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Ocean Science
Discussions

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

Anonymous Referee #3

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Onken et al. Very high-resolution modelling of submesoscale turbulent patterns and processes in the Baltic Sea; Ocean Sci. Discuss., https://doi.org/10.5194/os-2019-44

Review

Major comments

The authors present an interesting numerical experiment for studying the evolution and characteristics of submesoscale processes. It is worth to be published when appropriately revised, especially in regard to the interpretation of results.

My major recommendation is that the manuscript has to be rewritten to state clearly the aim of the study and the methods applied and to interpret the results accordingly. Authors present a numerical experiment where atmospheric forcing was turned off for

the model with the highest resolution while the initial and boundary conditions were created with the model with atmospheric forcing. It is unclear what we could learn from such an experiment regarding submesoscale processes. Is the aim to show how the submesoscale processes evolve in case of the sudden vanishing of external forcing and what are their characteristics in such conditions?

The other concern is related to the description of methods. You state that special care was taken for the preparation of initial and boundary conditions for the R100 model as the nesting ratio of five is challenging. However, you do not provide any arguments on why the cubic spline was used. Did you try other methods or run any sensitivity tests? Are some results (as the revealed false patterns) somehow related to this procedure? No information is given about the atmospheric forcing in HBM. It is not clear how the domain averaged TKE and cumulative averaged TKE are calculated, etc. See the specific comments below.

Specific comments

P1L8: Are the speeds of 100 m day-1 characteristic for submesoscale processes or internal waves?

P1L10-12: Sentences like "The conditions for inertial and symmetric instability are evaluated for the whole domain, and the components of the tendency equation are computed in a subregion." are not informative in the concluding section of the abstract.

P5L9-10: Why HBM daily mean fields were used? What about the atmospheric forcing for HBM?

P6L4-5: What is meant by the term "cycle"? How can the whole first cycle be used as initial conditions?

P6L12-13: The use of cubic spline is not justified. Did you run any tests how the interpolation method could influence the results?

P6L21-23: Atmospheric forcing for R500 and R100 is mentioned here for the first time.

What about atmospheric forcing in HBM? Do the forcing sources differ between HBM and ROMS? Mentioning of atmospheric forcing and interpolation of forcing parameters for R100 is not relevant since you present the results from runs without atmospheric forcing.

P7L1-2: I do not understand this statement about salinity as the ideal parameter. Your atmospheric forcing has a resolution of 6.5 km. What could cause the blurring of the surface signal of temperature in submesoscale range when using such forcing?

P7L5-6: No difference between the HBM and R500 on 1 June – it is the initial day when the fields are identical as seen in Fig. 2.

P7L12-13: How the domain averaged TKE is calculated (all model layers, volume average)?

P7L13-14: What is "the cumulative average TKER500"?

P7L15-25: The aim of the entire section is unclear. You wrote that the idea was to provide a rough estimate of the spin-up time, but you discuss something else. It is a numerical experiment to show what happens if you use the initial and boundary conditions with atmospheric forcing and finer model domain where the forcing is turned off.

P7L26-32: From this, it is clear that the results presented later as R100 outcome are non-realistic (these are the results of an artificial numerical experiment; no point to compare the results directly with measurements).

P7L33-P8L8: This qualitative/visual comparison is OK, but I would not recommend to focus on it too much. A question would be whether R500 did reproduce the pattern qualitatively better than HBM, for instance.

P8L10-15: It is a numerical experiment, where the forcing is turned off, and the aim could not be to reproduce the observed fields. A comparison with the measurements would be feasible only if forcing is on.

P8L22: A very thin surface layer is picked for the presented maps. Could it be possible to show the same maps, for instance, at 5 m depth?

P9L2-5: Is this effect caused by the fact that you have initial and boundary conditions taken from a model output with forcing and the maps presented are from a sub-region without forcing? It is not clear a priori what is related to the natural variability and what to the model set-up.

P9L13-14: In addition to dates, also the time should be referred (these figures are not daily average fields, I suppose).

P9L16-22: The same question as above – you should interpret the data as outcomes of a numerical experiment.

P10L9-13: What that means? Could cubic spline cause such structures in the derivatives of the fields?

P10L23-25 and P11L6-7: Could you reveal the period of these internal waves?

P11L3: Which Class?

P12L12: Fig. 9 top row is referred to as the vertical velocity at 5m depth, but in Fig. 9c the vertical velocity at the base of the top layer is presented.

P12L29-30: Could the used cubic spline create more structures with the length scale of 1 km (R500 has the resolution of 500m)? Such a scale is well visible in many figures.

P12L31: Correct to "bimodal". What are the three-modal structures?

P13L25: Could such false advection effects influence the results (statistics) in general as well?

P15L15-16 and P15L25-26: How these findings of fast changes during first days of integration can be interpreted? Are these caused by the fact that the initial fields were taken from a model output with atmospheric forcing and R100 was run without atmo-

spheric forcing?

P17L26-28: Please, give references.

P17L29-30: It is true, if you mean 2D distributions. However, there are publications based on ferrybox and glider measurements covering large areas and presenting statistics of submesoscale variability (even in the Baltic).

P17L32-P18L1: What do you mean by “hydrodynamical instability” here?

P18L9-10: Please, give references.

P18L14-15: Such 10m spots are not relevant in this context.

P18L21: Also Fig. 17 is not directly necessary to be presented.

P18L30-31: It is true in autumn-winter, but not in summer in the upper layer of the Baltic where the seasonal thermocline develops.

P20L8-10: This is a crucial point that the forcing was turned off in the high-resolution model domain, but it was still turned on in the model from where the initial and boundary conditions were extracted.

P21L3-4: Is this statement about cyclonic eddies favoring the plankton growth a result of the present study? You could insert a reference.

Tables

Table 1: I hope the number of vertical layers is more than 10.

Figures

Fig. 2: What is meant by cumulatively averaged TKE? Where the wind data come from?

Fig. 5: Could you add time (is it 0:00)?

Fig. 6: Time reference is missing.

Fig. 8: Density anomaly values could be given.

Fig. 11: Check the caption for English.

Fig. 15: What is the location of this image? Is it relevant here?

Fig. 16: It is something else than discussed in the manuscript. Consider dropping this figure.

Fig. 17. I am not sure how relevant this figure is, especially since it is from the ocean while the paper is about the Baltic where the scales are different.

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