Dear Reviewer#1,

Thank you very much for your comprehensive review of our manuscript. Please find below our replies to your comments. Note that below your comments are written in blue while our replies are black.

Reply to your comments of 14 June (os-2019-44-RC1-supplement.pdf)

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(1) 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,

They are indeed substantially different.

Action

see below **(2), (3), (11)**

(2) while the goal of the study is to model STPPs in the Baltic Sea, that is, including all "disturbing effects" existing in reality.

You are right, the goal of this study is *to understand and and interprete the observed features by means of of high-resolution modeling* (see manuscript P1 L21). However, in order to *understand* the features, all "disturbing effects" have to be ignored for the time being. This is only possible in numerical models, where – in the present case – the atmospheric forcing was turned off in R100. Of course, this is a simplification, but simplifications are the strength of any model (not just numerical models) to understand complicated processes. Moreover, the impact of atmospheric forcing on the evolution of STPPs is in the works in a follow-up study.

Action

- New piece of text in the Abstract (new ms P1L7)
- Rewritten Introduction (new ms P4L8-12)
- New piece of text in Section 4 (new ms P8L26-28)
- New Section 4.3 "Impact of atmospheric forcing" (new ms P12L14-33)

(3) 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?

The impact of atmospheric forcing is demonstrated by the new Fig. 8 which is the equivalent of Fig. 7, but with full atmospheric forcing. A comparison reveals that the atmospheric forcing has a dramatic impact on all near-surface variables.

We plotted as well the equivalent of Fig. 10 with atmospheric forcing, but the result was as expected: there was no more a submesoscale eddy C3 at all, at least not in the corresponding location. We will save ourselves the plots.

<u>Action</u>

- New Fig. 8
- Related text added in new Section 4.3 (new ms P12L14-33)

(4) 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.

We do not agree. According to Fig. 3 and P7L22–25, the spin-up time of R500_NF was estimated to 12 days. And the right column of Fig. 2 confirms that already on 10 June the domain is populated with mesoscale meanders and eddies. The mesoscale activity then increases until 20 June but it remains more or less constant thereafter. Hence, 15 June is well suited to initialize R100. Please note that R100 was initialized from R500 (not from R500_NF) which did not provide a realistic estimate for the spin-up period.

<u>Action</u>: none

(5) 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

Why do you say that the spiral is “sluggish“ in R500? In Fig. 4, we compared tracer patterns at the surface which do not provide any information about the magnitude of currents. Moreover, as can be seen in Fig. 5UV below, the magnitude of the currents at the western flank of that spiral exceeds 15 cm/s which is about 3 times larger than the background current < 5 cm/s.

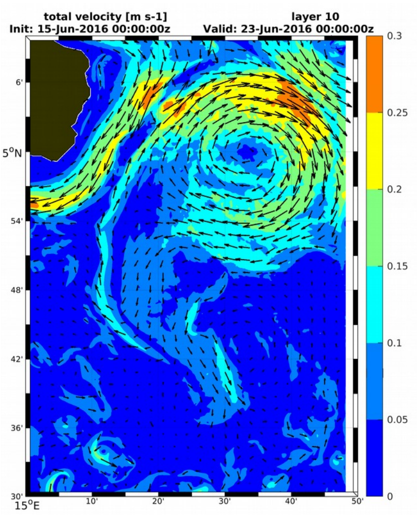

Fig. 5UV: Top-layer horizontal velocity on 23 June in a subarea of R100.

Action: none

(6) and no counterpart in R100 (cf. Figs. 4 and 5).

Unfortunately, on 23 June in Fig. 5 are shown only salinity (left column) and temperature (middle) while velocity vectors were omitted. In Fig. 5EQUIV (below) are plotted the same variables for a zoomed area but with vectors of the near-surface velocity superimposed. Those vectors indicate clearly the centre of the cyclone at about the same position as in Fig. 4a. Hence, there *is* a counterpart in R100.

