answer your question with: Yes, it would look much different. We added additional information in the new Figure 10 and added two new Supplementary Figures to help understanding this feature. All the additional information is in Chapter 3f now.

10) In the paragraph beginning at line 222, regarding the range of variability in the two forecast experiments, how do the NAO standard deviations for the individual ensemble members shown in Fig. 4a compare to the observational values? Also, is there any detectable difference in the ensemble spreads between the CTL and EXP forecasts, considering that the latter start with considerable ensemble spread in the land initial conditions?

REPLY: Thank you very much for your comment. We now added an additional Figure in the Supplement showing that the temporal standard deviation of the NAO index from the individual members in the model is very much in line with the ERA20C NAO index deviation. Additionally the new Figure shows the standard deviation among all members (read as member spread) between the 51 CTL members and 21 EXP members. Over the 110 years, the EXP members show higher variability in standard deviation, but the median over all 110 years is virtually the same, see lines 226-228.

11) The legends in Fig. 4 say AFS instead of ASF.

REPLY: Fixed

12) At line 242, please include a reference in order to provide some context about the early twentieth century Arctic warming, such as Polyakov et al. 2003, https://doi.org/10.1175/1520-0442(2003)016%3C2067:VATOAT%3E2.0.CO;2

REPLY: Added the reference

13) The placement of "as depicted in Figure 4" in line 244 suggests that what is being referred to is the depiction of the positive dipole snow pattern in Fig. 3a. If indeed this is referring to Figure 4 then it would be better placed at the end of the sentence.

REPLY: Fixed to Figure 3

14) One or more citations should be added to the sentence in 254-257 (even if cited previously).

REPLY: Added a reference

15) At line 281, regarding "significantly reduced geopotential heights over the extratropics", according to Fig. 7b these changes are only statistically significant over the Mediterranean region. Please reword accordingly.

REPLY: Reworded

16) Line 301 should state e.g. that anomalies between the two ensemble means in Fig. 7h are "less than" 1 hPa rather than "around" 1 hPa, considering that they never exceed the 0.83 hPa contour level. (Similarly in line 403.)

REPLY: Reworded

17) More attention should be drawn to the different color scales in Figs. 6 vs 7, and 5 vs 8 in order to keep the differences in the magnitudes of the observed and modeled show dipole responses in perspective.

REPLY: We highlight the systematic differences in those two approaches (see reply to your comment 24) now in the discussion section to make the reader more aware of the two very different approaches.

18) It seems noteworthy that the stratospheric evolution in Fig. 8 is similar to, but delayed with respect to that in Fig. 5. This merits at least mentioning, as would any hypotheses the authors might have for the origins of the delay.

REPLY: We now mention this fact at the end of section 3d. Since the atmospheric initial conditions are identical among all the members, it takes some time for the diverging behavior linked to land initialization to manifest.

19) Although the caption to Fig. S2 says "Shading indicates 90% significance level", there is in fact no shading. Does this mean that none of the anomalies are statistically significant?

REPLY: This is correct. None of the anomalies are statistically significant due to large spatial averaging.

20) Is the heading in line 332 intended to be there (and to be labeled "e.")?

REPLY: The heading was intended to be there, but our formatting was faulty. We now formatted the heading accordingly.

21) The term "resulted in" on lines 352 and 354 may imply causality more strongly than intended considering the context of opposite NAO responses to the same sign of snow anomalies. Suggest changing to "preceded".

REPLY: Reworded throughout the manuscript

22) The terminlogy describing the snow anomalies relating to Fig. 10 is inconsistent and somewhat confusing in that the main text in lines 351-352 refers to "high-minus low snow cover anomalies" and the Fig. 10 caption to "positive snow dipole forcing". It would be preferable if consistent terminology describing snow anomalies were used, as addressed also in comments (4) and (5).

REPLY: We fixed the inconsistent wording to "high-dipole" and "low-dipole" throughout the manuscript. We fixed the inconsistent wording to "west-east snow depth gradient" throughout the manuscript.

23) The composite November 1 SST and sea ice concentration differences shown in Fig. S4 which are around 0.1C and 0.02 respectively seem unlikely to have any major impact of DJF NAO. Also, do the authors have confidence that pre-satellite sea ice concentrations during 1901-1978 in ERA20C are sufficiently accurate for such an analysis?

REPLY: Thank you very much for your comment. We agree that sea ice concentrations for the period 1901–1978 come with large uncertainties. Nevertheless, we wanted to stay in the ECMWF ecosystem with our analysis. There are other sea ice and SST products available, however they also come with uncertainties. We change the rather absolute wording in the main manuscript to a more relative wording, highlighting that these are the preconditions found in ERA20C, rather than being the preconditions that actually occurred or that are generally found in reality.

24) Do the authors have any hypotheses for why the lagged atmospheric responses to autumn snow dipole index differences are so much weaker in the hindcasts than in ERA-20C? In particular, could this be related to the "signal-to-noise paradox" whereby circulation responses to radiative and surface forcings, as well as the predictability of the NAO, appear to be much weaker in models than is observed as argued by Scaife and Smith 2020, https://doi.org/10.1038/s41612-018-0038-4 ?

REPLY: Thank you very much for your comment. There are several reasons as to why the absolute response is different in magnitude. 1) What you refer to as ERA20C is a linear regression outcome, which units are "per Standard Deviation" and not raw physical units. As such, they are only indirectly comparable to raw units. 2) The regression with ERA20C represents ONE deterministic reality and any results out of one "member" should be stronger than any ensemble mean. 3) Our high and low dipole ensemble means incorporate a wide array of snow states (rather than one deterministic snow state per November), cancelling out possible effects and dampening the overall strength of response. 4) With the regression we sample possible (invisible) covariates with snow. Those covariates might strengthen the NAO response. Not 100% of the regression field we see might be attributable to snow. 5) ERA20C being a reanalysis, we can expect higher variability and better representation of reality than in models and added to that 6) land surface –troposphere–stratosphere interactions in the model are highly likely to be very imperfect. And as such, the model might substantially underestimate the impact of snow depth variability on the wintertime NAO in its model world. 7) As you pointed out, the "signal-to-noise paradox" always impacts predictability studies with dynamic forecast models.

Minor:

lines 141 and 170: a-forementioned -> aforementioned

REPLY: Fixed

line 153 identically -> identical

REPLY: Fixed

line 211: computes -> “represents” or “indicates”

REPLY: Fixed

line 297: mirrors -> mirror

REPLY: Fixed

line 298: increased -> positive

REPLY: Fixed

line 336: pre-ceding -> preceding

REPLY: Fixed

line 379: is -> does ?

REPLY: Fixed

line 383: remove 2nd “the” ?

REPLY: Fixed

line 412: “re-enforced” and “re-inforced” -> reinforced (also line 419)

REPLY: Fixed

line 413: pre-cedes -> precedes

REPLY: Fixed

line 423: mi- -> mid-

REPLY: Fixed

line 424: remain -> remains

REPLY: Fixed

line 424: suggest “dominant” -> “prominent” (or else simply “remains”)

REPLY: Fixed

line 447: “On the opposite” -> “Oppositely” or “On the contrary”

REPLY: Fixed

line 482: could remove “additional”

REPLY: Fixed

line 488: suggest adding “as in this study” after “years”

REPLY: Fixed

line 740: heighst -> heights

REPLY: Fixed

Caption to Fig. S3: “< 1 stand. dev” -> “< -1 stand. dev.”

REPLY: Fixed