Reviewer 2

I think the authors have done a good job in revising their work and replying to most of the comments by both reviewers.

We thank the reviewer for the time spent to evaluate our manuscript, and we are happy that the revised version of the manuscript addressed most of the previous comments. We are also grateful for additional remarks, which we have tried to clarify both in this reply and in the manuscript. Our answers to reviewer's comments are given in blue font. In the revised manuscript, all new changes introduced are highlighted by dark red.

1) I am still struggling with the CEOF analysis applied to the SLP. With the analysis presented in Fig. 11, you are still expecting that the atmosphere is the main driver of the SSH anomaly as it propagates around the basin. An SSH anomaly will do so without any influence of the atmosphere as soon as it is introduced to the eastern SPNA. If the authors insist in keeping this analysis, the rational should be explicitly laid out.

We understand the reviewer's concern. In fact, the reviewer raises one of the questions, which lies beyond the scope of this study and requires further dedicated investigation. We should first clarify that we did not apply the CEOF analysis to the SLP. Fig. 11 shows regression maps of SLP and wind velocity on CPC1 computed for SSH anomalies. So, by comparing Figs. 8 and 11, we report on the correlation between the tripole-related SSH changes and shifting SLP/wind forcing patterns. We agree with the reviewer that an SSH anomaly generated in the eastern SPNA should be carried towards the Labrador Sea by the mean ocean circulation even without any additional wind forcing, albeit we do not know how quickly it would dissipate. On the other hand, we believe that it is important to report on the observed correlation between the propagating SSH anomalies and shifting SLP patterns, as it suggests that wind forcing is a possible driver as well. Physical mechanisms of the air-sea coupling in the SPNA are beyond the scope of this study, but we hope that the results of our investigation will motivate further research. To address the reviewer's concern we have modified the last three sentences of the manuscript as follows:
"*While we identify shifting wind forcing patterns and the mean ocean circulation as possible drivers for the observed westward propagation of SSH anomalies in the SPNA on the interannual-to-decadal time scales, it remains unexplored which process is more important. Further research is needed to understand the mechanisms of air-sea coupling in the SPNA, including potential oceanic feedback on the atmospheric circulation. Finally, it is of particular interest to explore how the tripole variability is related to inter-gyre exchange and to the AMOC*."

2) The new paragraph in the last section (Discussion and Conclusions) on gyre strength and subpolar front changes seems speculative. The authors need to be careful with these connections and show evidence of these.

This paragraph was added to address the reviewer's question in the first review: What does this signal propagation tell us about the two-way coupling or communication

between the two gyres during its evolution? To address the concern with this addition we slightly edited the text in the paragraph trying to improve its message (lines 539-554). This should make it clear to readers that we discuss observations and figures presented in the manuscript in the context of a previously suggested hypothesis on inter-gyre exchange (Hakkinen et al., 2011; Piecuch et al., 2017). With the revisions of this paragraph we believe that it is well suitable for the discussion section.

3) Please add relevant references to the role of eddies, page 18, line 1 “...to the interior basins may be carried by eddies generated by the boundary currents.”

We thank the reviewer for pointing this out, and we have added related references in lines 548-549 (Fan et al., 2013; de Jong et al., 2014).