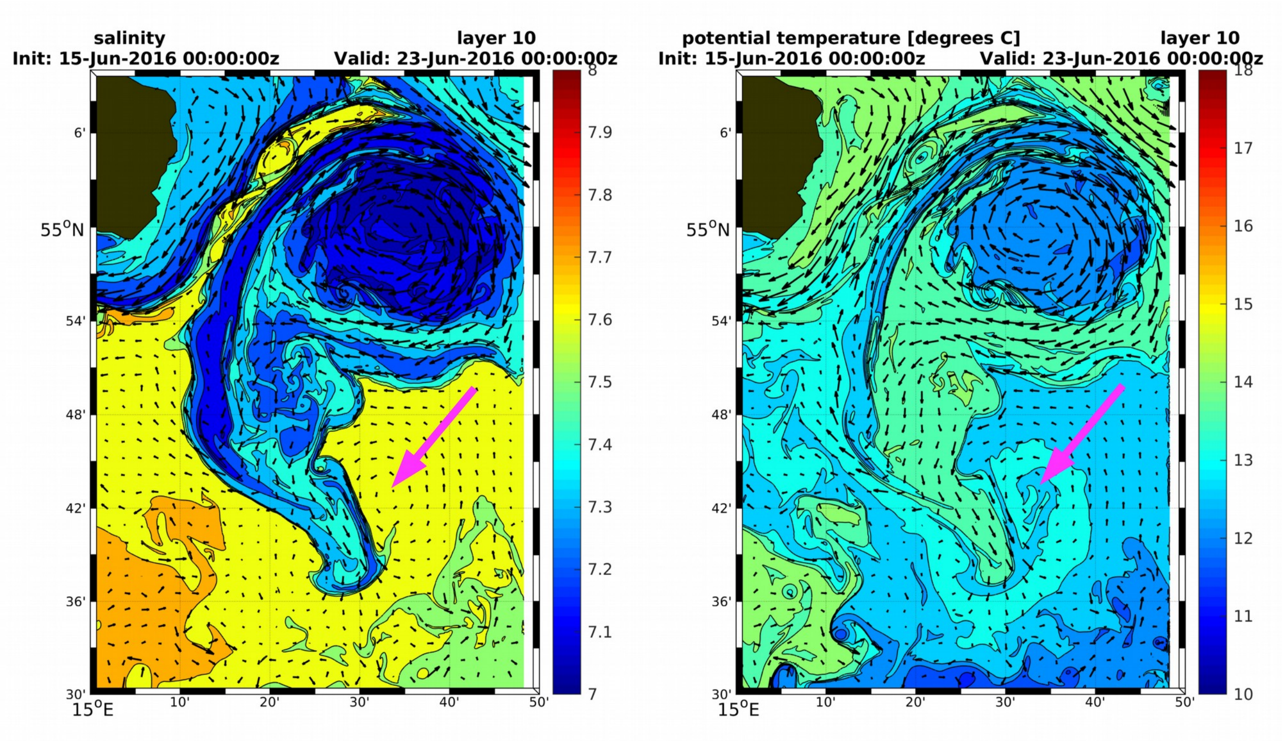

Fig. 5EQUIV: Top-layer salinity, temperature and horizontal velocity on 23 June in a subarea of R100. The magenta arrow points to the centre of the cyclonic spiral.

Action: none

(7) 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.

In our opinion, the issue with the spiral is solved (see above **(5)** and **(6)**). Please note that other observed features of Fig. 4b (C2 and the fronts in the NW corner) are reasonably well reproduced in Fig. 4a. We doubt that the suggested earlier start of R500 would lead to a significant improvement of the model results.
On the other hand, a start of R500 on 1 May would mean to redo the entire manuscript, including all the graphics. Moreover, no atmospheric forcing is available for May; we would have to purchase it from DWD.

Action: none

(8) 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.

Most of the work for this article was done in 2017/2018. A that time, tangible observational results from “Expedition Clockwork Ocean“ were not yet available.

Action
Fig. 16 and the related pieces of text were dropped.

(9) 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)

Action
- Väli et al. (2017) added to references.
- Väli et al. (2018) added to references.
- Related text added
  - in Section 4.2.3 (new ms P11L27-28)
  - in Section 4.4.3 (new ms P15L20-21)

Minor remarks
(10) 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).

You are right!

Action
We have rephrased the corresponding sentence (new ms P7L6-8)

(11) 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 botto#m 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.

Action
- Kubryakov and Stanichny (2015), Zatsepin et al. (2005), Zhurbas et al. (2008), Renault et al. (2018) added to references
- Section 3 was rewritten: new ms P7L29-P8L8

(12) 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.

