

We would like once more to thank the editor and the three anonymous referees for their constructive comments along the reviewing process that undoubtedly improved the manuscript. In the following, we will address the referees' comments point by point. We mark **red** the comments given by the referee, give our answers and comments in black and indicate how we addressed the amendments in the manuscript in **green**.

### **Comment on tc-2021-388**

Anonymous Referee #1

Referee comment on "The impact of climate oscillations on the surface energy budget over the Greenland Ice Sheet in a changing climate" by Tiago Silva et al., The Cryosphere Discussions

*I have read the responses and tracked revisions in "The impact of climate oscillations on the surface energy budget over the Greenland Ice Sheet in a changing climate" by Silva et al. The authors have made an effort to address my concerns and improve the paper, especially by re-writing much of the abstract and introduction to clarify the intent of the study and its main outcomes. My remaining comments are aimed at attaining further clarity, especially toward describing the cluster methodology and results stemming from this tool.*

General comment:

The manner of describing and presenting the cluster method and developing the classification is still a bit confusing. I suggest in the abstract and elsewhere clearly stating that the k-means clustering method (applied to GBI, NAO, IWV, etc) was used to create the NAG index. For ease of interpretation I suggest referencing the cluster "method" or "classification" (these are synonymous terms), and describing the optimal cluster solution for this analysis as three clusters (which describe the spectrum of the NAG index, i.e., positive, neutral, and negative NAG). Referring to each of these three clusters as a "classification" is confusing (e.g., Lines 247, 250, etc).

Thanks for the general comment. We now refer to k-means clustering and optimal clustering solution in the abstract as:

By using k-means clustering, we name the combination of the Greenland Blocking Index and the North Atlantic Oscillation index with the vertically integrated water vapor as NAG. NAG captures the influence of atmospheric circulation patterns from the North Atlantic on Greenland with the optimal solution of three clusters (positive, neutral and negative phase).

In addition, we revisited all instances in the manuscript where NAG is mentioned in order to be more coherent with the naming of the clusters along the manuscript.

These lines (L) below reference the tracked version of the manuscript.

L22: Would "intensified" rather than "reinforced" be a better word choice here?

Thanks! The word "intensified" is indeed a better choice.

L113: "The vertical tilt of temperature and pressure within large-scale systems" may make this more clear than simply "geopotential."

Thanks, we changed this as suggested.

L118: "dependent on phase of the concurrent climate oscillation" or similar wording is recommended.

Thanks, we changed this as recommended.

L120-121: Again, for simplicity in describing your analytical approach I recommend a revision such as "To overcome this dependency on one atmospheric index, we use a cluster method to derive the NAG index, which groups phases of the NAO and GBI with..."

Thanks for the suggestion. The simplified sentence reads:

To overcome this dependency on one atmospheric index, we apply a cluster method to derive the NAG by using NAO, GBI and the atmospheric water vapor (IWV) over the GrIS. Therefore, NAG links the role of the NAO with the prevailing mid-tropospheric circulation pattern over Greenland (GBI), along with the IWV over the GrIS.

L172: "All terms are in  $Wm^{-2}$ , and represent..."

Thanks, we changed this as suggested.

L234-235: By using Spearman's approach, dataset normality is not assumed a priori. Unless certain variables are correlated with time, I would encourage omitting the sentence "In such a way, no trend in the data is assumed a priori" as the presence of trend is not assumed by using this method, but rather is tested for using the Mann-Kendall approach. These assumptions should be clarified and if detrending is conducted prior to calculating Spearman's correlations that should also be stated.

Thanks for the clarification, we fully agree with Referee and removed the sentence as suggested.

L279: Do you mean "500 hPa geopotential height and surface temperature (or pressure?) vertical tilting." – please clarify.

Thanks for the remark. The word "height" was missing in the sentence. Also, it is indeed misleading to mention temperature vertical tilting when only geopotential height at 500 hPa and surface pressure are examined. Surface pressure in Figure S4 comes by suggestion of Referee #1 in the previous revision round, but since the result is the same, we kept the statement with the "geopotential height tilt" for simplicity. The sentence reads now as:

Despite the typical life cycle of the NAO phase lasting about two weeks (Feldstein 2003), the geopotential height vertical tilting described by Martineau et al. (2020) remains within seasonal composites due to strong baroclinicity (Fig. S4).

L508: "not captured by isolated indices" may be more clear

Thanks, we changed this as suggested.

