A review of “Very high-resolution modelling of submesoscale turbulent patterns and processes in the Baltic Sea” (authors Reiner Onken, Burkard Baschek, and Ingrid M. Angel-Benavides)

Overall rating

This is an interesting study aimed to simulate submesoscale patterns in the Baltic Sea and comprehensively discuss different aspects of the phenomenon. The paper can be eventually published after moderate revision.

There are several major remarks and a handful of minor ones and typos.

Major remarks:

1 In R100 the atmospheric forcing was turned off “*to analyse the kinematic and dynamical properties of STPPs without disturbing effects*”. However, one would expect the that the STPPs generated without and with atmospheric forcing to be substantially different, while the goal of the study is to model STPPs in the Baltic Sea, that is, including all “disturbing effects” existing in reality. In view of the above, I’m not sure that e.g. the main features of the evolution of submesocsale eddy C3 shown in Fig. 10 will be reproduced by R100 with turned on atmospheric forcing. Could the authors present analogue of Fig. 10 with turned on atmospheric forcing?

2 The prognostic run of R500 started from initial and boundary conditions generated by not eddy-resolving HBM on June 1, 2016, and already in 15 days, on 15 June, the R100 was initialized from R500. The 15 day period does not seem long enough to provide a well-developed (populated with eddies) STPPs from not eddy-resolving initial fields. Very high-resolution modelling previously performed in the Baltic Sea (more specifically, in the Gulf of Finland) by Väli et al. (2017) showed that some cyclonic eddies that can be referred as submesoscale creatures in view of the relative vorticity well exceeding *f*, can live more for than a month. The only comparison of the simulated STPPs with satellite imagery for the modelled period showed that the observed cyclonic spiral, the most prominent feature of the Sentiel-3 image (Fig. 4, bottom) had rather sluggish counterpart in R500 and no counterpart in R100 (cf. Figs. 4 and 5). If the R500 started earlier, e.g. on May 1, the observed spiral would be probably reproduced more realistically/reliably. Since the submesoscale eddies can travel for a long distance (Väli et al., 2017) it seems preferable also to take the nested domain for R500 larger, e.g. including the whole Arkona and Bornholm basins.

3 The authors did not seem to be able to find any convincing link between the results of the field experiment “Expedition Clockwork Ocean” and the submesoscale modelling they carried out. The related pieces of text and drawing (Fig. 16)) could be dropped, which would make this long article easier to read.

4 It seems that the authors are not familiar with recent publications on STPPs modeling in the Baltic Sea (Väli et al., 2017, 2018). Meanwhile, based on a 0.125 nautical mile grid model of the Gulf of Finland, Baltic Sea, Väli et al. (2017; 2018) found submesoscale patterns of relative vorticity, absolute horizontal gradient of potential density and many other tracers similar to presented in this paper, so it would be nice to compare one with the other.

Citation:

Väli, G., V. Zhurbas, U. Lips, J. Laanemets, 2017. Submesoscale structures related to upwelling events in the Gulf of Finland, Baltic Sea (numerical experiments), J. Mar. Syst., 171(SI), 31–42.

Vali G., Zhurbas V.M., Laanemets J., Lips U., 2018. Clustering of floating particles due to submesoscale dynamics: a simulation study for the Gulf of Finland. Fundamentalnaya i prikladnaya gidrofizika, 11(2), 21-35, DOI: 10.7868/S2073667318020028 (open access at http://hydrophysics.info)

Minor remarks

P7L5 "*a high-salinity eddy in the Arkona Basin, and mushroom-like patterns east and southeast of Bornholm on 1 and 10 June, respectively*" There is no any high-salinity eddy in the Arkona Basin on 1 June when both HBM and R500/R500NF display the same not eddy-resolving pattern (see Fig. 2).