Hmmm … in priciple, a comparison of $|\nabla\rho|$ (Fig. 5) with Fig. 5 in Väli et al. (2018) is possible, but just saying "The structures resemble each other" would be pretty poor! Same with ζ (Fig. 6b in our manuscript, Fig. 4 in Väli et al. (2018)). We shouldn't rush it!

Action: none

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

Indeed, that would be worth an effort. However, a comparison is impossible, because in our manuscript we have statistical information for $\zeta/f < 0$, $\zeta/f > 0$, and $\zeta/f < -1$, while Väli et al. (2017, their Fig. 8) provide statistical numbers for Ro<-1 and Ro>1. A comparison is possible for Ro<-1, but this would be misleading because it depends on the length of the coast!

Action: none

(14) 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.

It is known to the authors that internal waves are not correctly reproduced in hydrostatic models. As none of us is an expert on internal waves, we have explored the corresponding literature and found the following relevant papers:
- Wadzuk, B. M., and Hodges, B. R.: Hydrostatic versus nonhydrostatic Euler-equation modeling of nonlinear internal waves. Journal of Engineering Mechanics, 1069-1080, doi: 10.1061/(ASCE)0733-9399(2009)135:10(1069), 2009.
- Vitousek, S., and Fringer, O. B.: Physical vs. numerical dispersion in nonhydrostatic modeling. Ocean Modelling, 40, 72-86, doi: 10.1016j.ocemod.2011.07.002, 2011.
- Shakespeare, C. J.: Spontaneous generation of internal waves. Physics Today, 72, 6, 34, doi: 10.1063/PT.3.4225, 2019.

Wadzuk and Hodges (2009) found "*The hydrostatic model cannot replicate basin-scale wave generation into a solitary wave train, whereas a nonhydrostatic model does represent the downscaling of energy. However, the hydrostatic model produces a nonlinear traveling borelike feature that has similarities to the mean evolution of the nonhydrostatic wave.*"

Similar results were obtained by Vitousek and Fringer (2011). They say that numerical solutions of internal waves when modelled with second-order accuracy in time or space will be realistic only when $\lambda=\Delta x/h1< O(1)$ or $\Delta x < h1$, where Δx is the horizontal grid spacing, and h1 is the depth of the interface. Our case (Δx = 100m, h1 ≈ 10m ⇒ λ=10) is comparable to the λ=8 case shown in Fig. 7 of Vitousek and Fringer (2011). The authors say "*When the lepticity is increased to k = 8, numerical dispersion is so large relative to physical dispersion that the nonhydrostatic and hydrostatic results are nearly identical, as shown in Fig. 7. This result agrees with Marshall et al. (1997) who conclude that at coarse horizontal resolution, hydrostatic and nonhydrostatic models give essentially the same numerical solutions. Although the results look qualitatively similar, the nonhydrostatic result is far too dispersive due to the numerical dispersion, and so the width of the leading wave is larger than the "exact" result shown in Fig. 6.*"
Hence, the properties of internal waves in R100 are obviously not correct, even if R100 were nonhydrostatic. However, this impacts mainly the width of the leading solitary wave and reduces the phase speed of the hydrostatic wave, but the amplitude is almost not affected.
From a video of the vertical speed at 10-m depth in R100, we estimated the period of internal waves to τ=4 h=14 400 s which is equivalent to an angular frequency $\omega \approx 4\times10^{-4}$ s^{-1} . This is comparable to the buoyancy frequeny at the same depth of about 5×10^{-4} s^{-1} but not comparable to the (near)inertial frequency of about 10^{-4} s^{-1} as mentioned by you above. Therefore, the frequency of the internal waves in R100 is intermediate and they propagate both horizontally and vertically (see Shakespeare, 2019).

Action

New pieces of text added in Section 4.2.3, new ms P11L15-21

(15) P17L19-22. Ro~O(1) and Ri~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).

Action

- A plot of the Richardson number has been added (new Fig. 17)
- The text has been changed accorcingly (new ms P19L10-12);

(16) 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.

Action

Fig. 15 was dropped and the corresponding text in Section 5 removed.

(17) 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 secondary 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.

Action

- We have added the 15-m temperature in new Fig. 18.
- The corresponding pieces of text have been changed in the manuscript (new ms P20L10-12)

As you see, the temperature range is about 0.7°C (from 12.2°C to 12.9°C) and the salinity range around 0.12 psu (from 7.73 to 7.85).

