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Reply on RC1

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Author comment on "Interannual to decadal sea level variability in the subpolar North Atlantic: the role of propagating signals" by Denis L. Volkov et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-354-AC1, 2022

We thank Dr. Fox for his time to review our paper, constructive comments, and for attention to details, in particular for finding errors in the equations. We have tried to address all concerns raised by Dr. Fox, and our answers to his comments are shown below in bold font.

Specific comments
Introduction and conclusions
Subpolar Gyre Index (SPGI)?
There exists extensive literature on the SPGI, which is often defined similarly, but not identically, to the EOFs used here. Generally, the SPGI had settled on the use of the first EOF (EOF1) of SSH variability (including the long-term trend I think) over the subtropical and subpolar gyres (about 30 to 65 N, contrasting to the larger area north of the equator up to 70N used here). Hatun and Chafik (2018, https://onlinelibrary.wiley.com/doi/abs/10.1029/2018JC014101), show that more recently EOF1 in this region has become dominated by the largescale linear trend in the SSH, with subpolar gyre variability now in EOF2 (or a combination of EOF1 and EOF2). Superficially the first two EOFs in Hatun and Chafik appear similar to those presented here, but the time series in the principal components appear different. I think, given the focus on the SPNA, the present study needs more reference to the existing SPGI literature, highlighting the differences and similarities between 'classic' SPGI and the metrics used here, in both the introduction and in discussion. Does the study, for example, show that a single EOF is insufficient or inappropriate to characterise the major, propagating, variability in the SPNA?

Reply: This is an important remark, in response to which we have included relevant information to the Discussion and Conclusions (3rd paragraph):
"… ***It is necessary to mention that in order to describe the variability of SSH and ocean circulation in the SPNA, several authors have also used the 'subpolar gyre index', which, like the tripole, is based on an EOF decomposition of SSH fields (Häkkinen & Rhines, 2004; Hátún et al., 2005; Berx & Payne, 2017; Foukal & Lozier, 2017). The main difference between the subpolar gyre index and the tripole is that the global mean sea level is not subtracted from SSH fields prior to the computation of the former. Therefore, the subpolar gyre index exhibits a trend characteristic of the global mean sea level rise. The regional dynamic changes are then represented by higher modes, which led Hátún & Chafik (2018) to justly conclude that PC2 in their calculation is a better metric for a gyre index***

***than PC1. Our results imply that with the signal propagation, if the global mean sea level is not subtracted prior to the EOF analysis, even the third mode needs to be considered*."**

Section 4. Results
I was occasionally confused about whether the authors were talking about their work in the current paper or previous published work by other authors. Examples are paragraph 1 of both sections 4.1 and 4.2. I think this may be due to a tendency to switch between describing results and figures in the past and present tenses.

Reply: Thank you for noting this issue. In the revised manuscript, we have modified those paragraphs. We hope that the current version better separates our work from the work done in the past.

Section 4.1, lines 236-245, Figure 5. As well as the west-east differences in variance explained by EOF1/EOF2, there are differences based on water depth, with EOF1 explaining the largest part of the variance in waters over 2000m deep (in the west) and EOF2 explaining more variance in waters less than 2000 m deep. What are the reasons for these differences? Are the warming/cooling signals associated with the SSH changes propagating in depth as well as space?

Reply: The likely reason is advection. We cannot say whether there is propagation in depth, but it appears that SSH anomalies first appear within the flows along the flanks of Reykjanes Ridge and along the East Greenland Current and then reach the deeper parts of the Irminger Basin and Labrador Sea. So, this might be a horizontal signal propagation due to eddies generated by those currents. In Section 5 (Discussion and Conclusions) of the revised manuscript, we have added the following sentences, which hopefully provide some discussion of the questions posed by the reviewer: "*It appears that while the overall propagation is westward, SSH anomalies associated with EOF1 and EOF2 first spread over the shallower areas in the east-northeast, including the currents along the eastern and western flanks of the Reykjanes Ridge and the East Greenland Current (EOF2; Figs. 2b and 5b), and then propagate towards the deeper parts of the Irminger Basin and Labrador Sea (EOF1; Figs. 2a and 5a). The horizontal transfer of signals from the currents to the interior basins may be carried by eddies generated by the boundary currents*."

