

Weather Clim. Dynam. Discuss., referee comment RC1  
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## Comment on wcd-2021-79

Anonymous Referee #1

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Referee comment on "European summer weather linked to North Atlantic freshwater events in preceding years" by Marilena Oltmanns et al., Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2021-79-RC1>, 2022

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North Atlantic freshwater events influence European weather in subsequent summers, by Oltmanns et al

This manuscript examines the impact of summertime NAO events on European weather in subsequent summers. The paper is framed as being the impacts of North Atlantic freshwater events on European weather but the index that is used to depict these "freshwater events" is actually the summer NAO. The paper demonstrates some interesting connections between the summer NAO and the following summer weather. At this point, I'm unsure as to whether this manuscript is acceptable for publication. I have a number of comments on the analysis as outlined below. Overall, I'm giving a recommendation of major revisions to allow the authors to respond to these. My major concerns are that the direct link between the summer NAO and the freshwater events is unclear to me. This may be because I'm not an oceanographer and I haven't read the authors previous papers, so I hope that one of the other reviewers will be able to assess this aspect. I have some other concerns about the statistical methods used and the choices made for the scatter plots as outlined in my comments below.

General comments:

(1) The link to freshwater anomalies and the role of low frequency North Atlantic ocean variability. I am not an oceanographer, so I hope that one of the other reviewers will have the expertise to comment on this. The link between the summer NAO index and the freshwater anomalies was a bit lost on me. My understanding of lines 84-92 is that the authors are assuming that the temperature anomalies associated with the summer NAO are due to freshwater anomalies because they find that the cooling is not strongly related to surface fluxes, wind driven Ekman transports, Ekman pumping and re-emergence of SST anomalies from previous years, so by a process of elimination they conclude that it's freshwater anomalies. But I don't see how the role for other ocean circulation anomalies such as the AMOC or advective heat convergence due to circulation anomalies produced by things other than the wind driven Ekman transports has been eliminated. The Atlantic

ocean circulation exhibits variability on long timescales which can be a driver of the NAO and vice-versa (e.g., Zhang et al 2019, Review of Geophysics, 10.1029/2019RG000644 and references therein). It's not clear to me (a) whether it can really be concluded that the SST anomalies are related to freshwater inputs and (b) whether such low frequency variability in the ocean circulation has been appropriately taken into account. The NAO index is being described as a "freshwater index" (199) but I'm not sure how appropriate this is and I'm not sure that much would be lost by instead referring to it as the NAO index and focussing on the impact of the summertime NAO on the climate in subsequent years.

(2) Detrending: It's stated at line 53-55 that regionally averaged trends were subtracted from the air temperatures to remove the greenhouse gas effects. It doesn't really seem appropriate to me to remove the linear trend from one field but not others. The NAO index that is used clearly has a linear trend in it (Figure 1a). I'd suggest detrending everything or detrending nothing. I'm not arguing that the NAO trend seen in Figure 1 is greenhouse gas forced or that this trend should necessarily be removed, but it just doesn't seem appropriate to me to remove the trend in one field and not in the others. Is the detrending also done on the SSTs? It doesn't make much sense to me to remove the trend from the surface air temperature but not the SSTs.

(3) For the scatter plots, the regions where the correlation is significant at the 95% level is used for the spatial averaging. This seems like cherry picking to me. Of course the correlations look good because you've chosen them to be that way. It would make more sense to choose a physically motivated region or it would seem to be less cherry picking if a regular spatial region such as a rectangle were chosen. The result is that in Figure 2b there is a correlation of 0.98, which seems quite unbelievable to me, but maybe it isn't if you are just averaging over regions where the correlation is high.

(4) Has autocorrelation been accounted for when calculating the significance levels? If not, I think it should be. Clearly each year is not independent and there is some low frequency variability and autocorrelation, as apparent in the NAO index (Figure 2a).

(5) It is argued that this work reveals new potential to enhance the predictability of European summer weather, but I think for the impacts on European summer weather the results have only been presented in the form of regression coefficients. To make this more relevant for predictability, it might be worth showing the variance explained.

Comments by line number:

Figure 2: It seems like it would be interesting to have the regression maps for F\_C as well as F\_M. You use the regions based on the regression onto F\_M for both F\_M and F\_C, so it would be good to see whether the regression map for F\_C has a similar spatial pattern to that for F\_M or not.

Figure 2 caption: F\_M and F\_C are only defined in this figure caption. Given their central importance, I think they should also be defined in the text. Furthermore, it would be worthwhile making clear the motivations for this naming convention. It's not very intuitive where the choice of "F\_M" and F\_C" comes from and I think it would help readers to follow if you make that clear. In the end, I realized that this corresponds to "melt-driven" and "circulation-driven" events and I'm overall just very confused about how this distinction can be made just on the basis of the NAO index, which relates to my general comments above. I think this needs to be made clearer throughout the manuscript.

Figure 3 caption: Maybe explain a bit more what the "absolute dynamic topography" is. Is it just sea surface height?

l138: "expansion of the cold anomaly" - perhaps be clear about what this "expansion" is relative to? Is it relative to the previous summer?

Section 4.5: It might be worth making it clear at the beginning here that this is now back to looking at the observations, since in the previous section the focus was on model simulations.

Figure 11 caption: The referencing to the panel labels is messed up in the caption.

Typo's/wording suggestions:

l62: "this index" --> "the NAO index"

l68: suggest "smaller values" --> "more negative values" because the magnitude of the NAO index isn't smaller.

l84: "Fig. 2d" --> "Fig. 2c" (I think d is showing salinity, not temperature)

Figure 4 caption: "The thick contours show the 95% confidence levels" --> "The thick contours encompass regions that are significant at the 95% confidence levels"

l158: "SST-forced" is a bit unclear. Suggest "Simulations performed with prescribed observation-based SSTs".