- For a mean salinity of 7.8 and the extreme temperatures 12.2°C and 12.9°C, the density varies by about 0.098 kg m^{-3} (using the Matlab function `potden80.m`)
- For a mean temperature of 12.5°C and the extreme salinities 7.73 and 7.85, the density varies by about 0.092 kg m^{-3}.

Hence, the density is controlled both by temperature and salinity at approximately equal parts.

(18) Table 2. Were A^T_H , A^M_H [m^4s^{-1}], and A^M_H [m^2s^{-1}] really taken constants? Why the Smagorinsky parameterization was not applied?

The answer is very simple: in the ROMS wiki, the Smagorinsky parameterization is not listed as an available option. See https://www.myroms.org/wiki/cppdefs.h

Action: none

Technical corrections/typos

(19) P6L4. cyle → cycle (→ record) Action: done (P5L31)
(20) P11L3. Class number is missing. Action: done (P11L33)
(21) P12L10. Two "are" in a row Action: done (P13L23)
(22) P13L21. Two "is" in a row Action: done (P15L6)
(23) P18L23. "spiraliform" . Google Translator doesn't know such a word.
But dict.leo.org (German ← → Englisch) does! Action: none
(24) Table 1. The number of vertical layers is 10. This is a typo, isn't it?
See below **(33)**
(25) Figs. 9, 10, 11, and 13. Scale for velocity vectors is missing.

Action:
Scales for velocity vectors have been added for new Figs. 11, 12, 13, 15, 17

Reply to your comments of 27 June (os-2019-44-RC3-supplement.pdf)
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(26) Re: disturbing effects
This comment is polemic → no reply
(27) Re: Wonderful patterns
This comment is polemic → no reply
(28) Re: C3 a virtual creature
See above **(2)**
(29) Re: R500 spin-up time
It was never our intention to populate the domain with OLD eddies. Action: none
(30) Re: sluggish spiral
See above **(5)** and **(6).**
BTW, the local Rossby number is close to 1! See Fig. 5VORT below Action: none

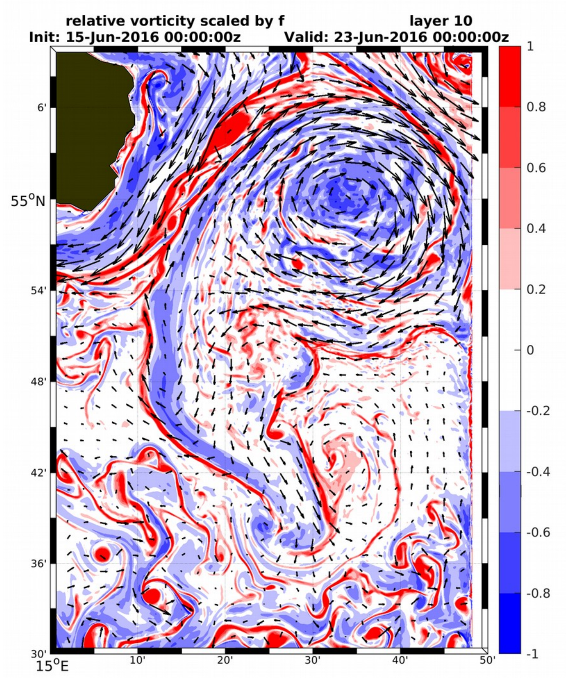

Fig. 5VORT: Top-layer relative vorticity on 23 June in a subarea of R100.

(31) Re: issue with the spiral

This comment is polemic → no reply

(32) Re: Smagorinsky

see above **(18)**

(33) Re: vertical resolution

The water depth at the position of the section in old Fig. 8 is around 50 m. The vertical levels are at about [0 1 3 5 9 15 21 26 32 39 50] m depth. Therefore, the upper 10 m of the section shown in Fig. 8 are resolved by 5 depth levels. Please note as well that the minimum water depth in the model is 5 m. There, the vertical levels are at [0 0.3 0.7 1.1 1.6 2.1 2.6 3.1 3.7 4.3 5.0] m. Further increase of the vertical resolution would require a much smaller time step in order to avoid violation of the vertical CFL.

By the way, in the 2016 version the HIROMB model has 10 vertical layers in the upper 50 m. See as well Väli et al. (2017), P32, 3rd paragraph in the right column “The vertical grid step in HIROMB ...“

Action

none

We hope that our actions satisfy your criticism!

Best regards,

Reiner Onken and co-authors