P7L26 "*An analysis of the prognostic fields of R500_NF yielded an unexpected finding: the tracer fields exhibit much more spatial variability in comparison to the corresponding fields of R500 (see the right panel in Fig. 2)*" To my mind, it is a very expected finding: results of remote sensing (Kubryakov and Stanichny, 2015), modelling (Zhurbas et al., 2008; Väli et al., 2017) , and even laboratory experiments (Zatsepin et al., 2005) showed that mesoscale/submesoscale structures begin to grow rapidly when the wind subsides.

Citation:

Kubryakov A.A., Stanichny S.V., 2015. Seasonal and interannual variability of the Black Sea eddies and its dependence on characteristics of the large-scale circulation, Deep-Sea Research I, 97, 80–91.

Zatsepin AG, Denisov ES, Emelyanov SV et al., 2005. Effect of bottom slope and wind on the near-shore current in a rotating stratified fluid: laboratory modeling for the Black Sea, Oceanology 45(Suppl 1): S13–S26.

Zhurbas, V., J. Laanemets, and E. Vahtera, 2008. Modeling of the mesoscale structure of coupled upwelling/downwelling events and the related input of nutrients to the upper mixed layer in the Gulf of Finland, Baltic Sea, J. Geophys. Res. - Oceans, 113, C05004.

P8L20. It seems worth to compare the tracer patterns of $|\nabla\rho|$ and ζ with that of Väli et al. (2018) simulated in the Gulf of Finland at 0.125 nautical mile grid.

P9L23 It seems worth to compare the relative vorticity statistics with that of Väli et al. (2017).

P10L28. "*The topography of potential density surfaces in the anticyclone shows that the patches are accompanied by large excursions of isopycnals, indicating intense internal wave activity*." ROMS is a hydrostatic model which does not describe internal waves except for near-inertial waves that propagate almost vertically and therefore are hardly able to produce large vertical excursions of isopycnals at short horizontal scales of O(1km). Please comment the issue.

P17L19-22. $Ro \sim \mathrm{O}(1)$ and $Ri \sim \mathrm{O}(1)$ are mentioned as the criteria of submesoscale fronts, but in Fig. 14 the plot of *Ri* is missing (in contrast to the *Ro* plot).

P17L31. Fig. 15 is really a spectacular satellite image of a phytoplankton bloom but in the context of this article, it seems far-fetched because it was received at another time, in another place with other bottom topography, shoreline, stratification, currents, atmospheric forcing… The fact that the Rossby radius in this place is of the same order than that of the Bornholm and Arkona basins does not seem to be a serious legitimation. The authors did not model circulation off the Estonian coast and therefore have no information on whether *Ro* is large enough to attribute the spirals in Fig. 15 to submesoscale structures. I would suggest to drop Fig. 15 and the related piece of text.

P18L30. "*Moreover, salinity was chosen for comparison because it is the primary component controlling the stratification in the Baltic Sea.*" There is some confusion here… That is true that in the whole the Baltic Sea stratification is controlled by salinity due to the presence of a lower layer filled with high salinity water of the North Sea origin. But in the upper layer of 60-m depth (i.e. above the permanent halocline), density stratification is primarily controlled by temperature, especially in Summer when the seasonal thermocline is developed. The 15-m depth salinity in Fig. 17 (right) displays ~0.1 psu excess in the C3 centre which contributes to density stratification as much as the temperature deficit of ~0.3°C, but one would expect that the actual temperature deficit is much larger, e.g. >1°C, and therefore the salinity is a **<u>secondary</u>**

component controlling the stratification in C3 (i.e. the salinity in C3 behaves like a passive tracer). To clarify the issue, please add the 15-m depth temperature to Fig. 17.

Table 2. Were A_H^T, A_H^M [m^4s^{-1}], and A_H^M [m^2s^{-1}] really taken constants? Why the Smagorinsky parameterization was not applied?

Technical corrections/typos

P6L4. *cyle*->cycle

P11L3. Class number is missing.

P12L10. Two “are” in a row

P13L21. Two “is” in a row

P18L23. “spiraliform” . Google Translator doesn’t know such a word.

Table 1. The number of vertical layers is 10. This is a typo, isn't it?

Figs. 9, 10, 11, and 13. Scale for velocity vectors is missing.