Section 4.2, lines 263-274, Figure 7. I was a bit surprised at the percentage of SSH variance explained by SSHST in the WSPNA, as there appears to be a larger misfit between the black and blue lines in Figure 7b? For the period described in the text, SSHST decrease of 5 cm was part of an SSH decrease of nearly 8 cm. What explains these differences? Changing barotropic flows? Please include some discussion of the part of the SSH signal which is not captured in SSHST.

Reply: Indeed, the mismatch between SSH and SSHST in the WSPNA is larger, however, the correlation between these time series in the WSPNA is a little greater than in the ESPNA (0.99 vs 0.97). To reflect these points, we modified the text by replacing the explained variance information with correlation and root-mean-squared differences between the time series as follows: "*The time series of SSHST closely matches those of SSH, with the correlation between them above 0.95 and the root-mean-squared differences of 0.4 in the eastern SPNA and 0.8 cm in the western SPNA (compare black and blue curves in Fig. 7). This means that the variability of SSH in the SPNA is mostly steric in nature. The remaining difference between SSH and SSHST can be attributed to density changes at depths greater than 1000-m and to errors in data; the contribution of barotropic signals is expected to be small at the time scales considered*."

Section 4.5. In the light of the previous sections on propagating signals, why were the periods here selected based separately on periods between maxima and minima of PC1 and PC2 (rather than phases of the propagating signal)? The first period (1994-2010) covers more than 2 periods of the propagating signal. And having chosen periods based on PC1 and PC2, why are they discussed in the order presented alternating PC1- and PC2-based periods? I think it would be useful to mark these periods on Figures 3 and 9.

Reply: Note that the modes have different spatial footprints, so that selecting time intervals characteristic for the main tendencies in each mode helps to better assess and visualize the role of buoyancy forcing in driving each individual mode, including the real and imaginary parts of CPC1. If we selected the time intervals based on the temporal phase of CEOF1, then EOF1 (real part of CEOF1) and EOF2 (imaginary part of CEOF2) would be mixed because of propagation. To add more clarity to the text, we have added the following sentence to the first paragraph of the revised manuscript: "*Because the two leading modes of variability have quite distinct spatial footprints, this allows to better assess and visualize the role of buoyancy forcing in driving each mode*."

Regarding the order the periods are discussed, we think it is more logical to present them in chronological order. To alleviate a potential confusion, in the first paragraph of Section 4.5 in the revised version of the manuscript, we mention that the discussion of SSH changes during the selected time intervals follows a chronological order. Also, following the recommendation of the reviewer, we marked the time intervals in Fig. 3 by adding horizontal bars with diagonal stripes.

Section 4.5. This section discusses absolute SSH changes, rather than those reconstructed from the EOFs or CEOFs. How much difference does this make?

Reply: We believe it is more appropriate to compare the absolute SSH changes with the steric and those driven by surface fluxes. Note that the reconstructed SSH is a filtered version of SSH, and the two leading models of variability have different spatial footprints. For example, in 1994-2010, the SSH increase occurred mainly in the WSPNA, well depicted by the EOF1/PC1, while the SSH change in the ESPNA was small, because in this region SSH peaked earlier in 2004 and in 2010 it was already close to values in 1994. The difference between the absolute and reconstructed SSH in the SPNA is small, as suggested by Figs. 5 and 6.

Does the west-east propagating SSH contain less local surface flux signal and more advection, for example? Or is this not possible to determine?

Reply: It is an interesting question, but because the ocean is very dynamic and constantly exchanges properties with the atmosphere and ambient water masses, it is problematic to determine how much surface-generated and advective signal is contained in a propagating SSH anomaly with available observations.