### **Comment on tc-2021-388**

Anonymous Referee #3

Referee comment on "The impact of climate oscillations on the surface energy budget over the Greenland Ice Sheet in a changing climate" by Tiago Silva et al., The Cryosphere Discussions

Summary and remaining comments

*Thanks to the authors for their thorough responses to my comments and those of the other two reviewers. The paper has been improved considerably and I am satisfied with the authors' responses to most of my specific comments.*

*I feel the main aspect that needs further improvement before publication is the abstract, which in my opinion is still rather aimless and lacking clear statements of the main goals and findings of the study. If I am interpreting the results correctly, the authors have found that (1) the large-scale North Atlantic climate conditions have had a major impact on recent Greenland warming (with the North Atlantic influence more pronounced in certain regions and seasons); and (2) strong warming has occurred in some regions and seasons across all NAG phases, which the authors interpret as an indication that more localized influences (i.e. sea ice loss) have also contributed to Greenland warming. I think the abstract would be more effective if it were organized more clearly around these large-scale and local-scale contributions to Greenland mass loss and their interactions. The authors do a good job of describing potential interactions between the large-scale North Atlantic conditions and regional sea ice conditions in L47-54 of the introduction.*

*I have also listed several remaining technical corrections that I think the authors should make.*

Thanks for the remaining comments! We now organized the abstract in a way that large-scale (NAG) and local-scale (sea ice loss) influences on GrIS are separated.

Technical corrections

L10: The meaning of the phrase "for contrasting NAG phases" here is unclear. Does this mean the atmosphere has become warmer and moister across all NAG phases?

Thanks, the word "across" suits much better than the previous wording.

L20: "gotten" --> "become"

Thanks, we changed this as recommended.

Section 2.3: State up front (e.g. in L140) that the variable used to calculate trends in mass loss is the surface integrated ablation rate during summer (this is stated in the Fig. 1 caption and in L165, but it should be stated clearly when introducing the method in the text as well).

Thanks, we now included the variable stated in the Figure 1 in L140 and it reads:

In order to determine the breakpoint of the marked summer surface mass loss in RACMO2, we divided the GrIS into its main seven drainage basins (see Fig. S1) and regionally calculate 612 trends of the summer surface integrated ablation rate for periods with different lengths.

L176: “The used product” --> “This product”

Thanks, we changed this as suggested.

L177: based on \*a\* specific surface station

Thanks, we included the missing indefinite article.

L179: Restate here that GBI is derived from 500-hPa height over the region (60N-80N, 80W-20W)

Thanks, we changed this as suggested.

L209: If I am interpreting the supplement correctly, the distinction between the van den Dool and Hurrell NAO methods is that van den Dool uses 500 hPa height and Hurrell uses surface pressure? It would be helpful to state this difference between the two methods at this point in the paper text.

Thanks, although we pointed it out in the first sentence in Section 2.4, we stress this difference once more in the last sentence of the same section, and it reads:

A sensitivity analysis of the clustering and percentile classification using NAO (van den Loon et al. 2000) derived from 500 hPa geopotential height or NAO (Hurrell et al. 2003) derived from surface pressure and GBI is addressed in the Supplementary Material.

L213-216: I’m not sure of the purpose of the first paragraph in section 3.1. I think it could be removed.

Thanks, we partly agree with Referee #3. However, we would like to stick with most of the first sentence in Section 3.1, as often readers jump the Methods (+Supplementary Material) and dive immediately into the Results. In this case, they are informed about the large-scale resemblances among indices used in the study.

L269: due to \*the\* steep surface

Thanks, we changed this as suggested.

L429: shorthwave --> shortwave

Thanks, we changed this as suggested.

Responses to specific author comments

Thanks for the clarification on my comment on L120 about averaging variables over adjacent seas. It is now clear that the point of this analysis is not to quantify the magnitude of moisture sourced from these seas, but rather to assess whether NAG influences on atmospheric conditions in these seas are similar to the adjacent sectors of the ice sheet.

Thanks for the clarification on my comment on L282-284 about the seasonally lagged effect of winter skin temperature warming. I now understand the argument the authors are making, and I think the introduction of the statement “Nevertheless, the NAG phase in summer governs the overall surface mass loss” in L318-319 helps the reader understand that the authors are not explicitly connecting winter skin temperature warming in individual seasons to enhanced mass loss in the subsequent summer.