4. September 2023

Dear Prof. Pfahl,
Dear Reviewers,

We are pleased to submit a revised version of the manuscript "European summer weather linked to North Atlantic freshwater anomalies in preceding years". Based on the detailed and constructive feedback provided by both reviewers, we have carried out a major revision to clarify and improve the manuscript.

The main shortcoming of the last version was a lack of clarity and a poor presentation. This was evident from both reviews. Reviewer 1 criticised a lack of details and precision in the description of the methods and results. Reviewer 2 was additionally concerned about too much certainty in the language, which did not adequately reflect the underlying uncertainties. Both reviewers also recognised a substantial improvement, however.

Considering that the lack of clarity has been an ongoing issue with this manuscript, we have followed the reviewers' suggestions and removed the analyses that evoked most comments and that are not necessary, including the model and trend analyses which had shifted the focus. In the remaining analyses, we have provided more details and clearer explanations of the methods, a more comprehensive uncertainty analysis, and a more precise description of the results. We are also more cautious about the language, clearly separate interpretation from the results, and we have fully removed contentious statements from the manuscript.

Since Reviewer 2 was additionally concerned about the robustness of the results, we have carried out a rigorous statistical analysis, including a new section "Significance and robustness" (Section 4.5) and Appendix B showing that the results are not sensitive to the choices made in the methods. In conclusion, we now demonstrate the link between freshwater anomalies in winter and subsequent European summer weather using three techniques: (1) correlations and regressions, (2) composites, and (3) a multi-taper coherence analysis. Moreover, the correlation and regressions are now based on three freshwater indices – including a new un-subsampled SST index covering all years. We clearly state the advantages and disadvantages of each index and explain the trade-off between the magnitude of the correlations and the number of years included in each index, reflecting the complexity of freshwater variability. Combined, the regressions, composites, and coherence analysis show that the identified links are robust, irrespective of the method that is used, independent of the spatial and temporal characteristics of the freshwater indices, and significant on all resolved timescales from years to decades.

Detailed changes are explained in the response letters, where the responses to the reviewers' comments are shown in blue, and the resulting changes in the manuscript are shown in red. Figure and line numbers refer to the revised manuscript without tracked changes:

- Reviewer 1: page 2
- Reviewer 2: page 17

The manuscript has greatly benefitted from the review. Thus, we thank both reviewers and the editor for reviewing and handling this manuscript, helping us to clarify and improve this study.

Sincerely,
Marilena Oltmanns, on behalf of all authors

Responses to Reviewer 1

The authors use statistical analysis of observations and reanalysis data to examine a potential link between freshwater anomalies in the North Atlantic subpolar gyre region and summer weather in Europe in the following year. They propose that stronger freshwater anomalies resulting from increased glacial runoff in Greenland leads to an stronger meridional SST gradient between the subpolar and subtropical gyre and consequently increased baroclinic instability in the atmosphere above. The resulting deflection of the jet stream alters the advection pathway of maritime air masses over Europe. The foundation of the analysis are freshwater indices derived from a mass balance equation that are used to identify freshwater anomalies in relation to simultaneous sea surface temperature (SST) anomalies linked to the North Atlantic Oscillation (NAO).

The revised version of the manuscript is a substantial improvement to the previous draft, particularly with respect to the derivation of the freshwater indices. However, there is still lack of clarity and detail in some methods, as well as precision in the description of the results. Therefore, the manuscript in its current form is not suitable for publication and I suggest major revisions based on my comments below.

We thank the reviewer for reviewing our manuscript once more, and for again providing many detailed and helpful comments and suggestions!

The main comments referred to a lack of clarity and detail in some methods, and a lack of precision in the description of the results. Following your comments and suggestions below, we have carried out a major revision to provide clearer and more detailed explanations of the methods, remove any ambiguities, and describe the results more precisely.

I. General Comments and Suggestions: Throughout the manuscript, multiple indices are used, but their exact definition unclear. Some details regarding the methods are often only mentioned in the figure captions leading to interruptions in the reading flow. Additionally, the description of results often remains rather vague or general, and does not necessarily match the figures. Similarly, I encourage the authors to be more concise and precise in their language, e.g., sometimes it is not immediately clear if they refer to the ocean or atmospheric anomalies or geographic descriptions are very broad. Some of these instances are mentioned in my comments below.

We thank the reviewer for making us aware of the remaining unclarities, vagueness, inaccuracies, or impreciseness in the manuscript.

The revised manuscript includes a clear, unambiguous derivation of the indices. We also motivate the use of multiple indices more clearly and explain their specific advantages and disadvantages. In addition, the text is now more specific and precisely describes the figures. All information in the figure captions is also provided in the main text. Special care is taken to differentiate between ocean and atmospheric anomalies and to clearly describe detailed, geographic features.

II. Main Comments:

1. Section 3: The combination of the appendices from the previous version with the main text helps better understanding and following the construction of the freshwater indices. However, there are a few points that require some clarification.

1. l. 141-143: Equation (3) already has the mean state subtracted, yet you say “we subtract the mean values from the resulting equation”.

We agree that the term "resulting equation" was confusing. We have rephrased this paragraph and restructured the associated section. Thus, we now include more steps in the derivation. We have also changed the nomenclature to make it consistent with earlier studies, and we explain the individual terms more clearly (Section 3.1).

2. l. 145: Please specify the values of α and β.

We apologise that the preceding term "linearised equation of state" was confusing, because α and β are not constant in space and time. Thus, we cannot specify their exact values.

We have now removed the term "linearised equation of state" and only specify α and β as the thermal and haline expansion coefficients. Moreover, when we calculate the surface mass balance in Section 4.1, we now explain (line 271):

"We further compute α and β using the Gibbs Seawater Routines (McDougall et al., 2009), in accordance with the highest standards of current knowledge. Noting that the effects of salinity and pressure on α and β are small and only affect the second decimal place or less, we use nominal values of 35 g kg^{-1} and 10 db for the subpolar region in winter to compute α and β. The dependence of α and β on temperature is larger, however. For instance, α can vary from $5 \cdot 10^{-5}$ to $18 \cdot 10^{-5}$ °C^{-1} across the subpolar ocean surface. Thus, for an enhanced accuracy, we allow α and β to vary with temperature."

In the subsequent paragraph, we also point out that all terms in Eq. (5) are calculated before the regression onto the freshwater indices to ensure that Eq. (5) remains valid after the regression.

3. l. 165: There is no Figure 1a that shows the NAO in summer.

Thank you for pointing out that we referred to the wrong panel. In the revised version, this sentence no longer appears.

4. Section 3.2: Some important information/motivation is either missing or presented later in the text which makes it difficult to follow the approach and understand the derivation of the freshwater indices:

Thank you for letting us know that this section was still unclear. We have rewritten the full section and now provide a clear, unambiguous, mathematical derivation.

> 1. It is unclear how the resulting freshwater indices are defined. Are they (scaled) summer NAO indices, i.e., FE = {-NAOS,i | NAOS < -0.5, i=FE years} and FW = {NAOS,i | NAOS > - 0.5, i=FW years}?

You are right that the F_E and F_W indices directly correspond to the NAO_S values. They are not scaled and we now state this in the text (line 217).

Using the formulation you suggest would be misleading since the indices do not contain all NAO_S values above or below -0.5 but subsets of it. Since there exists no simple expression, we now provide a clear mathematical derivation to avoid any ambiguity (Section 3.2, line 209 – 236). The resulting years are uniquely identifiable in Figure 1c, and we now also list them in Appendix B.

At the end of the derivation, we further clarified their naming (line 244): *"Since the associated cold anomalies are strongest over the southeastern subpolar region (Fig. 1e), we refer to the selected 8 years as* F_E *subset – shown by the clear red coloured bars in Figure 1c. For the other subset, the maximum cold anomalies extend over the full subpolar gyre, including the western part (Fig. 1f). Thus, we refer to the selected 17 years as* F_W *subset – shown by the clear blue coloured bars in Figure 1c."*

We further added a sensitivity analysis to the number of years included in the subsets (Appendix B). Thus, we show how the results change with alternative definitions, with the corresponding years clearly shown (Figs. B3 to B7).

2. l. 173-179: This paragraph is missing some motivation why and how you use the meridional SST gradient to establish a relationship between the summer NAO and freshwater anomalies in the following winter. I suggest something along the lines of: "Given the theoretical considerations based on the mass balance analysis (Section 3.1), there are certain conditions under which freshwater anomalies associated with the summer NAO are accompanied by temperature anomalies. To identify these conditions, we regress winter SST onto two subsets of the summer NAO…". Please give a rationale for the seasonal lag.

Thank you for this helpful suggestion. We have now integrated your suggestion.

Since we think this information needs to come earlier, we have rewritten the end of Section 3.1 and the beginning of Section 3.2. Thus, we now motivate the use of SST-based indices by the constraint of salinity on the SST (line 174), and then explain the use of SST anomalies associated with the summer NAO specifically (line 194).

We further explain the seasonal lag. First, we motivate focussing the SST analysis on the winter period (line 170): *"Considering the competing influences of salinity and temperature on stratification, the conditions, in which freshwater may impact the temperature, can only occur in autumn and winter, when surface water is cooled by the atmosphere, becomes denser and sinks. In summer, the temperature and salinity do not compete in their influence on stratification and thus, do not constrain each other."*

In the subsequent derivation of the indices, we further explain (line 199): *"[...] Yet, even if the freshening occurs in summer (when melting and runoff is strongest), the effect of the freshwater on the SST would only become visible in autumn and winter (when the freshwater impedes the sinking of surface water). Thus, we focus on the SST in winter to infer the potential existence of freshwater anomalies."*

The seasonal lag of ~6 months is well in line with the observed and theoretical propagation time of seasonal runoff and meltwater from the boundary currents into the interior subpolar region. For instance, a typical length scale from the boundary to the interior is 1,000,000 m and a typical timescale from summer to winter is 3600 · 24 · 30 · 6 seconds (= 6 months). Thus, the minimum current speeds required to distribute freshwater over the subpolar region is ~0.06 m s^{-1}, which is smaller than the observed velocities ranging from up to 1 m s^{-1} near the boundary to 0.1 m s^{-1} in the interior (based on the altimetry).

