

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

**Martin Wegmann et al.**

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This study presents and discusses statistical relations (diagnostics based on correlations and linear regressions) between Eurasian snow cover in autumn and wintertime atmospheric circulation anomalies, claiming a causal link (forcing and response relationship) the strength of which varies in different historical epochs. The authors make valid references to recent and past literature on this broad topic and show original and valuable results. The Reviewer would recommend this study for publication after some minor points are addressed (minor revision). In particular : (i) the authors should account for serial correlation in the timeseries when assessing statistical significance, this

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is an important point since it can potentially affect (quite strongly) the discussed statistics and the associated conclusions. (ii) the authors should make an effort to be more explicit when referring to dynamical pathways, even if they do not directly assess any of the mentioned dynamical relationships (a weakness of this study). (iii) the authors should explain (otherwise remove) their line of argument on the likely driving role of ENSO in respect to low-frequency (decadal to multi-decadal) variability.

REPLY: Thank you very much for your comments. We address the topics for the specific comments below. We removed most of the discussion concerning the low frequency impact of ENSO and make sure to highlight the dynamical pathway more.

Specific Comments 1. Line 17 Perhaps the mathematical term “non-stationarity” does not convey the right message here. Obviously, predictability due to ESC varies from year to year for two basic reasons: (i) ESC anomaly may be small, thus not providing a strong forcing leading to a predictable signal, (ii) other processes affecting predictability may be more dominant.

REPLY: Nonstationarity appears to be a common phrasing in climate science for the time dependence of the predictor to the predictand (see e.g. Kolstad & Screen 2019) and as such we keep this phrase for now but are open to specific suggestions.

2. Line 20 “tendency” also means time derivative. For this reason, avoid this expression, or clarify.

REPLY: We changed the wording to “NAO-like impact” throughout the document

3. Line 23 Delete “slowed”

REPLY: deleted

4. Line 24 “correlation power” is not approved terminology.

REPLY: We changed the wording to “strength”

5. Line 29 Three times using “power” in the abstract alone.

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REPLY: We changed the wording to “value”

6. Line 34 “climate mode... over” → climate variability pattern affecting winter climate over

REPLY: We changed the wording

7. Lines 36–37 Here and elsewhere, please put a comma between “et al.” and the publication year and use semicolons to separate different references.

REPLY: Changed accordingly

8. Line 38 The NAO is not defined as the strength of the gradient, it rather refers to the variability of this gradient (seesaw). Please rephrase.

REPLY: Rephrased accordingly

9. Line 40 “its configuration” → its variability

REPLY: Changed accordingly

10. Line 42 high-priority (with hyphen)

REPLY: Changed accordingly

11. Line 59 manifests itself / occurs / is manifested

REPLY: Rephrased accordingly

12. Line 79 a mechanism described by...

REPLY: Rephrased accordingly

13. Line 89 What exactly is meant here? “forming...” how?

REPLY: Specified the dynamic linkage

14. Line 93 summarized → discussed

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REPLY: Rephrased accordingly

15. Line 110 consequences → conclusions

REPLY: Changed accordingly

16. Line 111 who point to the prediction power of

REPLY: Changed accordingly

17. Line 114 link → chain (?)

REPLY: rephrased accordingly

18. Line 129 For a detailed description

REPLY: Rephrased accordingly

19. Line 143 “found” → defined (?)

REPLY: Rephrased accordingly

20. Line 148 The NAO centers of action are known to migrate zonally, but not so much meridionally [e.g. Barnston and Livezey (1987)].

REPLY: Deleted the mentioning of NAO and instead replaced with “jet”

21. Line 159 “normalized” → standardized

REPLY: Rephrased accordingly

22. Line 165 “is above” → is higher than

REPLY: Rephrased accordingly

23. Line 182 “the second dimension” → two dimensions (meridional and zonal direction)

REPLY: Rephrased accordingly

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24. Line 188 Blocks do not always divert the westerlies (they can also block).

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REPLY: Rephrased accordingly

25. Line 190 fulfill the two above-mentioned conditions

REPLY: Rephrased accordingly

26. Lines 213–214 “window” → period (?)

REPLY: Rephrased accordingly → 27. Line 217 “any” → each

REPLY: Rephrased accordingly

28. Line 233 This hints toward

REPLY: Rephrased accordingly

29. Line 244 Please check typos (missing spaces)

REPLY: Corrected

30. Line 269 increase polar (“heights” is plural).

REPLY: Corrected

31. Line 287 “increase” → aid

REPLY: Rephrased accordingly → 32. Line 306 anomalies are regressed

REPLY: Corrected

33. Line 309 Remove “a” (two occurrences)

REPLY: Corrected

34. Line 310 “is able to support” : please rephrase

REPLY: Rephrased accordingly

35. Line 325 “it” : please be more explicit for lucidity, what does “it” refer to?

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REPLY: Clarified and extended the sentence

36. Lines 327–328 “which in turn favors...” : how and why?

REPLY: Added additional information for the reader

37. Line 329 “slightly” : this undervalues the significant differences (4 half periods vs 3 half periods, not just “slightly out of phase”). In this paragraph the authors jump from an NAO reasoning to a direct connection of continental anomalies to the BKS, yet the respective dynamics are not compatible: the NAO links to more/less zonal advection, while Ural blocking links to meridional advection.

