

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.

Anonymous Referee #2

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Review Comments:

The present paper reports results from a sensitivity study on the ability of two different numerical circulation models (ROMS and FESOM) in reproducing comparable eddy dynamic properties in terms of generation sites and propagation pathway in Fram Strait, a region characterized by a complex bathymetric configuration. Main difference between these model lies on their numerics and formulations, which includes numerical grid discretization, horizontal and vertical mesh resolution, parameterizations, and coverage (i.e. global vs. regional models).

The study is generally well written, and the analysis performed are appropriate. The

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figures are neat and clean. The discussion of the results is superficial. The authors should inspect more thoroughly the results presented. Nevertheless, the study stands as a good contribution, as it adds knowledge to the region. However, unfortunately it doesn't add new knowledge to the subject field whether in ocean modelling or in geophysical fluid dynamics. If it does, then perhaps it has not been clearly presented.

Find below few/minor points which the authors should address before its acceptance for publication.

The reference for ROMS in the introduction section may not be the most appropriate here!

Line 105 the sentence seems incomplete. In part it reads as “we also the Okubo-Weiss”. Do you mean “we also used the Okubo-Weiss”?

Are there any criteria on the choice of the number of days taken as threshold to discard eddies with lifetime lesser than 3 days? Please note that the caption in Figure 10 and in other parts of the manuscript indicate a lifetime of 30 days. Is there any inconsistency??

Paragraph 130. What do you mean by eddy detected by experts?? Please make it clear.

Did you use any filtering on the field of Okubo-Weiss parameter?

Perhaps the differences between the model maps shown in Figures 2 and 3 should be quantified to highlight geographical/spatial sites where the models converge and where they diverge (e.g: $r = \text{ROMS} - \text{FESOM}$ for the parameters presented in Fig2 and Fig3).

Under the Model assessment section, paragraph 155, the authors indicate the model simulates similar spatial distribution of the water masses. However, no T/S diagrams have been shown. Perhaps replacing the term “water-masses” with “thermo-haline properties” would be more appropriate. The same is true wherever this term appears in this manuscript.

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Lines 170 – 173, is the difference only related to the effects of tides simulated in ROMS? Could not be also somehow related to differences in the surface forcing fields between the models?

Lines 180 – 184: Are the eddy statistics computed in a Lagrangian or Eulerian frame of reference? Please clarify this important aspect.

An important eddy property which could be included in this study is the eddy nonlinearity parameter. This parameter would give a good insight on the eddy's coherence and ability to trap and transport material along their pathway of propagation.

Lines 248 – 249. Are the tides the only difference between the models? What about the vertical discretization of the water column??

In section vertical extension and hydrographic properties: Are the values of vorticity in the eddy centers, a single point values or are averaged values within the eddies? Please make this aspect clear.

Line 279: is the superscript number after (Figure 11a, b)¹ meant to be there?

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