A more detailed description of the seasonal freshening with additional references is provided in Section 4.2.

3. l. 174-175 & l. 181: Is the "meridional SST gradient" the same as ΔSST? Please define ΔSST in the text.

To avoid confusion, we have removed the word "meridional SST gradient". Instead, we state that we observe an increased SST difference between the subtropical and subpolar gyres. We have also added two panels in Figure 1 (Fig. 1a and b) to confirm this statement and show the associated SST anomalies.

4. l. 180: The "conditions c" are not explicitly stated in the text nor referred to in the remainder of the manuscript. I suggest to remove the variable name "c".

Thank you for suggestion this. We agree with you and have removed the variable name "c", as you suggest.

5. l. 200-203: The significant SST anomalies associated with FW extend from the Labrador Sea in the west almost all the way to the British Isles in the east while the SST anomalies associated with FE occur mostly in the central North Atlantic subpolar gyre region. Thus the naming can be misleading unless you specifically refer to maximum cold anomalies.

Thank you for pointing this out. We have followed your suggestion and now specifically refer to the maximum cold anomalies. In addition, we now describe the different spatial characteristics of the cold anomalies more precisely.

2. Section 4.7: This section lacks a clear structure, clarity, and important details that make it hard to reconcile the results with the rest of the paper. In Section 3.2, you establish a non-linear relationship between a subset of summer NAO and SST anomalies associated with freshwater anomalies. Here, you use the "full, un-subsampled summer NAO" as a linear predictor to "assess the role of freshwater triggering the SST signal". By regressing different variables onto the summer NAO and resulting SST pattern (which is quite different compared to the SST patterns related to FE and FW, cf. Figures 6a and 7a with Figure 10a), you describe the associated atmospheric response in both observations and models. As you point out in your concluding sentence of this section, "further studies are necessary to confirm the dynamical contribution of freshwater anomalies to the largescale SST pattern". Since an explicit link to freshwater anomalies, and their role in triggering your proposed chain reaction, is missing, this section creates confusion for the reader. I suggest to add more details following my comments below or remove this section entirely from the manuscript.

Thank you for pointing out that this section was unclear. We have now removed it entirely from the manuscript. However, we still answer your questions about this section below.

By referring to different subsets of years, different timescales and time periods, the previous Figures 6, 7 and 10 from the last manuscript cannot be compared to each other and do not contradict each other. In Section 3.2, we now clearly explain the temporal and spatial complexity of freshwater variability in the subpolar region, resulting from different drivers of freshwater variations with different timescales and different spatial characteristics. The complex 3-dimensional variability of freshwater in space and time cannot fully and precisely be captured by a single, one-dimensional index. Thus, we use several indices. By regressing the SST (or SSS estimates) on one specific freshwater index, we reduce the influence of other drivers of freshwater variations and obtain a near-linear relationship.

The full, unsampled summer NAO was used as an indicator for one component of the freshening – the component that is attributed to seasonal melting or runoff (Fig. 3a and b). The associated freshening is strongest over the eastern subpolar region, and it is characterised by a significant trend.

However, we agree that the mix of the different NAO indices was confusing. Thus, we have removed this section, and we do not use the full, un-subsampled NAO index anymore. Moreover, in the revised manuscript, we took care to clearly explain the different characteristics, advantages, and disadvantages of each freshwater index.

The following comments (1 to 9) refer to an analysis that we have now fully removed. While the analysis was meant to provide insights into the processes associated with the SST anomalies in summer, we agree that it is not necessary for understanding the link between freshwater and SST anomalies. Thus, to improve clarity and conciseness, we have removed it.

1. Please specify what you are trying to predict using the “un-subsampled summer NAO as predictor”.

The un-subsampled summer NAO was used an index for the seasonal freshening and runoff, and it was used as a predictor for the summer weather in the subsequent year.

2. l. 420: Please clarify what you mean by “reduced NAO states”.

We referred to years in which the summer NAO index was small (or more negative).

3. l. 422: I believe this should be Figure 10a-c.

Yes, that is correct. Thank you for noticing this.

4. l. 423: Please discuss the significance that the warm anomalies occur mostly over land around the Mediterranean Sea, but the dry anomalies over the ocean.

The regions of the warm and dry anomalies did not exactly coincide which we attribute to their different drivers within extra-tropical weather systems. While we have removed this section, we include this information now in Section 4.4, where we state (line 437):

"Considering that precipitation anomalies preferentially occur along trailing cold fronts and are shifted southward relative to cyclone centres (Booth et al., 2018; Kodama et al., 2019), the observed displacement of the dry anomalies relative to the warm anomalies is expected from their locations within individual weather systems and consistent with other studies (Yu et al., 2023)."

5. l. 429: Please specify that the upwelling occurs in the ocean below the center of the cyclone.

Yes, that is correct. Since the Ekman transports on the northern hemisphere are directed to the right relative to the wind direction, all atmospheric cyclones in the extra-tropical region lead to upwelling in their centres.

6. l. 442-443: Please define SSTFW explicitly. Is this related to the FW index? If not, I suggest to use a different name to avoid confusion.

The index referred to the variability of the SST pattern in the panel next to it. We have now removed the index, and we do not use this name anywhere in the revised manuscript.

7. l. 443-444: This sentence is unclear. Please specify which "observational analysis" you are referring to. Does that mean you define the SST index based on the model simulations?

Since the model simulations were forced by the observed SST, the index used in the simulations was the same as that in the observations. We carried out the same regressions of temperature and P-E in the observations (obtained from ERA5) and simulations (obtained from the models) to allow for a direct comparison.

8. l. 446: Please add more details in the text: Are you referring to temperature and precipitation anomalies from climatology or ensemble mean?

We referred to the anomalies obtained by regressing the precipitation and temperature variability in all simulation runs onto the freshwater index. Thus, the anomalies were relative to the ensemble mean climatology.

9. l. 449-450: The observed and simulated atmospheric responses agree well over the ocean, while the simulated response over land is much smaller. It is not clear how you determined the statistical significance from the 90 ensemble simulations given that deems very small anomalies close to zero as statistically significant – please add more details in the text and discuss the implications.

The significance was obtained from Student t-tests. Due to the high number of realisations (90 simulations x 40 years = 3600 realisations), the warm and dry anomalies do not all occur at the same location. They are shifted over different areas and sometimes cancel each other out, for instance when the jet stream is shifted southward or northward. Thus, given the high number of realisations, it is expected that the magnitude of the resulting warm and dry anomalies, obtained from the simulations, is smaller and distributed over a larger area compared to the observations that only include 40 years or realisations. This was consistent with our results.

There is a trade-off between the number of degrees of freedom and the magnitude of the correlations and regressions. It is expected that, with increasing number of simulations and increasing degrees of freedom, the magnitude of the correlations and regressions decreases because of the increased variance of the temperature and precipitation variability. However, the significance of the correlations and regressions is unaffected because the reduced correlations are compensated for by the higher number of degrees of freedom.

In the revised manuscript, we explain the trade-off between the number of degrees of freedom and the magnitude of the correlation in Sections 3.2 and 4.6 and in Appendix B.

Again, we thank you for providing the above comments. We have fully removed the corresponding analysis (the previous Section 4.6 and 4.7). However, we took care that we have integrated your comments in the other sections to avoid any potential misunderstandings in the revised manuscript.

III. Additional Comments and Suggestions:

1. l. 4: What are “medium-term climate trajectories”?

We referred to seasonal to interannual climate evolution. We have now removed this term and the associated sentence.

2. l. 97: Please state years of “recent period”

We have now removed this term. Instead, we state: *"Over the investigated period [1979 to 2022], the climate system has been characterised by increasing greenhouse gas concentrations."*

3. l. 101-105: The 2 m-temperature trends are different over land than over the ocean (e.g., Simmons, 2022). Your averaging area includes large parts of the North Atlantic. Please discuss if this has any implications for your results.

Thank you for pointing out this study by Simmons (2022). The trends in Simmons (2022) show the 2-m air temperature trends. Over the North Atlantic and Europe, the trends largely show the signal that we explain in our study by changes in the ocean and atmospheric circulations following enhanced freshening in the subpolar North Atlantic. Thus, the study nicely supports our method of subtracting regionally averaged trends.

Since, in this study, we are specifically interested in the warming and cooling effects induced by changes in the ocean and atmospheric circulations, removing this trend signal would be counterproductive. After all, it represents the central motivation behind subtracting regionally averaged trends.

We have now included the reference in the manuscript and explain (line 101):

"[...] Considering that the freshening trend is an important part of the signal we are investigating, removing trends at each location (or grid point) would remove an important part of a signal that we are interested in. Thus, to reduce the influence of increasing greenhouse gas concentrations on European air temperatures, we subtract regionally averaged trends from the air temperature. The method of subtracting regionally averaged trends is motivated by the

observation that greenhouse gases are distributed comparatively uniformly in the atmosphere (Reuter et al., 2020) whereas the observed surface warming exhibits large regional differences (Simmons et al., 2022). These regional differences in surface warming result from changes in the ocean and atmospheric circulations, which are redistributing the excess heat. Since, in this study, we are specifically interested in these dynamic processes associated with changes in the ocean and atmospheric circulations, we are subtracting a spatially uniform warming trend associated with increasing greenhouse gases."