REPLY: We removed the slightly notion and clarified the train of thought for the connection between the BKS and the continental anomalies (lines 340-352).

38. Lines 338–341 This approach requires a proper evaluation of the effective number of degrees of freedom, which most likely are seriously reduced due to serial correlation (related to the low-frequency nature of the discussed variability but also to the applied filter).

REPLY: We addressed the question of serial correlation by performing Durbin-Watson tests for every pair shown in Figure 7 and did not find compelling evidence for the existence of serial correlation in these relationships. We added that information to the text (lines 384-389) and show the Durbin-Watson test statistics in the Supplement.

39. Lines 342–353 So the previously-discussed dynamics work in one decade but fail to work in another?

REPLY: Thank you for your comment. We are not able to exclude the possibility that the dynamics (as in physical mechanism) are still working during times with weak correlation. However, the mechanism might be weaker due to reduced variability in the predictor. Therefore we focus here on the statistical strength of this relationship rather than excluding the possibility of the mechanism still being the same mechanism, even in times of low correlation.

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40. Lines 371–373 Please help the reader see whether there is anything new here in respect to the cited studies.

REPLY: We edited the introduction and discussion part substantially to allow the reader to focus on the key messages we want to deliver and to highlight new findings.

41. Lines 375 “popular” (is this the right word?)

REPLY: deleted

42. Lines 397 low-frequency (with hyphen)

REPLY: Changed accordingly

43. Line 412 . . . pattern via a stratospheric pathway.

REPLY: We add information about this in the discussion (lines 464-474)

44. Line 428 Remove “that” before “seem”. Referring to this paragraph, the reviewer finds the reasoning related to ENSO to be poorly based given that ENSO itself cannot be claimed to be a primary driver of (multi)decadal variability. This is an important point that should be addressed in a revised version of the manuscript.

REPLY: We agree with the reviewer that the ENSO discussion is weak and not helping the focus of this manuscript. We therefore deleted this paragraph.

45. Line 435 strength (not in plural)

REPLY: Deleted this sentence and moved the necessary information to the beginning of the discussions section

46. Lines 433–443 Even two noisy processes after 21-year smoothing will exhibit periods of correlation and anticorrelation (purely an artifact related to limited samples and sub-samples). For robust statistics, the time window / period considered should contain at least a few periods... otherwise any result can be expected.

REPLY: Thank you for your comment. It is not entirely clear for the authors what is

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meant by considering different time windows for which analysis. However we show now in the Supplement a new Figure 8, which is basically Figure 7b for different running correlation windows ranging from 5 years to 31 years. We find that the main outcome of the analysis is not dependent on the time window changing from 11 to 21 to 25 to 31 years. The 5year window is noisy due to the nature of the high frequency variability in the system. These findings are consistent between ERA20C and 20CRv2c. We also added that information to the text (lines 384-389).

#### 47. Lines 513 “counterintuitive” → contrasting (?)

REPLY: We stay with counterintuitive but now it refers to “anthropogenic global warming”

FIGURE 2: How is statistical significance assessed? A suitable and rigorous test is required accounting for serial correlation (which tends to decrease the effective number of degrees of freedom). The colorbar (in this and other plots) is not a good choice as it does not allow distinguishing high from moderate values (e.g. 50 and 100 have very similar tones). Please choose a colormap with more colors. Also, add more ticks and labels in the colorbar, including the max and min values covered.

REPLY: Thank you for your comment. We now assess auto-correlation of the ERA20C snow index time series, the 20CRv2c snow index time series, the ERA20C 10hPa GPH time series and the BKS time series with auto-correlation function plots in Supplement Figure 5. We found no clear auto-correlation signal in the snow and stratosphere, however we found auto-correlation in the BKS sea ice index. We now highlight that fact in the text concerning Figure 2 to make the reader aware of that issue. Moreover, we found auto-correlation for the AMO index (as expected for) and no auto-correlation for the ENSO index. We do not show that information in the Supplement but we mention it in the manuscript text. Furthermore we updated the colormaps for Figures 2&3 and 5b with a higher range of values as well as higher color step change resolution.

FIGURE 4: The figure caption was found in a different page (unacceptable).

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REPLY: Moved up the figure caption.

FIGURE 5: The pressure unit is “Pa”, not PA. Also, please define what is meant by “time unit”.

REPLY: Clarified and corrected

REFERENCES: Why some appear gray and other in black font?

REPLY: That seems to be an artifact of the conversion process to pdf. We double checked and hope to have fixed that issue.

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Interactive comment on Earth Syst. Dynam. Discuss., <https://doi.org/10.5194/esd-2019-68>, 2019.

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