Section 4.6. While opening with the 2011-2015 cold blob in the Iceland Basin as an example of an advective feature, the discussion doesn't really consider the source of that feature, just its subsequent westward, downstream advection. Can your analysis say anything about the upstream source of this feature?

Reply: The possible sources of the 'cold blob' are mentioned in the third paragraph of the introduction with relevant references. We have removed a reference to the 'cold blob' from the first paragraph of Section 4.6 and only

mentioned that the observed cooling and freshening was caused by advection associated with the North Atlantic Current. Regarding the source of this feature, at the end of the fourth paragraph of Section 4.5 we have added the following sentence: "*This agrees with a recent study by Holliday et al. (2020), who attributed the unprecedented freshening in the eastern SPNA in 2012-2016 to large scale changes in ocean circulation driven by atmospheric forcing*."

It should be explained why velocities at 1000-dbar from Argo/altimetry are used when the cited method of Schmidt produces horizontal velocity estimates throughout the upper 1000m. It isn't clear to me, either from the methods or this results section, what different information is provided by the velocities and the eddy propagation velocities, and why both are used. I couldn't find the time-span over which these mean currents are calculated.

Reply: In Section 3.3 of the revised manuscript, we have provided more information on 1000-dbar velocities. We have also tried to stress that the 1000-dbar velocities and eddy propagation velocities complement each other.

Figures 10 and 11. These regressions are presumably come with an associated measure of significance? Perhaps consider including the arrows/colours only where they are significant?

Reply: In Figs. 10 and 11 of the revised manuscript, we have plotted arrows only at locations where regression coefficients are significant at 95% confidence.

Technical corrections
3.1 EOF analysis
This section appears to have some errors in the equations:
Eq. 1: EOFj is just a function of position (not position and time). You could use bold for the position vector, x. Explain that N is the number of EOFs in the reconstruction and that SSHR is reconstructed SSH

Reply: Thank you for noting this. We have made all suggested changes.

Eq. 2: I think the LHS of this equation is variance explained by the jth EOF, but the RHS is variance explained by the sum of the first N EOFs.

Reply: Correct! We have removed the subscript j from sigma on the LHS.

Eq. 3: LHS should make it clear it is the augmented complex SSH, perhaps call it $SSHC to differentiate it from $SSH.

Reply: Thank you for catching this. We have changed the LHS to SSH*.

Figures: Panel labels should be formatted as (a), (b), etc.

Reply: The labels have been changed.

Figure 3 caption refers to 'SSL' as opposed to SSH. I can't find reference to SSL in the text. Should SSL be SSH_ST?

Reply: Thanks for noticing this. In an earlier version of the manuscript, SSH_ST was called SSL. We have changed the caption accordingly.

Figure 11. I was very confused by the reversal of the blue-red color scale compared to everywhere else in the manuscript (to red negative, blue positive here), for a long time

thinking the figure showed exactly the opposite thing to that being described. I understood (eventually) that this was to help the comparison with the SSH patterns in Fig 8. This reversal of the scale needs to be made much clearer or, preferably, return to the conventional use of the scale. I think readers can still easily compare figures 8 and 11.

Reply: We have decided to replace wind stress curl with sea level pressure (SLP) in Fig. 11. We believe that the revised figure is visually easier to read with respect to the shifting atmospheric circulation patterns and how they relate to the westward propagation of SSH anomalies.

Figures 10 and 11. I think these regression maps are anomalies, it isn't always clear when the full variable is meant, and when it is the anomaly.

Reply: Because these are regression maps, it does not matter whether the full variables or their anomalies are used for computation. The regression maps show the change in SLP/wind per 1 standard deviation change in PC1&2 and CPC1.

Figure 17. It is difficult to see the red, green and blue contours mentioned in the caption.

Reply: The figure has been revised.