4. l. 230-231: This is only true in the absence of vertical mixing.

Yes. We now explain in Section 4.1 that vertical mixing cannot explain the SST anomalies. We agree that this sentence raised confusion since we did not clearly explain the different roles of the different terms in the mass balance equation. Vertical mixing can have two causes: (1) passive entrainment in which case it only affects the mixed layer depth but not its density, and (2) an active energy source that mixes denser water upward (against gravity).

In the revised manuscript, we have changed the nomenclature of the mass balance, Eq. (1), to make it consistent with earlier studies (e.g., Griffies and Greatbatch, 2012), and we clearly explain the individual terms. Thus, we clearly distinguish between passive entrainment and active drivers of vertical mixing (line 133):

"The passive component is defined as the entrainment of mass into the mixed layer that results from mixed layer deepening as the mixed layer density increases. The active component results from externally forced, horizontal and vertical mass fluxes, such as wind-driven Ekman transports and upwelling. The passive component can only change the mixed layer depth, but not its density, while the active component does change the mixed layer density."

In the subsequent derivation in Section 3.1, the passive entrainment term cancels out since it is not relevant for the mixed layer density. Moreover, we have included additional references providing a detailed evaluation of the energy sources and forcings that drive ocean currents and vertical mixing (Ferrari and Wunsch 2009; Wunsch and Ferrari 2004). These studies agree that, on the timescales and spatial scales we consider, the wind stresses and air-sea fluxes are by far the largest energy source that can drive horizontal and vertical motion and hence, mass fluxes. Additional sources include biological activity, tides, geothermal activity, and others. However, they are negligible on the timescales and spatial scales considered (line 276 – 284).

The influence of the largest and most important, active drivers of vertical mixing over the open ocean, away from topographic boundaries, and on interannual timescales – the wind forcing and air-sea fluxes – is evaluated as part of the mass balance analysis (Section 4.1, Appendix A). Thus, we find that both drivers are not significantly correlated with our freshwater indices, their magnitudes are also too small, and their spatial characteristics also do not agree with those of the identified SST anomalies (line 285 – 292).

In conclusion: Vertical mixing does exist in the mass balance equation, but it cannot drive a substantial mass flux across the base of the mixed layer that is comparable to the density anomaly implied by the identified temperature anomalies.

5. l. 238-239: To keep the reader on board it would be helpful to state that "using this relationship, we estimate SSS from SST anomalies for the two NAOS subsets".

Thank you for suggestion this. We have now included this statement (line 300).

6. Figure 3: Did you actually do the regression? Given the linear relationship between SSS and SST, and assuming a constant coefficients, panels a and b should be equal to α/β * (negative areas in Figures 2a and b). However, the SSS anomalies show more structure. Is that due to the choice of contour levels? Please clarify.

The relationship between SSS and SST anomalies is not linear because α and β depend on temperature. Thus, they must show a different structure.

By integrating your second comment in Section 4.1, and explaining how α and β are calculated (line 271), we think this has been clarified.

7. l. 241-242: Please be more precise: the significant area of FW SSS anomalies extend further eastward compared to the anomalies associated with FE.

We are now more precise and state (Line 301): *"[] the maximum freshwater anomalies (or minimum SSS anomalies) associated with F_E occur over the central subpolar region (corresponding to the south-eastern subpolar gyre) and are spatially more confined than the maximum freshwater anomalies associated with F_W. Moreover, the significant area of F_W freshwater anomalies extends further eastward, westward, and northward compared to F_E freshwater anomalies and the anomalies have a smaller amplitude, consistent with the associated cold SST anomalies (Fig. 1e and f)."*

8. l. 244: Please refer to the appendix to ensure that the reader knows how the uncertainties were estimated.

Following your suggestion, we now refer to the appendix. Thank you for suggesting this.

In addition, we state in the main manuscript (line 307): *"These uncertainties apply to the cold anomaly regions, enclosed by the 95% lines. Uncertainties at each individual grid point can differ."*

9. l. 252-253: The different phases of the NAO are not associated with "opposite atmospheric circulation patterns". They differ in the strength of the pressure difference between the subpolar low and subtropical high pressure system. In this regard, the wording "lower/higher NAO states" seems slightly odd. I suggest to make explicit links to FE and FW throughout the whole text.

We agree that the term "opposite atmospheric circulation patterns" was misleading. Instead, we should have referred to "opposite atmospheric circulation anomalies" since F_E freshwater anomalies are associated with a more anticyclonic atmospheric circulation anomaly over Greenland and the subpolar North Atlantic in the preceding summer, while F_W freshwater anomalies are preceded by a more cyclonic atmospheric circulation anomaly over Greenland and the subpolar North Atlantic.

However, after revising the manuscript, we found that this sentence is not needed and mostly raised confusion. Thus, we have now fully removed it. In addition, we make explicit links to F_E and F_W throughout the text.

10. l. 280-281: It is unclear how you identify “an intensified subpolar gyre circulation in the Labrador Sea” in Figure 4c.

Indeed, this sentence was misleading. We have now removed it.

11. Figure 4: What are the vectors in panel b? Please define dSSS in the text: it is unclear what you mean by “newly arriving, seasonal surface freshening from summer (August) to winter (January to March). Do you accumulate or take the mean of the freshwater anomalies during the winter months and subtract it from the August value?

We now specify: *"The arrows represent the mean geostrophic surface flow, obtained from the absolute dynamic topography, averaged from August to March (the period of the freshening)."*

Regarding your second question: Yes, we subtract the August value from the winter value to calculate the accumulated freshwater anomalies from summer to winter. This is now clarified both in the figure caption and the main text (line 329).

12. l. 299-301: If the “seasonal freshening is mixed down and too small to affect the absolute SST anomaly in winter” when NAOS ≥ -0.5, does that not contradict the construction of the FW indices or does the sentence refer to the years that were excluded in the optimization process?

This may have been clarified by answering your preceding question. The summer NAO is linearly correlated with the seasonal SSS change from summer to winter but it has a nonlinear relationship with absolute SSS anomalies in winter.

When the $NAO_S > -0.5$, the seasonal mixed layer is eroded before an absolute cold SST and freshwater anomaly develops. In that case, the seasonal freshening (which is typically confined to a shallow surface layer) is too small to affect the SST because it mixes with deeper and larger volumes of freshwater. In this case, other sources of freshening – particularly the advection of cold and fresh polar water into the subpolar region – are more important. These other sources are filtered out by considering seasonal differences since they have no strong seasonal dependence (when compared to runoff, which has a very strong seasonal dependence).

Thus, there is no contradiction. We just need to use a different freshwater index to describe the freshwater variability in this regime.

We have now removed the sentence to avoid any confusion. Instead, we no simply state (line 329): "*While the full, un-subsampled summer NAO is a suitable indicator of the seasonal freshwater input from summer to winter, it is not necessarily correlated with absolute SSS anomalies in winter. Once a seasonal mixed layer is eroded, the SST and surface salinity are expected to be influenced by other factors, consistent with the nonlinear relationship between the summer NAO and subsequent winter SST anomalies (Fig. 1d).*"

13. l. 354-355: Please discuss the fact that the SST anomalies in Figure 6a are largely not statistically significant. What are the implications for your conclusions?

The SST difference between the warmer subtropical region and the colder subpolar region is highly significantly correlated with both freshwater indices. The significance of absolute SST anomalies does not show at individual grid points because the exact location of the SST front between the subtropical and the subpolar gyre differs across the years and is therefore poorly constrained by the regressions. However, when considering larger areas and spatial differences instead of individual grid points, the increased SST difference between the warm subtropical and the cold subpolar region is very robust.

In the revised manuscript, we explain (line 415):

"[...] The SST signal in both summers after the freshwater anomalies implies an increased SST difference between the warm subtropical gyre and the cold subpolar gyre. The exact location of the SST front between the subtropical gyre and the subpolar gyre can differ between the years included in the subset and is therefore poorly constrained, resulting in reduced significances at individual grid points. However, the resulting increased SST gradient – which is of greater dynamical relevance than absolute SST anomalies – is highly significant. For instance, the SST difference between the region, in which the SST anomalies exceed 2 °C and the region, in which the SST anomaly falls below –2 °C, includes a substantial area of the extra-tropical North Atlantic (Fig. 5a and b) and is significantly correlated with the F_E index with a correlation coefficient well above 0.7 in both summers ($r \approx 0.76$ and 0.84 in the first and second summer respectively), with p-values well below 0.05"

Accordingly, we have updated the figure panels to include the +/- 2 °C lines (now Figure 5a and b).

14. l. 359-361: This sentence is unclear. I don’t understand what you mean by “ the anticyclonic circulation anomaly is in part rotated over the continent”.

In winter, we show that there is an increased SST gradient between the subpolar and the subtropical gyres (Fig. 1e and f). Associated with this SST signal is a more cyclonic atmospheric circulation anomaly over the subpolar region and a more anticyclonic atmospheric circulation anomaly to the south of the SST front (Fig. 4b).

In summer, there is an additional temperature contrast across the coastline because the land typically warms up faster in summer. Thus, in contrast to winter, there is not only an increased SST front over the North Atlantic, but also between the subpolar North Atlantic region and the European continent. Accordingly, we found that the more anti-cyclonic atmospheric circulation anomaly (that was over the subtropical region in winter) is now shifted over the continent.

We agree that the word "rotated" was misleading. Also, we do not show the anti-cyclonic circulation anomaly since it is not needed. We already show the meridional wind velocities. Thus, we have removed the sentence.

15. l. 362: Interestingly, the warm and dry anomalies during the first summer are not necessarily colocated. Please discuss/speculate why the dry anomalies occur more toward the southeast.

