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Earth System Dynamics Discussions

Interactive comment on "Eurasian autumn snow impact on winter North Atlantic Oscillation depends on cryospheric variability" *by* Martin Wegmann et al.

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The manuscript addresses an interesting and challenging topic. Information on the relationships between Eurasian autumn snow cover and following winter North Atlantic Oscillation would be very useful for seasonal prediction. The manuscript has its merits: (a) it convincingly presents a statistical relationship between the November snow cover and winter NAO and the lack of relationship between October snow cover and winter NAO, (b) it addresses the stability of the relationships over a period of 150 years, and (c) it also pays attention to other relevant factors such as Barents – Kara sea ice cover, the Atlantic Multidecadal Oscillation, and El Nino. Further, the Introduction is

very well written, demonstrating thorough knowledge on the study topic and its remaining challenges. However, the manuscript also has weaknesses, which I summarize below. Whether the revisions needed are minor or major, depends above all on how convincingly the novelty of the results can be demonstrated (my first comment below).

Major comments 1. It should be made clearer which of the results found are novel. In the Discussion section, it is mentioned in several places that the findings support the results shown by previous studies (Gastineau et al., 2017; Han and Sun, 2018; Douville et al., 2017; Cohen et al., 2014; Wegmann et al., 2015; Yeo et al., 2016), but the novelty of the results presented remains unclear for a reader.

REPLY: Thank you for your comment. We agree that the focus of this study needed to be clarified. We therefore edited the introduction and discussion part substantially to allow the reader to focus on the key messages we want to deliver.

2. The manuscript includes parts that are carelessly written, and generate a lot of confusion. a) In Figure 5, the projection between BKS ice concentration and November SLP anomalies shows positive values over a large region just east of Urals, but in general from the manuscript (and previous studies) I have got an impression that the decline of sea ice in BKS should favour Ural Blocking. Shouldn't this be reflected as negative projection in Figure 5b (similarly to Figure 5a)?

REPLY: Thank you for your comment. We realize that we forgot to mention that for Figure 5 sea ice concentration is multiplied by -1, thus Figure 5b and 5c show strong blocking together with a decline of BKS sea ice. We added that information in the figure caption of Figure 5.

b) I guess that on line 350 you should refer to Figure 7e instead of Figure 7c, and make it very clear that in Figure 7e the sea ice concentration is multiplied by -1 (I guess). Also, the positive correlation seems to last until late 1960s instead of late 1970s.

REPLY: Thanks for pointing out that mistake. We fixed the error with the Figure description and edited the whole paragraph accordingly. The improved description of Figure 7 can now be found from Line 361-374

c) It is not clear for me how Figure 8 supports the text on reduced variance of the snow index time series on lines 442-446. The standard deviation seems lowest in early 1900s and in 1960s.

REPLY: We reshuffled and rewrote large parts of the discussion to make the link between Arctic warm periods, increased cryospheric variability and the link to the prediction skill more apparent.

3. The Discussion includes vague parts, such as what could be done ("doubble could" on lines 389-392), references to preliminary results not shown on lines 453-455, and lines 476-479 (this paragraph should be removed). Also, how do centennial trends impact the results, if these trends were subtracted (lines 464-466)?

REPLY: Thanks for the comment. We agree that the discussion part was both incoherent and repetitive. We edited large part of the old discussion section and hopefully improved the train of thought throughout the section. We reworded the notion about the centennial trends, which are in fact not significant for the snow cover indices we use in this study (nevertheless we detrended the data just to be in line with comparable studies). What we wanted to mention are decadal trends found by Wegmann et al. 2017 for snow in long-term reanalyses. We changed the wording accordingly in lines 552-558.

Minor comments: Line 82: remove comma

REPLY: removed

âĂÍLine 112: months

REPLY: corrected

âĂÍLine 243: which snow indices?

REPLY: clarified

âĂÍLine 244: separate “and” words

REPLY: corrected

âĂÍLine 271: The impact does not look weak.

REPLY: Clarified this point

Line 329: Remove “slightly”

REPLY: removed

âĂÍLine 421: Supplementary Figure 6

REPLY: not sure what is the issue with this statement. We keep it like this for the time being.

âĂÍLine 428: Remove “that”

REPLY: removed

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