

Response to Reviewer 2 (Yueng-Djern Lenn)

We appreciate the insightful review of our paper by Dr. Lenn of our manuscript "Import of Atlantic Water and sea ice control the ocean environment in the northern Barents Sea". We found the comments very helpful, and we believe they have contributed to a better manuscript upon revision.

The reviewer found the part of the paper dealing with atmospheric forcing of inflow pulses less satisfactory. While we agree up to a point, and have addressed some of the specific concerns (see below), we would also like to stress that we cannot conclusively pin down the mechanism at work based on our data, and we want to remain completely open about this in the manuscript. What we can confidently say based on our observations is that there is a significant correlation between the ocean inflow and the synoptic atmospheric forcing. In the revised manuscript, we have stated more explicitly that we do not in fact believe that we can confidently determine the dynamical mechanisms at work based on our (single-point) current measurements.

Detailed comments are found below. Line numbers refer to the revised manuscript.

Atmosphere-ocean mechanism:

- Both reviewers reacted to the somewhat unmotivated use of the pressure differential between the two weather stations as an atmospheric forcing index used in the correlation analysis. In the revised manuscript, we have reorganized these parts of the paper, moving the introduction of the pressure difference index to the results section and spending more time justifying its use (L540-554). We have also included a correlation analysis between ocean currents and reanalysis winds (Fig. 10) in the preceding parts (L524-539), which hopefully makes the pressure index appear in a more logical sequence.

We have also taken up the reviewer's sensible suggestion of including a much larger region when showing correlations with atmospheric forcing (Figs. 10 and A2).

We have not included the appendix figures in the main manuscript. Instead, we hope that the inclusion of the new Figure 10 can serve to give the reader a good overview of the relationship between atmospheric forcing and ocean response over a larger spatial scale.

We have elaborated somewhat on remote vs local generation of the observed signals in Section 4.3 (L838-845). We have not made an attempt to estimate wave speeds directly (a rather complicated exercise involving detailed knowledge of wave dynamics, scales, stratification, and topography), but have instead made reference to the phase speeds reported by Inall et al 2015 in western Spitsbergen. We have also modified the statement in question to not so categorically discount remotely generated waves.

Minor points:

- We appreciate the suggestion to broaden the references in the Introduction. The revised version includes a wider and more comprehensive set of references, including relevant papers by Schauer and Mauritzen. We have not removed the previous references as we find that they also provide meaningful background context to this study (the knowledge base has expanded significantly during the last 10-20 years).

- We have followed the reviewer's suggestion of writing out certain acronyms in order to facilitate easier reading and avoid confusion with water masses. "nBS" has been replaced with "northern Barents Sea", "KT" with "Kvitøya Trough", etc. We have left abbreviations of water masses and named ocean currents in line with standard practice.
- We appreciate the suggestion to clarify the language in order to make a clearer distinction between implied advection and local processes. In the revised manuscript, we have avoided words like "warming" and "freshening" unless local processes, as opposed to advection, are implied.
- We take the reviewer's point in that the discussion about freshwater and sea ice could be strengthened by a more quantitative analysis. Unfortunately, we believe that our options are somewhat limited given that we have few sensors to work with (one CTD sensor near 20 m). Given the major advective sea ice imports, it is also difficult to distinguish between sea ice increase due to local freezing versus advection from other regions. We agree with the reviewer that a more quantitative assessment of the ice-air-mixed layer interplay would strengthen this part of the paper, but have avoided estimates requiring extrapolating from the data beyond what we believe is reasonable. For example, any estimate of the depth of the mixed layer, the structure of the haline and thermal stratification, or integrated heat/salinity budget terms for the mixed layer would be of great interest, but unfortunately, we cannot estimate any of these based on our data. (As pointed out by Reviewer #1, exactly the same can be said about estimating freshwater content based on very few sensors in the vertical. We have therefore elected to remove the freshwater content estimate altogether in the updated manuscript, sticking to a discussion in terms of salinity.) We have tried to make our approach clear in the Discussion (L611-614).