Thank you for pointing this out. We attribute the spatial differences to the different drivers of temperature and precipitation anomalies within individual weather systems such as cyclones and anticyclones. However, we believe a more in-depth discussion of the detailed dynamical features would go beyond the scope of the study. Thus, we now state (line 437):

"Considering that precipitation anomalies preferentially occur along trailing cold fronts and are shifted southward relative to cyclone centres (Booth et al., 2018; Kodama et al., 2019), the

observed displacement of the dry anomalies relative to the warm anomalies is expected from their locations within individual weather systems and consistent with other studies (Yu et al., 2023)."

16. l. 364: Is the "cold SST anomaly" in Figure 7a statistically significant other than the very small region around 40 °N, 40 °W? What are the implications for your conclusions?

Please refer to our response to your Comment 13 above. We believe this has now been clarified.

17. l. 367: Please discuss why, after FW freshwater anomalies, the warm anomalies occur over France and Great Britain while the dry anomalies occur more to the northeast over the Baltic region.

Please refer to our response to your Comment 15 above. We believe this has now been clarified.

In addition, we now clearly describe the different regions in the text.

18. Figure 6: The coastlines in panels c and d are hard to make out. This makes it difficult to relate it to the other maps, particularly since each row shows a different domain.

Thank you for pointing this out. Since the figure panels c and d were already quite busy, and we already show the arrows in panels a and b, we have removed the arrows from panels c and d. Thus, the coastline is now easier to see (now Fig. 5c and d).

19. Figures 6 & 7: Does "SST with the 700-hPa winds (…) on FE/FW" mean you performed a multiple linear regression? Please provide more detail in the text.

We regressed the SST and the 700 hPa separately onto the freshwater index. This is now clarified in both figure captions. We also better describe the identified wind field more in the text.

20. l. 385: Please be more precise: Does "cold anomaly over the North Atlantic" refer to the composite of air temperature (Figure 8b) or the SST anomaly in the western subpolar gyre region (Figure 8e)?

We are now more precise and specify "cold SST anomaly".

21. Figure 8: What are the red lines in panel f?

Thank you for spotting that there were lines at the edge of the panel. They referred to positive salinity anomalies. However, since we did not calculate SSS in the associated regions, the lines were misplaced.

We have now removed the lines.

22. l. 400: Please clarify what you mean by "SST variability each summer". Are you referring to the spatial distribution of summer SST anomalies?

We referred to the time variability of the spatial SST pattern. However, this analysis has now been removed to improve the overall clarity and conciseness.

23. l. 403-406: What is the correlation of the SST pattern with the summer NAO? It might be worthwhile adding the NAO time series in Figure 9e to demonstrate the relationship.

Thank you for suggesting this. We have now removed this figure and the associated section.

If we would have calculated the correlation, it would have depended on the underlying period. Over the last two decades, the time series would have been highly correlated. Over the earlier period, they would have been only weakly correlated. Along with this switch, we observe a shift of power from decadal variability towards interannual climate variability both in ocean and atmospheric variables. However, to improve clarity and conciseness, we have shifted this analysis into another manuscript.

24. l. 407-410: To keep the reader onboard, I suggest to add a sentence that links the SST variability back to the freshwater anomalies.

Thank you for suggesting this. We have removed this analysis to improve the overall clarity and conciseness of the manuscript.

25. Figure 9f: Please add a label for the x-axis.

Thank you for spotting this. This figure has now been removed.

26. l. 506: Please specify if you refer to an “increased meridional temperature gradient” in the ocean or atmosphere.

We referred to an increased meridional SST gradient, although we would expect that the increased SST gradient affects the meridional temperature gradient in the lower troposphere.

We have now removed this analysis.

27. Figure A2: Why are the regions enclosing the 95% significance level different from Figure 2b?

Thank you for spotting this. In Figure 2b, we had calculated the significance level of the correlation, while in Figure A2 we had used the significance level from the regression, which is the correct one, since we are showing the regressions. The difference is only minor, however.

We have now corrected the significance lines in Figure 1 (previous Figure 2).

28. l. 580-581: The cold anomaly regions cover the entire subpolar North Atlantic including areas of deep convection. Please discuss the implications and added uncertainty using a spatially constant value for the mean winter mixed layer depth.

Since the average was obtained from Argo floats, which are more often found in regions of shallower mixed layers (because these regions cover a larger area), the average may underestimate the true mixed layer depth, particularly in the regions of deep ocean convection. Using a deeper mixed layer depth in the mass balance equation increases the magnitude of the terms on the lefthand side of Eq. (5). Thus, the uncertainties that would result from using deeper mixed layers would be smaller than the ones we provided. However, using a different mixed layer depth has only small effects on the uncertainty.

To remind the reader that we are using spatial averages, we now state (line 307): *"[...] The uncertainties apply to the cold anomaly regions, enclosed by the 95% lines. Uncertainties at each individual grid point can differ."*

If there are much shallower mixed layer depths compared to the mean mixed layer depth, sampled by the Argo floats, the surface mass balance may underestimate (but not overestimate) the surface freshening. In that case, the stratification must be controlled by salinity and the salinity anomalies even overcompensate the density increase implied by the cold anomalies (to justify that there shallower mixed layers in the regions of cold anomalies).

We now explain this in the text (line 308): *"[...] If the freshwater forcing is very large, the surface mass balance may underestimate the freshening because freshwater anomalies can (in theory) decrease the surface salinity up to a threshold of near zero, while SST anomalies cannot drop below the air temperature. Still, we find that even during the strong observed freshwater anomalies in 2015 and 2016, the surface mass balance provided a good approximation (Appendix A), suggesting that a potential underestimate of the obtained freshwater anomalies is only small."*

IV. Typos/Wording:

I suggest the following changes:

l. 36: “requiring a high grid spacing of ~1/12° “ to “requiring ocean models with high grid spacing of at least ~1/12°”
l. 64: “spatial resolution” to “grid spacing”
l. 84: “run at a resolution of” to “run with grid spacing of” l. 158: “surface temperature” to “ocean surface temperature”
l. 160: “subpolar cold anomalies” to “subpolar SST cold anomalies”
l. 189: Please remove second “warm”.
l. 317: “to north of” to either “north of” or “to the north of”
l. 326: “resolution” to “grid spacing”
l. 327: “resolution” to grid spacing”
l. 372: “subsets” to “subset”
l. 416: “autocorrelations” to “autocorrelation”
l. 417: “point” to “point out”
l. 435: “of SST signal” to “of the SST signal”

We have now integrated all suggestions in the text. Thank you for providing these suggestions!

Again, we sincerely thank the reviewer for their careful and thorough assessment of our manuscript, providing many helpful and constructive suggestions. Your comments have helped us to remove any remaining ambiguities and make the manuscript much clearer.

References:

Ferrari, R., & Wunsch, C. (2009). Ocean circulation kinetic energy: Reservoirs, sources, and sinks. *Annual Review of Fluid Mechanics*, *41*, 253-282.

Gill, A. E. (1982). *Atmosphere-ocean dynamics* (Vol. 30). Academic press.

Griffies, S. M., & Greatbatch, R. J. (2012). Physical processes that impact the evolution of global mean sea level in ocean climate models. *Ocean Modelling*, *51*, 37-72.

Wunsch, C., & Ferrari, R. (2004). Vertical mixing, energy, and the general circulation of the oceans. *Annu. Rev. Fluid Mech.*, *36*, 281-314.

Responses to Reviewer 2

I would like to thank the authors for responding to my points. This has led to significant changes to the paper, many of which are positive. However, it also led to many wholly new analyses being presented that didn't quite address the concerns the last time, and the presentation is still confusing in areas, with either insufficient information or statistical rigor to fully follow the arguments. Therefore, although I still find the ideas being presented here to be highly interesting and exciting, I am left feeling that the paper is not publishable in its current state. In particular, I feel as though it just does not do enough to discuss the uncertainties in the analysis, and has too much certainty in its conclusions. I list my major issues below followed by minor points (which may overlap into the more major ones).

We sincerely thank the reviewer for reviewing our manuscript once more and providing many helpful and constructive comments and suggestions!

Following your comments and suggestions below, we have carried out a major revision, paying particular attention to your questions regarding uncertainties, significance, and the phrasing of the conclusions. There has been some degree of repetition in a few comments due to overlapping concerns. We have tried to avoid repetition in our responses by referring to earlier comments but could not avoid some overlap. We apologise for any inconvenience.

Major points

The discussion on subselecting is still confusing - I think this is because there are two steps that are discussed as one and referred to as "subsampling"? For example, there is the choice to split the record into two, i.e., above and below the value of 0.5 for SNAO, and then again where individual years are removed. I understand the first part of this sub-selection, and it seems warranted given the relationship between SST gradients and summer NAO. But I am still unsure of the second, which seems more focused on getting a higher correlation between the two. I still do not understand the physical basis for this, and am worried that it over emphasizes the potential role of this mechanism. Indeed, the authors say they are not worried about the mechanism, but I'm not sure how they can be so certain. I guess in particular, the logic seems not ideal here, as the authors state they are looking for years where SSTs are being driven by freshwater, but then focus on the relationship with the summer NAO. An obvious question is whether the same years come out if you first look for years where you think the SST anomalies are driven by FW changes? This would go some way to better understand the thinking here.

