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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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Eurasian autumn snow impact on winter North Atlantic Oscillation depends on cryospheric variability This study investigates the changes in the relationship between the November snow- dipole and the following winter NAO using century-long reanalyses and modern reanal- ysis data. The relationship between snow variability and the NAO is an important topic. The study demonstrates the correlations between the November snow-dipole, BKS sea-ice, stratospheric variability and the NAO. Using long-term reanalyses to study these correlations is a good point, although they were produced with the assimilation of limited observations. I think this is important given that most of the existing studies are based on short temporal-range data. However, I have a few questions with the current version of the manuscript, which may be addressed by the authors. Major comments:

1) Conclusions in this study are drawn mostly from correlations/regressions, which would affect the robustness of them. Causality is also thus hard to determine. The November Snow-dipole does have some correlations with the following wintertime NAO variability (Fig. 2). This is also true for the November BKS sea-ice (Fig. 3a). However, the physical mechanisms remain unclear since studies often contradict each other and modeling results often don't support observational relationships. I think more analyses may be considered in order to generate more convincing evidence. In addition, as argued by Peings (2019), both anomalies in the snow/sea-ice and the winter stratospheric warmings can be driven by a common driver – Ural blocking. This raises the possibility that the correlations between snow/sea-ice and the wintertime NAO are statistical ones.

REPLY: Thank you very much for your comment. The focus on this study is not to determine causality between sea ice and snow cover. In fact other studies showed that link much better than we could here. Our study focuses on the fact that a) snow is a better predictor than sea ice and b) on the skill of the snow dipole for more than 150 years which is a novelty in the current scientific literature. We are well aware of the ongoing debate in the scientific literature about the dispute between observational studies and modeling studies. Here we argue that extending the investigation period from commonly 30 years to 150 years is important for the scientific discussion. Identifying strong relationships for 150 years is clearly a stronger argument for the existence of a physical mechanism than investigating 30 years. Our study helps to put modeling studies as well as the ongoing cryosphere changes in context. Concerning the very idealized study of Peings (2019) we mention in the discussion part the differences with our study and Peings (2019) as well as investigations we performed with blockings calculated from reanalyses. Nevertheless our study, even showing a linkage that is in line with the physical theory of snow to stratosphere to surface climate for 150 years, can not exclude the possibility of non-causality, that is correct. We made sure to underline that aspect in the discussion part.

2) The authors argue that the variability of the November snow-dipole largely determines the strength of the correlations between it and the wintertime NAO. But this conclusion is inferred from the 21-year running correlations and the 21-year standard deviations of the snow-dipole. The authors actually assume that the November snow-dipole is a driver of the wintertime NAO. As also mentioned in 1), causality may not be determined only from correlations/regressions.

REPLY: Thank you for your comments. It is unclear to the authors were exactly the issue is with the idea that increased variability in the predictor can strengthen the statistical relationship to the predictand. We still assume that the November snow-dipole is the physical driver behind the link to the wintertime NAO, we just highlight that the change in strength of this relationship is determined by the year-to-year variability of snow cover. We agree that our wording in the discussion of Figure 8 implied causality and we changed the wording accordingly. We also restructured the Discussion section to make highlight the implications of Figure 8 (now Figure 9)

3) The authors attribute increased correlation of the November snow-dipole (BKS sea-ice) with the wintertime NAO in recent years to the increased variability of the November snow-dipole (BKS sea-ice). Was the standard deviation of the BKS sea-ice displayed in the figures? From the analysis presented, it is hard to see how the three are correlated in a physical sense and which component of the cryosphere is more important in contributing to the recent NAO variability. There are a few studies exploring the impacts of the Arctic sea-ice on Eurasian snow. For example, Xu et al. (2019) studied the correlation between Autumn Arctic sea-ice and the winter snow cover in Northern Eurasia.

