Ocean Science
Discussions

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Interactive comment on "Very high-resolution modelling of submesoscale turbulent patterns and processes in the Baltic Sea" *by* Reiner Onken et al.

Anonymous Referee #2

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A nested numerical model is applied to a part of the Baltic Sea to analyse surface submesoscale dynamics. There are three nesting levels, using different models and model configurations, to forced the inner nest with a high spatial resolution of 100m.

For several reasons, this paper cannot be recommended for publication in Ocean Science:

1. The model nesting is inconsistent, with a daily forcing from the outer model and a bathymetry in the inner nests which is based on a coarse bathymetry reconstruction although high-resolution bathymetry data should be available for this region. Boundary data transfer from the out model to the inner model seems incomplete. The middle nest uses a high-order turbulence closure whereas the high-resolution nest uses a bulk method (the KPP model, which is not taking into account several relevant processes),

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Discussion paper

because the high-resolution method is computationally expensive.

2. The simulations in the high-resolution nest are carried out without wind forcing, because with wind forcing, the surface variability in salinity is much weaker than the observed ocean color variability. All analysis is based on this simulation without wind forcing.

3. There is no model validation carried out at all, although high-quality observational data should be available, as described in lines 8-14. The only qualitative comparison between model results and observations shows a snapshot of the simulated sea sur- face salinity (without wind forcing) and an ocean color image (representing chlorophyll). There is no evidence that those patterns should be similar.

Therefore, I recommend to reject this submission.

Some detailed comments to pages 1 – 8:

Page 1:

8: "high vertical velocities" instead of "strong vertical speeds"

11: What is the tendency equation?

18: delete "ambitious". All research should be ambitious.

19: The word "turbulent" is missing here.

20: Here, turbulent kinetic energy seems to be used with a different meaning than usual. TKE is generally understood as non-hydrostatic which cannot be reproduced by a hydrostatic model. Do you here mean eddy kinetic energy (EKE)?

22-24: Does this whole range of scales also occur at one location? I thought that sub- mesoscale turbulence is defined by non-dimensional parameters such as the Rossby number or the Richardson number (both being order of unity or larger). In the Bornholm Sea, you find probably not find submesoscale features of 100 m scale in the vertical.

Page 2:

1-5: I feel that this discussion of the many order of magnitude is an unnecessary overstatement. There is no turbulence on the 1000 km scale and I would base the definition on these non-dimensional numbers only. I propose that the Rossby number is defined here. For a Rossby number of order of unity, I would not say that Earth rotation is on minor importance. That should be the case only for much larger Rossby numbers.

6-14: The red cascade is in contradiction to the spontaneous emission of submesoscale structured from the meso-scale. So, we indeed have a (blue) forward cascade. The contradiction is due to the fact that there are about 40 years between the results referenced here. Therefore, I recommend to reformulate this paragraph.

Page 4:

16: Explain what an HBM operational model is.

Page 5:

10: It is a bit surprising that daily mean fields are used to forced a model simulations which is designed to reproduce highly variable dynamics. The reasoning for this needs to be motivated.

18: It is unclear why different turbulence closure models have been used for the 500 m and 100 m models. And specifically confusing is that the large-scale closure model KPP is used for the very high-resolution simulation of 100m. There will be many relevant processes at that scale which are not reproduced by the KPP such as static instabilities induced by differential advection. I would also suspect that the KPP model does not reproduce the logarithmic bottom boundary layer.

27: The use of the s-coordinate should be mentioned in the paragraph above, along with the information about the number of vertical layers. I find it very unusual to base a high-performance model simulation on such a weak data basis. The boundary data

must include depth information, and with that the depth should be known.

Page 6:

30-32: This information about the turbulence closure model has partially been given before (page 5). The additional information given here should be moved up to section 2.3.

Page 7:

6-7: Length scales are partially given in metric units and partially in nautical miles. I recommend to use metric units.

8-9: Which gap is there between the meso-scale and the submeso-scale?

12: TKE for Total Kinetic Energy is very confusing, also since turbulent kinetic energy is used as well. I propose using KE for Total Kinetic Energy.

Figure 3: It might not be clear to everyone what "cumulatively averaged" TKE is. It should also be stated that this is the kinetic energy per unit mass.

17-25: It does not become clear, why the unforced simulation is carried out and why it has been performed. Either do not discuss it or make sure that it gives a clear message. I specifically do not see how the spin-up time of the 500m nest can be estimated by this procedure. Lines 30-32 may contain an insight from this exercise. But it is not clear why vertical mixing should enhance horizontal mixing that would blur the fine structures. And if that happens, it should probably be a relevant process that needs to be discussed here in depth.

P 7, 33 – p 8, 8: It is not clear why one quantity in the model simulations (salinity) is validated with another quantity (ocean color). The latter is certainly related to chlorophyll which in turn might be related to phytoplankton concentration, a possibly positively buoyant particulate matter. It is not clear, why these two quantities should be related. Why do you not compare observed surface temperature with simulated surface tem-

perature?

Page 8

10-19: I have read this section many times, and I am quite sure that you say that you carry out the 100m simulation without any surface forcing because it would blur the submesoscale structures. It that really true? It is well acknowledged in the literature that submesosales are driven by surface forcing as well. So, if the model results detoriate due to the surface forcing, then the consequence should not be shut off the forcing but to find the reason between model results and simulations. Something seems to be fundamentally wrong.

If I see it right, a numerical is analysed here of which the only single validation is the qualitative comparison between a snapshot of surface salinity and an ocean color snapshot. There is wind forcing in reality and no wind forcing in the model. Some of the structures look similar. That's all. Afterwards, the model results are intensively analysed in terms of submesoscale dynamics.

Clearly, that is not science.