Thank you for pointing out that the method of subsampling was still confusing. To address your concern, we have now made the following changes:

(1) We have removed the sentence that the indices are used for a purely statistical purpose since it was confusing. Please note that the NAO is, itself, only an index. It is by no means more physical than any subset of it. In the manuscript, we now provide a clearer dynamical motivation for the subsampling (line 184 – 194):

"The challenge in detecting the conditions in which freshwater anomalies may have affected the SST, consists in the complexity of SST and freshwater variability in the subpolar region. In theory, changes in surface freshwater can be influenced by river runoff, sea ice and glacial

melting, evaporation and precipitation, mixing, and ocean currents. Considering that multiple factors can contribute to freshwater variations over a range of timescales and spatial scales, it may not be possible to reduce the complexity of freshwater variability in space and time into a single, one-dimensional index.

To overcome this challenge, we construct indices over subsets of years that allow us to closely constrain the variability of the SST, over the selected subset. Thus, this approach is different to traditional methods in which the dynamical mechanisms are known a priori, and statistical methods are used to assess the significance of these mechanisms. Here, we first select indices with a strong and significant statistical relationship with the SST, and then look for potential freshening mechanisms that can explain the relationship, assuming that these mechanisms exist but may be masked by other drivers."

Subsequently, we find (228): "[The subsampling] represents a powerful method for increasing the statistical relationship between two variables and thus identifying dynamical links, based on the assumption that noise, and other mechanisms, can mask these links. Once a strong statistical connection has been established, the physical basis is assessed by investigating the associated dynamical links with freshwater anomalies."

(2) In Section 4.2, we show that the subsampled index has a higher correlation with the physical causes of freshwater anomalies compared to the un-subsampled index, providing a physical justification (Fig. 3c and d). We have now clarified and shortened this section, stating (line 346):

"To assess the role of the wind stress curl and subpolar gyre circulation for the cold and fresh anomalies associated with higher summer NAO states, we inspect the associated absolute dynamic topography in winter. The full, un-subsampled summer NAO only displays a weak and mostly non-significant relationship with the geostrophic surface circulation in the southwest subpolar region (Fig. 3c). When using the sub-sampled summer NAO corresponding to the F_W *subset, however, the absolute dynamic topography north of 50 °N in winter is significantly reduced, implying a more cyclonic and hence, stronger subpolar gyre circulation in the northwest subpolar region (Fig. 3d). The strengthened relationship between the subsampled summer NAO and the subpolar gyre circulation thus supports the subsampling by providing a physical explanation for the freshwater anomalies associated with* F_W *(Fig. 2b)."*

(3) We now show that the results are not sensitive to the subsampling nor the number of years included (Section 4.5 and Appendix B). In the derivation, we explain (line 237):

"In Section 4.5 and Appendix B, we show that the results are not sensitive to the subsampling or the number of years included. However, having a close relationship between the index and the SST results in reduced uncertainties when estimating the associated freshwater anomalies. In addition, the high correlations help us to identify and assess potential dynamical links more clearly: Freshwater indices that are only poorly correlated with freshwater are only of limited use when assessing links between freshwater and other ocean or atmospheric parameters. Since the indices will be used as a tool for representing freshwater anomalies, high correlations between the indices, the SST and potential freshwater anomalies are a prerequisite, not a conclusion, and we make no assumptions on the suitability of both subsets outside the selected years."

(4) We have followed your suggestion and introduced a new index, which is solely based on the SST and not subsampled (Section 4.5). As with the other two indices, we find that the un-subsampled SST index is significantly correlated with freshwater anomalies in winter and with European summer weather in the subsequent summer (Fig. 7).

An advantage of the SST index is, that is based on all years and does not involve any subsampling. However, by using an un-subsampled SST index, we need to accept a greater loss in explained variance when linking the identified anomalies in European summer weather back to freshwater anomalies. Specifically, the correlation of the index with the freshwater anomalies is reduced, amounting only to up to 0.8 over the subpolar region (Fig. 7b). Thus, a critical reader might argue: "Are the identified summer weather anomalies really linked to freshwater anomalies if the freshwater index can only explain 64% of the variance of the freshwater variability?"

This loss in explained variance can be understood by considering the spatial and temporal complexity of freshwater variations in the subpolar region. The advantage of using the optimised (subsampled) freshwater indices is that they constrain the freshwater variability more closely over selected subsets. In a statistical sense, if we express the variability of European summer weather (y_i) in terms of freshwater (x_i), we need to account for two steps in the linear regression for European summer weather (y):

$y = r_1 \cdot r_2 \cdot x + residual$, where r_1 is the regression of European summer weather on the freshwater index and r_2 is the regression of the freshwater index on the actual freshwater variability.

A smaller correlation between freshwater and the freshwater index reduces the capability of the index to represent freshwater anomalies and link the obtained summer weather anomalies back to the freshwater anomalies. Using subsampled indices, the correlations exceed 0.9 over large parts of the subpolar region (Fig. 2c and d). Thus, the loss of explained variance is smaller, supporting the identified links to freshwater more strongly.

(5) In Sections 3.2 and 4.6 and in Appendix B, we explain the trade-off between the number of years included in the index and its correlation. To address this trade-off, we now provide three freshwater indices with different correlations, different spatial and temporal characteristics, and different numbers of years. In each case, we clearly state the advantages, and disadvantages of the respective index.

Overall, we find: The higher the correlation is that the freshwater index has with the freshwater anomalies (and hence the better constrained the freshwater anomalies are), the stronger is also the relationship of the index with European summer weather (line, Fig. 9).

We agree that having an additional index linked to the SST directly, covering all years, nicely complements the analysis. Thus, we have followed your suggestion and added this index.

Thank you for suggesting this!

Although there has been some attempt to explain the physical relationships - I'm still uneasy about the explanation for the two regimes of stronger SST gradients from only the freshwater fluxes related to the summer NAO. In the response the authors say that "The threshold of ~ -

0.5 in the summer NAO corresponds to a critical surface freshening above which the shallower, seasonal freshwater anomalies are mixed down", but supply no evidence. I take it that this is an assumption? I agree that this could be one interpretation, but another is that the SST gradient is not being driven by the same processes (i.e., freshwater) in the other cases?

Thank you for pointing out that the physical cause was still unclear.

We did not attribute the cold anomalies to the freshwater anomalies, and we have clarified this in the revised version. For the F_W regime, specifically, we identify a stronger subpolar gyre circulation advecting cold and fresh polar water into the region, consistent with earlier studies. Thus, we attribute the cold and fresh anomalies to the subpolar gyre circulation rather than attributing the cold anomalies to the freshwater anomalies.

Being cold and fresh however, it is possible that the freshwater affected the SST prior to entering the subpolar region, in the Arctic. The Arctic is defined as a β-ocean in which stratification is dominated by salinity changes (Stewart and Haine, 2016). Thus, there is a pronounced halocline with a cold fresh layer of polar water overlying a warm, saline layer of Atlantic water (Aagard et al., 1981). If there were no freshwater, the surface water would mix with deeper, warmer water. Thus, one might still argue that the amount of freshwater constrained the vertical mixing and hence, the SST, prior to entering the subpolar region by influencing the T-S characteristics of the polar water. However, we do not make this argument in the text since the connection with the subpolar gyre circulation is the more direct link.

We agree that the statement *"0.5 corresponds to threshold, above seasonal freshwater anomalies are mixed down"* included too much interpretation. Thus, we have removed this sentence.

We now simply state that both NAO regimes are associated with freshwater anomalies (Fig. 2, line). Moreover, for different NAO regimes, different drivers of the freshwater anomalies are important, which is well supported by our findings (Fig. 3, Section 4.2) and earlier studies, both for the F_E subset, which is linked to runoff and seasonal melting (Bamber et al. 2018, Dukhovskoy et al., 2019), and for the F_W subset, which is linked to the subpolar gyre current system (Häkkinen and Rhines 2009; Häkkinen et al., 2011, 2013; Holliday et al., 2020).

I don't fully understand the logic behind the mass balance either, and I worry it is missing key processes (and hence overestimating the role of freshwater). In particular, the authors basically assume that geostrophic currents do not contribute to the SST anomalies (and in the response argue that it is because they are in parallel to density contours). This is obviously the case, but how do you take account of weakened or stronger geostrophic currents and, hence, ocean heat transport convergence anomalies? This may be a bit simple minded, but if the NAC weakens and transports less warm water into the eastern subpolar gyre, but the sea water is being subject to the climatological heat loss, then this will cause a cooling. Another process that isn't taken account for is whether there is a shift in the currents. Confusingly, such a mechanism is discussed in terms of the ocean response to the summer NAO, and is argued to make key changes to temperature, but the mass balance model (which is used to argue that freshwater changes are the key process) doesn't take into account these processes, as far as I can tell.

Thank you for indicating that the mass balance was still unclear.

(1) First, we would like to reassure you that the mass balance considers all physical causes of mass changes. We have added further references using the same equation (e.g., Griffies and Greatbatch, 2012) and associated studies providing a detailed evaluation of the energy sources and forcings that drive ocean currents (Ferrari and Wunsch 2009; Wunsch and Ferrari 2004). These studies agree that, on the timescales and spatial scales we consider, the wind stresses and air-sea fluxes are by far the largest energy source that can drive horizontal and vertical motion and hence, mass fluxes.

(2) Importantly, we analyse the mass budget, not the heat budget. Geostrophic currents cannot contribute to a net mass increase or decrease. Of course, geostrophic currents can, and do, contribute to a net heat convergence. Being geostrophic, however, the mass changes associated with temperature changes must be compensated for by mass changes associated with salinity changes.

Specifically, for a geostrophic flow, the advective term A_{geo} in the mass balance equation can be expressed as:

$$A_{geo} = u_{geo} \cdot \frac{d(\rho \cdot h)}{dx} + v_{geo} \cdot \frac{d(\rho \cdot h)}{dy},$$

where we have integrated the advective term over the surface mixed layer, u_{geo} and v_{geo} are the average zonal and meridional velocities within the surface mixed layer, h is the depth of the surface mixed layer, and x and y are longitude and latitude.

