

# ***Interactive comment on “Properties and dynamics of mesoscale-eddies in the Fram Strait from a comparison between two high-resolution ocean-sea ice models” by Claudia Wekerle et al.***

**Ilker Fer (Editor)**

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Dear Claudia,

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.

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

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conditions strongly turbulent. If this is supported by microstructure measurements, please use and cite; if not please choose a different wording (energetic?).

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.

Li 38. How is the MIZ “shaped” 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.

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.

Li 100: What are “a and b”. Please clarify.

Li 105: we also use

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

Li 130: by experts? (please clarify)

Heading 2.4, remove one “and”

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).

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

Li 193: did you introduce a stream function?

Li 200: Isn't this the Rossby number?

Li 229: Molloy

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

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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>. Note 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.

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

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.

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

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.

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.

Thank you —————

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Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2020-24>, 2020.

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