

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.

Both for R500 and R100, the GEBCO_2014 grid with 30 arc seconds resolution was used. This is the highest-resolution available bathymetry data set for the Baltic. See P5L29 of the original manuscript.

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), because the high-resolution method is computationally expensive.

We used indeed the KPP model for the R100 nest, because the usage of GLS would have doubled the CPU time from 2.5 to about 5 days! Moreover, as no atmospheric forcing is applied in R100, the applied vertical mixing scheme is of secondary importance. We do not see, why the *boundary data transfer ... seems incomplete*.

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.

We have never stated that *with wind forcing, the surface variability in salinity is much weaker than the observed ocean color variability*

3. There is no model validation carried out at all, although high-quality observational data should be available, as described in lines 8-14.

Yes – there are observations available from “Expedition Clockwork Ocean“, but the observations during that expedition were confined to rather small areas (less than a square kilometre), focusing on isolated submesoscale patterns. Hence, these observations are not suited for any validation.

The only qualitative comparison between model results and observations shows a snapshot of the simulated sea surface salinity (without wind forcing) and an ocean color image (representing chlorophyll). There is no evidence that those patterns should be similar.

Both salinity and chlorophyll are passive tracers which can be used as proxies for circulation patterns. Therefore, it is absolutely legitimate to compare them. Moreover, the R500 run **with** wind forcing was used.

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” OK

11: What is the tendency equation?

The tendency equation is explained comprehensively in 4.3.1. Experts on submesoscale dynamics should be familiar with it.

18: delete “ambitious”. All research should be ambitious. OK

19: The word “turbulent” is missing here. OK

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)?

The Reviewer apparently associates TKE with turbulent kinetic energy occurring in vertical mixing processes – therefore *non-hydrostatic*. However, in the manuscript we never used the expression *turbulent kinetic energy* and consider only the *total kinetic energy* of horizontal processes.

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.

Lines 22-24 are intended as a general introduction into the subject submesoscale turbulence. There is not at all a relationship with the Bornholm Basin. And the question *Does this whole range of scales also occur at one location?* reveals that the Reviewer has no clue what’s is about.

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

This is not true! We are talking here about 2-dimensional turbulence; and the gyres in the ocean are elements of 2-dimensional turbulence (at the 1000-km scale, it is called geostrophic turbulence)

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.

That’s a matter of opinion.

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.

The mesoscale feeds both the gyre scale (red cascade) and the submesoscale (blue cascade). This is not contradicting Charney (1971) or Rhines (1979). Those classical papers focused only on the feedback of (2-d) turbulent kinetic energy from the meso/synoptic scale to the larger scale.

Page 4:

16: Explain what an HBM operational model is.

Is explained in Section 2.2

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.

Daily fields are fully sufficient for the forcing of a model with 500-m resolution. In addition, the boundary conditions are updated at every time step by interpolation in time and nudging.

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.

Static instabilities in ROMS are removed by a convective adjustment algorithm; a quadratic law is used for the bottom friction. See P5L19 and our reply to item 1 above.

27: The use of the s-coordinate should be mentioned in the paragraph above, along with the information about the number of vertical layers.

The use of s-coordinates is mentioned on P5L14, the number of layers is given in Table 1

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.

But the reality is that the water depth is **not** included in the HBM output available at CMEMS!

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.

OK

Page 7:

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

Generally, metric units are used. However, when graphics are based on a WGS84 coordinate system nautical miles are better suitable because 1 arc minute in latitude = 1 mile.

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

We mean the spectral gap in wavenumber space

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.

OK

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.

Cumulative average is a well-known method in data analysis, indicating when a system attains stability (e.g. see the function *cumsum.m* in Matlab).

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.

This is extensively explained on P7L10-25

Lines 30-32 may contain an insight from this exercise.

Of course! More references of similar findings will be added there (thanks to the other Reviewer), which support our findings.

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.

Atmospheric forcing does not simply induce vertical mixing. The wind forcing impacts the momentum transport on the scale of the forcing pattern, which in turn modifies the local vertical mixing and perhaps blocks the restratification by mixed-layer instability. A paper considering the impact of atmospheric forcing on the evolution of submesoscale patterns is in the works.

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.

See above 3.

Why do you not compare observed surface temperature with simulated surface temperature?

The mesoscale distribution of SST can only derived from satellite IR measurements. However, the resolution of those is too low.

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?

Yes – it is true! More references will be added.

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.

This was not the objective of this article.

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.

Apparently, the Reviewer stopped reading at P8.

Clearly, that is not science.

But this final remark is an offense to the authors! The first author (R. Onken) is visible in the oceanographic community since 1982, and he authored 40 articles in peer-reviewed journals and books. Frequently, Onken had disagreements with reviewers, but none of them criticised any of my manuscripts as "that is not science".

It is easy to make such derogative comments as an anonymous reviewer!

Interactive comment on Ocean Sci. Discuss., <https://doi.org/10.5194/os-2019-44>, 2019.