Moreover, the definition for geostrophic flows is: $u_{geo} = -\frac{1}{f \cdot \rho}\frac{d(p)}{dy}$ and $v_{geo} = \frac{1}{f \cdot \rho}\frac{d(p)}{dx}$, where f is the Coriolis parameter and p is pressure. Next, we integrate the hydrostatic balance over the mixed layer: $p = \rho \cdot g \cdot h$. Thus, we obtain for the geostrophic flows: $u_{geo} = -\frac{g}{f \cdot \rho}\frac{d(\rho \cdot h)}{dy}$ and $v_{geo} = \frac{g}{f \cdot \rho}\frac{d(\rho \cdot h)}{dx}$, where g is the gravitational acceleration. Plugging both terms into the expression for A_{geo} above, we find that $A_{geo} = 0$.

To answer your other question:

If the NAC experiences a greater (or lesser) heat loss prior to entering the subpolar region, it will adjust geostrophically. It cannot contribute to a net mass change. The geostrophic adjustment time is 1/f, which is approximately 2 days in the subpolar region, well below the winter averages we are using.

We have clarified the mass budget and changed the nomenclature to make it more consistent with earlier studies. Also, we better explain the individual terms (Section 3.1).

Geostrophic flows constitute by far the largest part of fluid motion over the open ocean, both as eddies and as the subpolar gyre circulation. However, we clearly state now in the main manuscript that geostrophic flows cannot contribute to the mass budget (line 276 – 284).

We have also included references investigating the mass budget (Griffies and Greatbatch, 2012) and potential sources of energy and hence mass fluxes (Ferrari and Wunsch, 2009; Wunsch and Ferrari. 2004), clarifying that the mass equation is well established and includes all relevant terms. These studies find that the winds and air sea fluxes are by far the strongest forcing for fluid motion. Additional external sources of energy that can drive fluid motion include biological activity, tides, geothermal activity, and others, which are negligible on the timescales and spatial scales considered.

In conclusion, ageostrophic motion (including vertical mixing) requires an energy source (for instance to mix denser water upward against gravity). Over the open ocean, the necessary energy can only be provided by air-sea fluxes and wind stresses, neither of which is correlated with the freshwater indices (Section 4.1, Appendix A). Their magnitudes are much too small to be connected to the SST anomalies and their spatial characteristics do not agree with those of the identified SST anomalies (line 285 – 292).

I still remain confused about the proposed mechanism. Moreover, in order to explain how freshwater affects summer weather, the authors invoke changes in ocean circulation (to explain Fe) and wind-driven changes in heatfluxes and Ekman upwelling. They do this using a very small sample (next point) to produce a range of statistical links, but still appear to conclude that it is freshwater anomalies that drive everything with little discussion of the uncertainties inherent to a coupled system (note that the simulations used still do not test even the specific hypothesis that subpolar SSTs are driving European summer temperatures as opposed to SSTs or external forcing more generally). Therefore, I feel the authors need to do a much better job in reflecting the uncertainties in their conclusions. Ultimately, the way this is written is that the authors seem to think that the freshwater (or really the summer NAO) drives everything, but this seems a bit extreme to me based on the evidence. I would expect a significant amount of discussion time on the limitations of the method and on the exact role of freshwater anomalies (e.g., are the responsible for everything (as presented here), or could they be an important and not fully appreciated feedback?)

Thank you for indicating that the language was still too definite, and for suggesting adding a more comprehensive discussion about the involved uncertainties.

First, we would like to point out that we do not state in the manuscript that the freshwater anomalies or the summer NAO drive everything. We are very cautious about the wording in that we say: *"the NAOs indices are associated with freshwater anomalies"* or *"the cold anomalies are associated with freshwater anomalies"*. We do not use active verbs like *"Freshwater anomalies drive the cold anomalies" or "Freshwater anomalies drive European summer weather"*.

We summarise your comment stating that there are three sources of uncertainties: (1) those involved in the freshwater estimates, (2) those involved in the statistical analyses, (3) those involved in linking the statistical connections to dynamical mechanisms. In the revised manuscript, we have clarified these sources of uncertainty and included additional significance analyses. Below, we discuss all three sources.

(1) First, we provide the uncertainty estimates of the freshwater anomalies, which are only small, amounting to 4% and 6% (Section 4.1). This is fully acceptable for the purpose of the analysis. We now do the same for the new, un-subsampled freshwater index (Section 4.5, Appendix A). Also, we point out that these uncertainties are much smaller compared to other, publicly available salinity products.

(2) We demonstrate the links based on the statistical analyses, using three different methods. Specifically, we use composites, correlations/regressions, and a coherence analysis, all of which provide statistically significant results.

(3) Moreover, in the case of the correlation/regression analyses, we use three different indices: F_E, F_W, ΔSST (Section 4.5), which have different characteristics in terms of autocorrelations, number of years included, and correlations with the freshwater anomalies. Yet, all four indices show statistically significant relationships with freshwater and European summer weather. The significance estimates are based on standard Student t-tests, which do account for the numbers of degrees of freedom (Section 4.5).

(4) We have added an Appendix B showing that the results are not sensitive to any choices that were made in the derivation, like adding or removing individual years, or excluding all consecutive years, or lowpass filtering European summer weather and adjusting the numbers of degrees of freedom for the resulting, increased autocorrelations.

(5) Regarding the link between the statistical correlations and the proposed dynamical mechanisms, we have followed your suggestions below, and removed analyses, paragraphs and sentences that were misleading. In each of the sections, we use words like *"associated with"* or *"linked to"*, when referring to either the freshwater anomalies or the freshwater indices, and we have rephrased the conclusions. We only use active verbs when referring to mechanisms that are shown and well-supported by theory or earlier studies (like *"Ekman transports being driven by the winds" or "surface pressure gradients driving geostrophic currents"*) but we do not use active verbs in connection with freshwater anomalies.

Moreover, when showing the air-sea feedbacks in the revised version, we explicitly state (line 404): *"By being highly correlated with the SST anomalies, the freshwater indices serve as valuable tools for visualising the associated ocean and atmospheric circulations, reinforcing each other (Figs. 1 and 4). However, we do not causally attribute the SST pattern to freshwater anomalies, and we do not infer that the freshwater anomalies act as a trigger for the characteristic tripole SST pattern."*

In addition, we clearly separate interpretation from results. Thus, we only discuss a potential coherent mechanism in the conclusions, where we conclude (line 564): *"This study identified statistically significant links and thus indicates an enhanced predictability of European summer weather arising from freshwater anomalies in the North Atlantic, without attributing the variability of European summer weather to freshwater anomalies as a mechanical trigger."*

We have also gone over all the formulations again and removed anything that could lead to misunderstandings. If there is anything you still find misleading, it would be great if you could let us know the specific sentences.

I'm still very worried about the robustness of the results in a statistical sense. The regressions for Fw are based on, as far as I can tell, 9 sample years(!). Moreover, even if the authors argue that the auto-correlation is small (it doesn't look that small over the 1980-2020 period), most of the years in their samples are not wholly independent (e.g., they come in clusters), and occur on a background of significant decadal change in atmospheric circulation and European temperatures, which both may be driven by external forcing). There is no attempt to address the particular uncertainty of small samples in the whole paper, which begs the question, how robust are the results regarding European Weather to further sub-sampling? Given the short time-series, I would at least expect some sort of jackknife resampling (i.e., leaving out 1 year at a time), but I would strongly recommend the authors to also compute figures 6 and 7 using non-consecutive years.

We thank the reviewer for encouraging us to add more information on the significance of the results and to add more analyses assessing the robustness of the results. We note there is some overlap with your preceding comments, so we only provide a short summary of the main changes:

(1) First, we are using three different methods (correlations/regressions, composites, coherence estimates), all of which provide statistically significant results.

(2) Using the correlations (which is the only method you criticise), we use three different indices that are characterised by different correlations with the freshwater anomalies and based on different sets of years. The new ΔSST index, specifically, includes 44 years. The F_W and F_E subsets (which are the two indices you are referring to), are based on only 17 and 8 years respectively. Yet, they are both characterised by exceptionally high correlations, compensating for the reduced number of degrees of freedom in the significance estimates.

Importantly, the significance tests we are using (Student t-tests), do account for the low numbers of degrees of freedom in all analyses.

For each correlation that is mentioned in the text, we provide the corresponding p-values. These p-values assess the probability that the high correlations are obtained by chance. In most cases, the p-values are well below 0.05. Thus, the probability that the links are obtained by chance is well below 5%. We also show the corresponding data points in the provided scatter diagrams to rule out that the high correlations are achieved by outliers or clusters.

(3) We consider the autocorrelations in all significance tests: Thus, we now show that autocorrelation of the summer NAO is below the e-folding correlation at one year lag (Fig. 8a). The autocorrelations of the subsets are even smaller. For instance, the autocorrelation of the F_E index at a lag of one "index year" is ~0.07, implying that subsequent freshwater anomalies are different from each other in a rigorous statistical sense, regardless of whether these anomalies are consecutive or not. In addition, we show the autocorrelations of European summer weather (Fig. 8b), which are more relevant and reflect high interannual variability (see also Figure 10a).

Despite the autocorrelations being only small at one year lag, we now show that the results are not sensitive to excluding anomalies in consecutive years (Appendix B). The results remain significant, which is expected since the scatter diagram (Fig. 1d) shows that there are no clusters of point responsible for the high correlations. The data points are evenly spread over the range of each freshwater index.

The autocorrelation of the new ΔSST index is higher, reflecting the low-frequency variability in the causes of the SST and freshwater anomalies. Yet, this study is focused on the subsequent links with European summer weather, for which the autocorrelations are small, suggesting that high-frequency variability contributes to the significance of the relationship.

