

## Comments by the Editor

Thank you for this interesting manuscript. As you see, both reviewers have some constructive comments. I wanted to drop this editor comment so that you can consider the points below when preparing your final response and the revised version. My major criticism is that the discussion section is not well developed and must be improved.

Thank you for your detailed comments. We improved the discussion section, and hope that it is better now.

Here is a list of minor issues which must be clarified or written differently:

Li 25: “strongly turbulent”: presence of mesoscale eddies does not make the oceanic conditions strongly turbulent. If this is supported by microstructure measurements, please use and cite; if not please choose a different wording (energetic?).

Corrected. “Energetic” sounds much better.

Li 33: do eddies lead to vertical eddy fluxes? I thought they would lead to lateral fluxes. Otherwise, please clarify the pathway from lateral to vertical.

Eddies lead to both lateral and vertical fluxes, and the reason is that fluid particles move along isopycnals which are inclined. When eddies flatten isopycnals converting APE to EKE, lighter water moves upward, and denser downward along mean isopycnals. This is the main part of the GM parameterization. Therefore, eddies contribute to vertical heat flux. We reformulated point (2) in the introduction section:

“As shown by Hattermann et al. (2016), this region is characterised by negative values of vertical eddy temperature flux. Thus, eddy processes likely play an important role for the subduction of AW.”

Li 38. How is the MIZ “shaped” by eddies?

We reformulated this sentence: “Eddies play an important role for sea ice-ocean interaction. The marginal ice zone is influenced by eddies...”

Li 47-48: I do not think Teigen et al or Johannessen et al are the key references for the theory or dynamics related to barotropic instability or topographic steering/trapping of eddies, respectively.

We added the reference to Cushman-Roisin's book, and modified the sentence about barotropic instability: "... and eddies can be formed by receiving kinetic energy from the mean flow as shown for the Fram Strait by Teigen et al. 2011." We also added the reference to the study by Smith et al. (1984) which describes the topographic generation of an eddy in the EGC.

Li 92: Sundfjord et al (2017) is about a Svalbard fjord and I am not sure how it is relevant to the ability of the model to reproduce the slope boundary current.

We removed this reference.

Li 100: What are "a and b". Please clarify.

We added a description of parameters a and b.

Li 105: we also use

Corrected.

Li 117 and 118: please justify the choices of 3 day and 100 m depth

We added a justification of the choice of the 3 days and depth of 100 m in section 2.3: "We decided to use a threshold of 3 days mainly because the temporal resolution of the model output data is daily, and the eddy should form a track. This also helps to make sure that the eddies detected are real and not an overdetection due to uncertainties in the detection method. Eddies with a lifetime of at least three days are also required when computing the translation velocity needed to compute the eddy nonlinearity parameter, for which centred differences are used."

"We decided to choose the depth of 100 m since both main water masses of the Fram Strait, AW and PW, are present at this depth (e.g. Wekerle et al., 2017, their Figure 9)."

Li 130: by experts? (please clarify)

We removed this sentence to avoid confusion.

Heading 2.4, remove one "and"

Corrected.

Li 145: these terms do not indicate instability but rather conversion from MKE to EKE and EPE to EKE (which can be related to the instabilities you mention).

We corrected this sentence: "... can be related to.."

Li 169-171: the syntax of frequencies and slopes are difficult to follow for a reader

We restructured the sentence, and hope that it is clearer now.

Li 193: did you introduce a stream function?

This was described in the methods section 2.3: "Eddy boundaries around each detected centre are determined by the outermost closed contour of the stream function field."

Li 200: Isn't this the Rossby number?

Right, this is the Rossby number. We used the Rossby number as an index for eddy intensity. We added a better description of the eddy intensity in section 4.3.

Li 229: Molloy

Corrected.

Li 293: The Ghaffari analysis is from a 2-layer model? I am not sure how this is directly comparable (at least you might want to point this out). About the instability of the slope current along the Lofoten escarpment, please see some recent conversion rate calculations similar to yours, using high resolution ROMS fields (Section 9, in a otherwise mooring observation paper): Ocean Sci., <https://doi.org/10.5194/os-16-685-2020>. Not that I authored this paper, so feel free to ignore this suggestion. However, the conversion rate fields (Fig 11 in both papers) are directly comparable.

Thanks a lot for hinting us to this recent study, it fits much better here and we thus cited it. It is also very valuable to be able to compare the magnitude of conversion rates to our estimates. We removed the reference to the paper by Ghaffari et al.

Li 309: Need a dot product before the buoyancy gradient?

Corrected.

Discussion section is not appropriate and must be improved. Also I note that the last paragraph (on providing information to develop GM type parameterization) is not really supported by your results or built upon them in a convincing way. Please improve this part or remove.

We improved the Discussion section. In particular, we added a new paragraph which discusses the differences between eddy occurrences and EKE. Moreover, we added a paragraph about “Implications for contributing to future model development”. Here we also point to the energy backscatter scheme mentioned by Reviewer 1. We also improved the paragraph about the GM parameterization.

Opening paragraph of the Conclusions is not conclusions (or findings from your study), and could be integrated to discussion or removed.

We removed this paragraph.

Fram Strait: (If I’m not wrong) in the English usage, you should drop “the” in front of Fram Strait (except referring to a specific feature associated with Fram Strait, say, the Fram Strait circulation etc.) This must be corrected throughout, including the title.

Corrected.

Fig 4: I think it is not very meaningful to show the spectra of daily averaged speed (this is what you mean by absolute velocity?). It would be better to show the sum of spectra from  $u$  and  $v$  components (this corresponds to distribution of double the horizontal kinetic energy, or divide by two and call it HKE spectrum). In any case, you need units on y axis ( $(\text{m/s})^2/(1/\text{day})$  ?). It would be helpful with 95% confidence intervals on the spectra.

As suggested, we computed spectra for  $u$  and  $v$  separately and show now in Figure 4 the sum of the two spectra divided by 2. Now units are shown on the y-axis, and the 95% confidence intervals are indicated too.