Response to reviewer comments on Kolås et al. EGUSPHERE-2023-2864*, The Polar Front in the northwestern Barents Sea: structure, variability and mixing.*

We thank the anonymous reviewer for the constructive comments and useful suggestions, which helped to improve the manuscript. Below we provide a point-by-point response to all comments. Reviewer comments are reproduced in *italic type in red* followed by our response in regular type in black color. The future tense refers to our plan to address the comments when preparing the revised version.

Response to Reviewer 2

The authors provide a nice description of the structure of the Polar Front in the Barents Sea based on extensive ship board data (a fall and a winter cruise) as well as data from several gliders. Even though this region has been extensively studied, the study provides some novel aspects of the dynamics of the front, in particular that it is baroclinic at the sill, unlike its barotropic nature encountered further south. The paper is well-written, easy to read and does not suffer from any major issues in the methodology or interpretations. On several occasions during the read I however asked myself "So what?". Where possible it might be nice for the authors to clarify the motivation for what they are doing or what the implications of their findings could be. That being said, the baroclinic nature as well as the level of mixing at the front (weaker than at the surface and at the bottom, but quite large for the mid-water column!) are important additions to the literature that are well placed in "Ocean Science". Consequently, I can recommend this manuscript for publication after minor revisions; the minor points below do not warrant a second round at the reviewers.

Thank you for positive feedback and for recommending our study for publication. We agree that the implications of our findings could be highlighted more, and we will make an effort to do so in the revised version. This is also consistent with reviewer 1's recommendation.

Minor points:

Fig. 1a This figure does not provide the important information. Most of the topographic features are not labelled and the isobaths are very hard to understand/interpret. The sill is not label or marked even though it is the key location of the paper. Fig. 11 of the authors' JGR preprint is much clearer and I'm wondering why the authors don't use a modified version of that figure here, in particular the labeling and the water depth as a color scale and not just as contour lines.

Thank you for your suggestions. We now revise the figure and use a version of Fig. 11 in the JGR paper.

L143 Do you mean "southward wind" or "northerly winds"?

Indeed, thank you for pointing this out. Corrected.

L199 It might be nice to explicitly give the equation for C_D as a function of sea ice concentration rather than only referring to the 2005 paper.

We inserted the equation for C_D.

L214 You give a reason, but that still leaves the question as to why you decided to do that.

The reason for only considering the water column below 50 m depth is because the stationary front is located there. Above 50 m, we do not really capture the front in any of the ship transects, but only in a few of the glider transects. The upper 50 m and the water column below are two very different domains that are influenced by different physical processes. Here we only consider the lower layer. We will elaborate on this in the revised version of the manuscript.

L221 How are L_x and L_z estimated?

They are estimated based on the semi-variogram analysis described in the appendix of Kolås et al. 2020. We now cross-reference this in the revised manuscript.

L237 Is this recalculation of salinity from the sorted density profiles common oceanographic practice? Then please cite examples from the literature. Otherwise, it is a worthwhile methodological advancement that should be motivated, justified, and also advertised a bit more prominently than only with this single sentence.

We are not aware of other examples in the literature. The reason we chose to recalculate salinity from sorted density fields in the objectively mapped sections is to reduce physical inconsistencies in the set of related variables (T, S, rho) that stem from individually mapped fields (T, S). One can objectively map the density observations, or one can calculate the density from the objectively mapped temperature and salinity fields. We opted for the latter to make the T/S and density fields consistent. Because an objectively mapped field is not natural data, errors in T and S can propagate into density calculations and result in spurious unstable layers. A simple approach is then obtaining a physically meaningful, gravitationally stable density field and calculating salinity from this. The difference is minor. We now expand the text with our motivation, but we do not consider this a methodological advancement.

L280 Does this bias your estimation of EKE at a grid point when sea ice is present (only) at certain periods of time (which might e.g. be high, or low, EKE time periods)?

Yes, it probably can bias our EKE estimate to some degree. EKE is likely stronger when sea ice concentration (SIC) is low versus times of dense ice cover (von Appen, 2022). Hence, removing data where sea ice concentration exceeds 15 % may cause some overestimation of the average EKE within our domain. However, the average EKE calculation within our domain is, and should be interpreted as, the average EKE within the waters with little or no sea ice, and is not representative of ice-covered waters. We now add sentences describing this: "In all EKE estimates, SLA measurements where SIC was above 15 % have been discarded. Hence the average EKE presented here is likely not representative of the region in periods when the region is mainly covered by ice. EKE is known to be stronger when SIC is low compared to times when SIC is high (von Appen, 2022).".

L318 Why do you not calculate the gradients from each transect directly and then average the gradients to substitute the numbers currently given in L319. Note that averaged T/S will smooth (among others due to differences in the horizontal location of the strongest gradients) the gradients substantially compared to what is presumably present in each of the individual sections.

Thank you for suggesting this. We agree and we now calculate the gradients from each transect.

L323-330 These lines are repetitive. Consider "This reversal is discussed in the next section." And then "Simultaneously, …"

Agreed. Corrected as suggested.

L336-339 I can't quite follow this Eulerian vs. Lagrangian view.

We agree that the description of the behaviour and movement of the eddy can be hard to follow. Reviewer 1 suggested to draw a box around the eddy in Figure 6 to highlight the eddy. We think this is a good suggestion which will help the reader follow our description. We revised the figure to reflect this and will improve on our description in the revised version.

Fig. 6 caption "specified in the lower left corner" I can't see it.

Dates are specified in the lower left corners of the subplot in panel (a). We now specify this.

Fig. 7 caption Consider rephrasing "at B5 in fall (left) and at B7 in winter (right)".

Agreed.

Fig. 8 right part of figure: delta time = 1 day is a different amount of centimeters on the printed page for the upper panel (October 2020) than for the lower panel (February 2021). Consider making it equal.

Corrected.

Fig. 9 caption "in Figure 2" (a space is missing in front of the "2").

Corrected.

L432 "We expect the contribution"

Corrected.

L440 "in mid-October"

Corrected.

L442 "between the averaging box and the position"

Corrected.

L456 There are other possible explanations (L 454 "may be related to"). There might be a sea ice related bias (see comment L280). There might be an uneven distribution of events driven by external *(non-climate change related) interannual variability in the 2 decades. E.g. (I'm just making up numbers/causal relations for point of illustration) EKE could be high during high NAO phases. In the first decade there were 3 years with high NAO and in the second decade there were 6 years with high NAO even though there is no long-term trend in the frequency of high NAO events.*

We agree that there could be other explanations and we now elaborate on this in the discussion. A sea-ice-related bias is unlikely as more ice will potentially cause an overestimate of the EKE (see our response to L280). The sea ice cover is declining, hence there is likely more sea ice present during the 2000-09 period compared to the 2010-19 period, and the effect of a bias likely would be to decrease the difference in EKE between the two decades.
NOA is linked to the AW transport through the Barents Sea opening (BSO), however, the mean NAO index was on average lower in the 2010-19 period than in the 2000-2009 period. In addition, local storms may affect the AW transport through BSO more than the NAO (Haukamp et al. 2023).

L501 "scientists" "Haakon cruise"

Corrected.