To be sure that the increased autocorrelation of the ΔSST index does not affect the significance, we lowpass filtered the European summer weather to match the increased autocorrelation of the ΔSST index. After correcting for the number of degrees of freedom using $N^* = N \cdot \frac{\Delta t}{2T_e} - 2$, where N refers to the number of data points, Δt refers to the time interval between them, and T_e is the e-folding timescale of the autocorrelation (Leith, 1973), we still obtain significant relationships (Fig. B8).

(4) Lastly, we have added a multi-taper coherence analysis (Section 4.5), showing that the relationship between freshwater anomalies and the subsequent European summer weather is significant on timescales from years to decades (Fig. 8c and d). Thus, the coherence analysis also indicates that interannual variability is important.

Following your suggestion, we find that randomly selecting 8 summers or winters does not lead to substantially higher or lower correlations. We tested this with 1000 iterations for both summer and winter (Fig. 1 below).

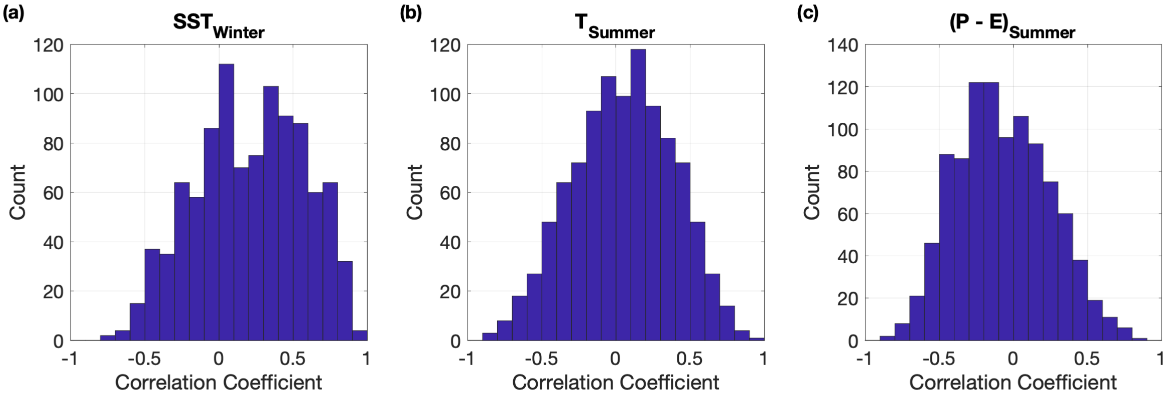

Fig. 1: Histograms for the correlation between the summer NAO and (a) the SST in the subpolar region in the subsequent winter, and (b,c) the 2-m air temperature and precipitation over Europe in the subsequent summer, obtained by randomly selecting 8 years, using 1000 test cases. We used boxes as regions, extending from 50 °N to 60 °N and 20 °W to 60 °W for the SST and from 30 °N to 60 °N and 10 °W to 30 °W for the European summer weather. However, the results are not sensitive to these choices.

However, we think that it not necessary to add this analysis of the 1000 random test cases in the manuscript. Importantly, the selection of years with a high correlation between freshwater anomalies and their index has no implications for the statistical significance of the relationships between the freshwater index and any other variable that is independent of freshwater. If there were no statistical links between the freshwater anomaly and European summer weather, the selection of years would be purely random for European summer weather.

In a mathematical sense, the definition of independence is P(A|B) = P(A), where P(A|B) is the probability of A conditioned B (e.g. Evans and Rosenthal, 2004). If there were no statistical links between European summer weather ("A") and the freshwater anomalies ("B"), then the probability to obtain a specific European summer weather anomaly would be unchanged by the subsampling. Thus, the validity of the Student t-tests remains unaffected by the subsampling.

(5) In the main text, we have now clarified that the selection of years with a high correlation between freshwater events and the freshwater index does not affect the significance of a correlation between the index and any other variable that is independent of freshwater events. If there would be no statistical connection between freshwater and any the proposed influences, the probability for randomly obtaining significant statistical connections by chance remains the same (line 457 – 460).

Minor points

Line 166 - should be figure 1c? Relatedly, I would recommend to move figure 1c to 1a (e.g., swap the time-series with the spatial maps). This seems more logical.

Thank you for suggesting this. We have followed your suggestion, removed the sentence with the figure reference, and rearranged the subplots (Fig. 1).

The time series is now shown before the regression maps on F_E and F_W. We have also added two more panels at the top to visualise additional explanations provided in the text.

Line 190 - I had not realized previously that you use a different spatial area to define your SST gradient. This seems slightly odd in that you may be picking up different processes (and related to my previous worries about not understanding why there is such a threshold in the SNAO SST regression). How sensitive is the SST gradient, and hence the correlations shown in figure 1d, to using fixed boxes for SST?

Thank you for pointing this out.

Using fixed boxes (for instance between 30 °N and 45 °N and between 45 °N and 60 °N) leads qualitatively to the same relationship when comparing the SST anomalies with the NAO but the correlations decreases since we are also including regions that are not statistically significant. Here, we used the regions in which the relationship is significant to ensure that the significance is not due to outliers or clusters (line 217).

Using slightly different regions is also motivated by the different characteristics of the two types of freshwater anomalies. Still both regions are highly correlated with each other. Thus, the correlation between the two, resulting ΔSST timeseries is $r \approx 0.96$ (with $p \approx 10^{-23}$), which is now stated in the manuscript (line 471).

We now explain the choice of the regions more clearly. The entire section has been rewritten to provide a clearer motivation of the regions to account for the complexity of freshwater variations in the subpolar region (Section 3.2).

Line 195 - Better to be specific - you mean for values above the -0.5 threshold? The process of "optimizing" the regression slope is very unclear - how do you do this? Did you just resample by randomly picking 17 years, and then pick the highest value? What is the physical basis for this? You say that results are not sensitive to this choice, which may be true, but then why "optimize" the correlation to 0.9 (from 0.68)? I think it would be good to show the sensitivity more clearly in the appendix.

Thank you for pointing out that this was still unclear.

We have now rewritten this section and clearly explain each step in the method (Section 3.2). Regarding the selection, specifically, we have added the following paragraphs (line 209 – 243):

"Next, we strengthen the identified relationships between the two NAO subsets and subsequent SST anomaly through subsampling. The subsampling is motivated by the objective of achieving a near-linear relationship between the subsampled NAO$_S$ *index and the subsequent SST anomaly. Specifically, if* x_i *correspond to the NAO subset years, and* y_i *correspond to the SST anomaly, we strive to derive a linear relationship* $y = ax + b$*, where* a *and* b *are constants and in which* $|a|$ *is high. The higher the magnitude of* a *is, the higher is the magnitude of* αT *on the lefthand side of Eq. (5) after regressing Eq. (5) onto the index. Thus, we aim to select NAO years, for which the magnitude of the slope* $a = \frac{y_i - y}{x - x_0}$ *is large, where* $x_0 = x|_{y=\bar{y}_i}$ *and* y_i *corresponds to the mean over* y_i*. At the same time, we strive to obtain a high correlation between the subset and the subsequent SST anomalies. Thus, we aim to select NAO years where* $(x_i - x_0)^2$ *is large, since this increases the variance of the index and SST anomalies.*

[...] Following these objectives, we maximise the slope and the variance of the subsampled index by selecting the N years where the term $(y - y_i) \cdot (x - x_0)$ *is highest. [...] Graphically, the subsampling is equivalent to increasing the slope of the regression line while keeping a high variance (Fig. 1d). It represents a powerful method for increasing the statistical relationship between two variables and thus identifying dynamical links, based on the assumption that noise, and other mechanisms, can mask these links."*

Further details are provided in Section 3.2 Following your suggestion, we now also show the sensitivity of the results to the subsampling in Appendix B.

Overall, we find: There is a trade-off between the numbers of degrees of freedom and the magnitude of the correlations. A higher number of years included implies an increased variance, reducing the correlations. However, the significance is unaffected because the reduced correlations are compensated for by a higher number of degrees of freedom.

In our responses to your preceding comments, we explain that higher correlations between the index and the freshwater anomalies support the subsequently identified links with European summer weather more strongly. Thus, we motivate the subsampling more clearly. Still, we also realise that a lot of space and time is now spent on the derivation of the indices. Since we show in Appendix B that the subsampling is not strictly needed (it strengthens the relationships but the conclusions remain the same without subsampling), we would be open to removing the subsampling from the manuscript for the sake of clarity if needed.

Line 280 - its not clear to me that figure 4c shows an intensification of the subpolar gyre - if anything, it is a weakening, in the mean (lower ADT in NAC region means weaker NAC, and higher ADT in Eastern subpolar gyre indicates weaker flow?)

Yes, you are right. This sentence was misleading. We have removed it.

Line 283 - what do you mean by subsampling in this case? When you say ADT on Fw, do you really mean it is the ADT when you only use sNAO value > -0.5. Is the field optimized in the same way as the delta SST?

Thank you for pointing out that this was unclear.

The F_W index is the same F_W index as the one used for the freshwater anomalies (Fig. 2b and d) and the SST anomalies (Fig. 1d and f). There would be no physical reason to show the correlation between $NAO_S > -0.5$ and the ADT since we are only using the F_W index. This analysis is meant to show that the correlations with the potential drivers of freshwater anomalies (in this case the subpolar gyre circulation) also increases after the subsampling.