REPLY: Thank you for your comments. You raised an important point. Indeed the overlay of correlation and standard deviation was not visible. We now incorporated a new figure (Figure11) in the supplement that shows both the running standard deviation of BKS sea ice and the running correlation of BKS sea ice with the wintertime NAO. We mention it now in the discussion part. We used partial correlation to highlight the fact that snow cover is a stronger predictor for the winter NAO over long time periods that than the BKS, especially since Figure 7 shows that the BKS has a very weak relationship with the NAO for most of the 20th century. We mention the Xu et al. (2019) study and highlight that the authors looked at DJF only where as we focus on the autumn period.

4) I think the focus of this study needs to be clarified. The stratospheric pathway for either sea-ice or snow to impact the wintertime NAO variability is not new which can be found in many studies already cited in the introduction. Does the study emphasize the predictive nature of the correlation between the November snow-dipole and the wintertime NAO? If this is the case, why not consider some techniques such as cross-validation procedure to assess the predictive skills of the November snow- dipole? Empirical models such as those used in Chen et al. (2019; Section 6) may also be considered.

REPLY: Thank you very much for your comments. 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. As you rightly pointed out neither the stratospheric connection nor the impact on the wintertime NAO are new findings. Showing however, that these linkages are substantial and detectable for more than 100 years is a new scientific finding and an important puzzle piece for the ongoing debate that you mentioned above. Moreover highlighting the strengths of this relationship for Arctic warm periods is a new puts the current warm period in context and helps the scientific community to assess current cryopshere–atmosphere links in the framework of past climatic variations. We also newly added a very basic comparison of multiple regression prediction models based on cryosphere

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predictors for the 20th century and beyond at the end of the results section (lines 396-439), which we then also discuss in the discussion section.

Minor comments: 1) In addition to Han and Sun (2018) and Gastineau et al. (2017), the November snow- dipole was identified in an EOF analysis by Ye and Wu (2017).

REPLY: Thank you for pointing out this study. We added Ye and Wu (2017) to the references.

2) L28-29: Does the increased sea-ice variability enhanced that of the snow? REPLY: There is a correlation of variability on decadal timescales especially with October snow cover, yes. It is however more non-linear than the correlation between standard deviation of snow cover and standard deviation of stratospheric polar cap height as shown in Figure 8 (now Figure 9). We added that information to the supplement.

3) The section of Data and Methods may need some modification. In particular, more details of the reanalysis data may be given. In particular, recent satellite observations of the snow cover can be included in the analysis.

REPLY: Rather than describing the snow representation of the reanalyses in this process oriented paper we refer to the studies by Wegmann et al. (2017) and Orsolini et al. (2019). If that is not enough information for the reader, we would ask the reviewer to provide specific points of information that are missing.

REPLY: From this comment it is unclear as to what information can be gained by incorporating satellite information since reliable snow cover information during by satellites is limited to the beginning of the 1980s and this study focuses on long term relationships. Nevertheless we incorporated a comparison of the Rutgers snow cover product with the reanalyses products in recent decades in Figure 2 of the new Supplementary Information and mention them in the Data & Method section.

4) L153-154: In the analysis, were all the atmospheric fields detrended as well?

REPLY: yes, we added that detail to the description of the data

5) L244: Change ‘aandd’, ‘bande’ and ‘candf’ to ‘a and d’, ‘b and e’ and ‘c and f’.

REPLY: Changed accordingly

6) Labeling those multi-panel figures such as Figure 2 with additional text to indicate which variable is correlated with or regressed on to which variable may be considered to help the readers.

REPLY: Unclear what is meant here since it is always the same variable (DJF sea level anomalies) regressed onto the same variable (snow index).

References: Xu, B., Chen, H., Gao, C., Zhou, B., Sun, S., & Zhu, S. (2019). Regional response of winter snow cover over the Northern Eurasia to late autumn Arctic sea ice and associated mechanism. Atmospheric Research, 222, 100-113.

Chen, S., Wu, R., Song, L., & Chen, W. (2019). Interannual variability of surface air temperature over mid-high latitudes of Eurasia during boreal autumn. Climate Dynamics, 1-17. Ye, K. & Wu, R. Adv. Atmos. Sci. (2017) 34: 847. https://doi.org/10.1007/s00376-017-6287-z

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