

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 #1

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This paper presents a comparison of eddy fields in two models: a ROMS configuration and a FESOM configuration. The models seem to agree in general but differ some in the details. The analysis focuses more on eddy statistics and there is little dynamical analysis. The main conclusion is that the two model configurations simulate roughly similar eddy statistics.

I recommend major revision because I think the paper would be much better if it demonstrated a connection between the dynamical analysis (regions of baroclinic or barotropic instability) and the eddy statistics (e.g. generation regions of long-lived eddies). The individual pieces for analyzing this connection mostly exist so this seems

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reasonable to expect. Otherwise, the paper is well written and easy to understand. Some of the diagnostics used (e.g. T, S anomalies) were not described clearly — these need some major improvement (see below). The figures were nice to look at and illustrate the main points well though some extra panels would be useful (see below). Most of my other comments are minor.

1. deformation radius $\approx 4x$ grid spacing. Is this "eddy resolving" or "eddy permitting"? Some discussion with references to literature would be useful background here.
2. Line 90: Does Figure 1 show the domain for the ROMS simulation? Is this the region with refined resolution for the FESOM run?
3. Line 100: Please list the 4 constraints.
4. Line 100: Do a and b have units? What is the minimum possible detected eddy size in km? How does this compare to grid spacing?
5. Line 167: What was the longest gap you had to interpolate over?
6. Line 162: Did you average the spectra from the 3 moorings?
7. Since the eddies in FESOM are weaker (see spectra), should you adjust your criteria to be more appropriate for these weaker eddies? In other words, are you undercounting eddies in FESOM because they are weaker than the thresholds you are using?
8. Figure 6: How does this figure compare to a map of EKE averaged over the 3 years? 1. Is the sign or caption wrong in Figure 6c, d? It seems to say there are more anticyclones on the shelves contrary to line 189. 2. Also are the topographic contours the same as in previous figures? They are hard to see and it is hard to identify the 1000m isobath referred to in the text.
9. Section 4.3: This "eddy intensity" is really a Rossby number (relative vorticity / f). Can you call it that?
10. Section 4.5: These eddy pathways should be interpreted with the help of mean

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velocity vectors. Jet instabilities generate cyclones on the anticyclonic side of the jet and anticyclones on the cyclonic side of the jet. For the Svalbard Branch for example, one would see anticyclones on the deeper side but cyclones in shallower side which would partially explain the asymmetry in trajectories, i.e. cyclones and anticyclones are being generated in different places. I agree that the long term tendency for anticyclones to cluster in Boreas basin is likely a topographic effect.

11. Line 255: How do you define the vertical extent?

12. Line 255: I also didn't understand how you define T, S anomalies. Are these T, S at all detected eddy center locations minus a monthly mean at the eddy center? Is this a climatological monthly mean or a monthly mean for a particular year the eddy was tracked? This definition should be clearly written.

13. Figure 8,9: Why is there a large difference in number of cyclone tracks between ROMS and FESOM in "FS Central South"

14. Lines 285-290: There are other regions with large conversions (e.g. just north and south of 77N) that are stable in Figure 12b. Would this disappear if you calculated the barotropic PV gradient properly accounting for u also? Did you use depth-averaged and monthly mean v? or monthly mean v at 100m? Why did you only use FESOM for Figure 12b?

15. "EPE" — this should really be APE (available potential energy) since eddies are deriving their KE from the APE of the /mean/ state (Vallis 2006 textbook).

16. Line 315: This is a nice demonstration that the flow is baroclinically unstable but is it unstable in the regions necessary to explain the eddy tracks in Figures 8,9? Some connection between the instability analysis and the eddy tracks is needed.

17. Lines 345, 346: I'm not sure there was any discussion of "EKE→MKE conversion associated with steepening isopycnals". Please describe this in more detail. The "energy backscatter" parameterizations being currently developed would be useful lit-

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erature to reference (e.g. Jansen et al 2015)

18. Discuss sensitivity of eddy maps to choice of 100m depth

19. Figure 1: Showing a second panel with a larger region would be nice for readers not familiar with this place.

20. Figure 4: It is more conventional to show logarithm axis scales instead of showing $\log_{10}(\text{quantity})$ on a linear scale. Why did you choose to not do that? Please mark 1/30 days since that's the frequency you chose to separate "mean" and "eddies"

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