We have rephrased the paragraph as follows (line 346): "*To assess the role of the wind stress curl and subpolar gyre circulation for the cold and fresh anomalies associated with higher summer NAO states, we inspect the associated absolute dynamic topography in winter. The full, un-subsampled summer NAO only displays a weak and mostly non-significant relationship with the geostrophic surface circulation in the southwest subpolar region (Fig. 3c). When using the sub-sampled summer NAO corresponding to the* F_W *subset, however, the absolute dynamic topography north of 50 °N in winter is significantly reduced, implying a more cyclonic and hence, stronger subpolar gyre circulation in the northwest subpolar region (Fig. 3d). The strengthened relationship between the subsampled summer NAO and the subpolar gyre circulation therefore supports the subsampling by providing a physical explanation for the associated freshwater anomalies (Fig. 2b).*"

Paragraph from line 299 - I found it very confusing to understand what anomalies were being explained by what. It starts by talking about sNAO anomalies >-0.5, but then talks about both Fe and Fw events. This part implies that ocean circulation changes control Fw events, however, this bring up two questions. Firstly, what sort of circulation anomalies are we talking about? In particular, previous work had talked about shifts in the size and location of the subpolar gyre (e.g., Hatun et al, 2005). Is this the sort of circulation anomaly that the authors are thinking of?

Thank you for pointing out that this was unclear. Indeed, we refer to the subpolar gyre circulation. In line with the study by Hatun et al. (2005), we find that the subpolar gyre circulation is intensified and more confined to the north.

Overall, we find that the study by Hatun et al (2005) nicely supports our results. Thank you also for providing the reference. We are referencing it now in the manuscript.

We have now fully removed the paragraph that you referenced in your comment since we agree that it included too much interpretation.

Instead, we explain more clearly the role of the F_W index in linking the identified freshwater anomalies with the subpolar circulation. The F_W index is highly correlated with the freshwater anomaly (Fig. 2d) and with the subpolar gyre circulation in the north-western subpolar region (Fig. 3d), which advects more cold and fresh polar water into the subpolar region and reduces the import of warm, saline subtropical water (e.g. Häkkinen and Rhines 2009; Häkkinen et al. 2011, 2013; Holliday et al., 2020). This is also in line with the study by Hatun et al. 2005.

In addition, we provide a more precise description of the location by stating: "*When using the sub-sampled summer NAO corresponding to the* F_W *subset, the absolute dynamic topography north of 50 °N in winter is significantly reduced, implying a more cyclonic and hence, stronger subpolar gyre circulation in the northwest subpolar region (Fig. 3d).* "

Second, the argument that circulation changes are important for Fw seems at odds with the argument that advection doesn't cause the SST anomalies in the derivation of the freshwater signal, where the authors say geostrophic circulation is not important?

Geostrophic currents cannot contribute to mass budget, but they substantially contribute to the heat and freshwater budgets and anomalies. The SST and freshwater anomalies balance each other in their effect on density (now explained in line 276).

We think has now been clarified, following our response to your earlier comments.

Line 358 - I think this is written with far too much certainty (the SST fronts destabilize the overlying atmosphere, resulting in an enhanced jet stream), and is presented as though it has been proven within this study. I take the point that this is what might be expected from theory, but a regression onto a short time series is not proof. Indeed, the previous section has just been highlighting how wind anomalies can drive SST gradients. Therefore, I would suggest wording here (and elsewhere) that better reflects the uncertainties. E.g., the enhanced jet along the SST front is consistent with…. Ultimately, if I have understood the analysis correctly - this regression is based on 8 years/events - this is an incredibly small sample.

Thank you for indicating that this sentence was written with too much certainty. We agree with your suggestion.

We have now removed the sentence. We have also carefully gone through the manuscript and rephrased all potentially contentious statements.

We still find that the link between the SST and atmospheric circulation is significant in a statistical sense. We note that we identify the same relationships for all indices and the composite (Figs. 5, 6, 7 and 10). Thus, the link is not just based on 8 years.

Still, we do not attribute the atmospheric circulation to the SST, and we do not state the freshwater anomalies triggered the SST pattern. We have also clarified this in the conclusions.

Figure 6 (and other relevant ones) - its not clear what the time period used for this regression - Previously I would have thought it from 1979 onwards using the 8 events - but, now with figure 9, I'm not sure…

Thank you for indicating that this was unclear. All indices are derived from the period 1979 to 2022.

To avoid confusion, we have removed the specific analyses 4.6 and 4.7 that you found unclear regarding the underlying period. Thus, we no longer use the extended period since 1950. We agree that this makes the study clearer and more concise.

On the subject of figure 6, please can you say more about the significance test, please? What is the test, specifically, and how have you ensured that it is robust to a small sample? Also, how do you take account of the fact that many of your years are clustered together? Maybe a jackknife resampling would be appropriate here to test the sensitivity of your results to omitting years from your analysis.

Following your major comments above, we have added a section on significance and robustness (Section 4.5) and an Appendix B, showing that the conclusions to not change after adding or removing years, excluding all consecutive years, and applying no subsampling.

We also explain that we are using standard Student t-tests to assess the significance. As the autocorrelations show, moreover, there are also no high autocorrelations, neither for the NAO_S index, nor for European summer weather. There are also no clusters or outliers responsible for the high correlations. Still, we show that the results remain significant if consecutive anomalies are excluded (Appendix B).

The Figure you refer to is now Figure 5, and the associated Figure that excludes all consecutive events is Figure B2. Both figures look very similar.

Please also see our responses to your earlier comments regarding significance and jackknife sampling.

Section 4.5 - I didn't really understand what this section was trying to say, other than it took a different analysis approach - e.g., composite. However, I found the analysis a bit confusing in the sense that freshwater anomalies are argued to be important, but the SSS anomalies do not look like the SSTs in this case?

Thank you for pointing out that this was unclear. The locations of the SSS and SST anomalies are different because the SSS anomalies are shown in winter whereas the SST anomalies are shown in summer. In winter, the SST and SSS anomalies look very similar (Fig. A4).

We now motivate the section more clearly. Specifically, we state that it assesses the role of freshwater anomalies as a predictor for Europe's warmest summer (line 256) and investigate the extent to which enhanced freshwater anomalies are not only a sufficient but also a necessary condition for warmer European summers (line 533).

Another advantage of this section is that it is based on a temperature index rather than an SST or freshwater index. Showing that the links remain significant regardless of the method or index that is used, increases the robustness of the identified links (line 541).

Since you were previously concerned about autocorrelations, another advantage of this approach is that it starts with a time series that has very low autocorrelations, reflected in a high interannual variability (Fig. 10a).

Figure 8 - The climate pattern seen here looks just like the response to the summer NAO… what is the correlation between your summer NAO index and the Tsummer shown in 8a?

The correlation is $r \approx 0.25$ and it is not significant ($p \approx 0.10$). There is also no reason for expecting the T_{Summer} timeseries to be correlated with the summer NAO. In this section, we link T_{Summer} to freshwater anomalies in the preceding winter, which have a nonlinear relationship with the summer NAO (Fig. 1d). Moreover, the summer NAO index, on which the freshwater indices were based, already occurs in the preceding year.

Section 4.6 - I found this section very confusing, and it wasn't clear to me what was being argued - in particular, I thought the authors had argued in their response that this analysis was was based on the interannual variability due to the use of the NAO, but then present an analysis of very long timescale changes. It also raises the question of what is the time periods are being used in all the other analyses in the paper. If this is not a key aspect of the story of the paper, then I suggest removing it - there is already enough in this paper.

Thank you for suggesting this. We had added the analysis since a previous reviewer was asking for it. However, we agree that it might change the shift the focus, and it makes the manuscript less concise.

We have now fully removed this analysis to improve the overall clarity and conciseness of the manuscript.

Section 4.7 - over what time-period do you compute the relationships?

It was computed over the period 1979 to 2022. However, this section has been removed since it was not necessary and mostly led to confusion.

Line 423 - do you mean figure 10?

Indeed, thank you. However, we have removed this figure now.

Line 443 - I don’t understand what you mean by “projet the SST each summer onto the observed SST pattern - please elaborate

We define the SST pattern as a mode of variability. This is the same method as in EOF analysis, only that we predefined the pattern based on its link with the freshwater anomalies and then looked at its time variability. Thank you for indicating that this was unclear.

We have now fully removed this analysis since we found that it was not strictly necessary for understanding the results. Instead, it mostly led to confusion.

Line 456 - the model simulations still don’t just test the impact of subpolar SST, but include all SST anomalies and external forcings. So I really do not see how you can attribute the signals to the subpolar SST anomalies.

The SST pattern is most pronounced in the subpolar region where we have linked it to a freshwater anomaly. The time variability of this pattern and the lag of one year has also been linked to freshwater anomalies and variations in the supolar region. In the preceding analysis, moreover, we showed that the remainder of the large-scale SST field could fully be explained by atmospheric feedbacks.

Despite the agreement of the temporal and spatial characteristics of this pattern with the freshwater variability – and despite the absence of any other possible physical mechanisms that can initiate this pattern (for instance in the tropics or stratosphere or random fluctuations) – we only stated that this pattern is statistically linked to freshwater anomalies, using the phrase "SST pattern linked to freshwater anomalies". Thus, we explained there is a significant statistical link to freshwater anomalies in the preceding winter and we provided the associated correlations. We did not state that freshwater triggered this pattern, and we did not attribute the SST pattern to freshwater.

Since the analysis was not needed, and to improve clarity and conciseness, we have now fully removed this analysis.

Section 4.8 - Over what time period is the R^2 computed? If this is based on just the 9 points shown in figure 2? How robust is this relationship to jackknife resampling? What happens if you remove all consecutive years?

Please also see our responses to your earlier comments.

Again, we sincerely thank the reviewer for providing so many detailed, helpful, and constructive comments and suggestions. We are particularly grateful for the suggestion to add the un-sampled SST index to the analysis. Your comments have helped us to clarify and improve this study!

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