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Comment on os-2021-86

Anonymous Referee #1

Referee comment on "Properties and evolution of a submesoscale cyclonic spiral" by
Reiner Onken and Burkard Baschek, Ocean Sci. Discuss.,
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A review of "Properties and evolution of a submesoscale cyclonic spiral" by R. Onken and
B. Baschek submitted to Ocean Science.

General comments:

The manuscript describes results from a triple-nested configuration of a submesoscale
cyclonic eddy in the Baltic Sea. The simulation is set up to represent observed situation
from 2016 and aims at elucidating evolution of the cyclonic feature.

The paper addresses a couple of very relevant questions within ocean science, namely
model representation of (sub-)mesoscale eddies in the Baltic Sea, and the evolution and
dynamics of these eddies. The triple-nested Regional Ocean Modelling System
configuration is novel and ambitious. However, I find significant shortcomings regarding
the experiment design, analysis and presentation of the results, as well as text
organization and writing. It is difficult to understand what conclusions of the study are. I
recommend rejection and optional resubmission after a major reworking of all the aspects
of the study. Below I am highlighting the major issues, noting that it is not practical to
comment on all the details at this stage.

Manuscript structure and writing:

The organization of the text and writing make reading and review of the content difficult.
It is hard to disentangle a thread of the narrative as well as the aims, objectives and
conclusions of the study. The parts pertaining to Methods are interspersed with and within
other sections of the manuscript. The Introduction starts with a vague description of the
background, emphasizing lack of understanding of the evolution and dynamics of the

submesoscale eddies and in particular cyclonic spiral-like eddies (pages 1-2) - however, without a definition of the "submesoscale" and "mesoscale" notions in the Baltic Sea region(!) - these two terms are usually defined in terms of the first Rossby radius of deformation (that depends on the local stratification and latitude-dependent Coriolis frequency) - no comment on this or any alternative definition is offered. Neither is it clear what is actually meant by "physical properties", "evolution" or "dynamics" in the text (neither of these notions being sufficiently quantified by the analysis, see below). On page 4 of the Introduction we find some details about the model configuration (which pertain to the Methods section 2) together with the information that the model "creates the mesoscale background for June 2016" ("mesoscale" being undefined) with times reported in UTC, but it is not clear whether the study aims to realistically reproduce the observed situation that day or is meant as a more general modelling study? I could not find answer to this question after reading the entire manuscript. Some more details missing in the Methods section are further found in the Result section 4 (e.g., Table 1 showing the setup of the experiments). A summary of the numerical Results (Table 2) is found in sect. 5 (Discussion) while it fits better in the Results section. The final sections Implications (5.5) and Transferability (5.6) miss the targets. It is incorrect to compare the results from the Baltic Sea to other regions where submesoscale eddies have been reported without a careful discussion of the different dynamical regimes (and different scales for submesoscales). A discussion of the relevance of the study as well as the implications of the existence of the cyclonic spirals for the Baltic Sea region are entirely missing(!). Some of the sentences and expressions are very intricate or unspecified ("submesoscale waveband", "the perceptions are rather fragmentary", "downscaling appears to be perfect", "the nesting procedure does apparently a good job"). An adverb "hence" (as a consequence; for this reason) is used multiple times while the logical consequence is not always clearly emerging from the preceding statement.

Methods and analysis:

Several important details about the model configuration are missing which undermines confidence in the results. The manuscript is very long and contains long descriptive passages or visualizations of the model simulations which bring in very little quantitative and qualitative information (e.g., a very detailed description of the Lagrangian simulations in sect. 4). The reported values of e.g., velocity or frontal tendencies are hard to interpret without a reference to expected values for this (regional!) regime and information what was the sampling (averaging) frequency from the model (are snapshots the daily or hourly averages?). The 23 (!) figures are merely visualizations of the model simulations while the analysis and statistical quantification of the results is nearly entirely missing. Some of the inferred interpretation (i.e., about the Vortex Rossby Waves or types of instabilities) is not (or at least not sufficiently) supported by the results. The references are cited somewhat selectively and not always aptly in various places.

Below I include a non-exhaustive list of questions and issues that need to be clarified in addition to the points raised above. I am not including new references to the existing list since a lot of information and suggestions for analysis and diagnostics are already found in the papers cited by the manuscript, but they have not been used.

Methods:

- Already mentioned, missing definition of the mesoscale and submesoscale in the Baltic Sea and physical background of the study (local stratification, shallow basin, turbulent regimes). As the definition of (sub-)mesoscale is not universal, the direct comparison with studies in other regions is not appropriate unless a care in interpretation of the differences in dynamical regimes is taken.

- Are spiral cyclonic eddies routinely observed in the Baltic Sea, how often, in what parts of the sea? How universal or specific is the study?

- Is the vertical resolution of 10 layers sufficient to quantify submesoscale dynamics in the region? Is the vertical resolution uniform, is the halocline/pycnocline sufficiently resolved?

- Is the resolution of 33m justifying the hydrostatic approximation of the model?

- Is the analysis performed on the snapshots or model output averages (daily or hourly averages)? This will certainly impact the (extreme) numerical values reported in e.g., sect 3.1-3.2.

- In sect. 4 we find a piece of information "As atmospheric forcing is turned off, the dynamics of R33 can be considered as adiabatic and nearly frictionless because no explicit vertical mixing is specified, and the biharmonic diffusivity coefficient is extremely small". First, this information pertains to Methods section, second, it should also be further elucidated. As the atmospheric forcing is not turned off in the parent simulations (if I understand correctly), is R33 indeed adiabatic (how the nesting procedure carries)? What is the mechanism for the MLD changes reported in sect. 3.5 if the atmospheric forcing is entirely off? Is this realistic?

- Results about the frontal tendencies (sect. 3.1.) hard to interpret. Please see Capet et al 2008 for more details. The fact that some other studies in other regions like coast off California showed that the major contribution to the frontogenetic tendency comes from the straining deformation of the horizontal velocity, while the vertical straining is the main driver of frontolytic processes, does not mean that this is also the case for the presented study in the Baltic Sea. Please verify the statement by appropriate diagnostics (Capet). What is the underlying theoretical explanation and meaning of this result?

- Sect. 3.2 and Fig. 5. Reporting the extreme values of geostrophic vs ageostrophic velocity with numerical values is superfluous if the local Rossby number is also used (also values of the velocities or the "existence of multiple maxima" compared to other studies is not meaningful). Section 3.2. c be entirely removed.

- Sect. 3.3. Please explain what is the rationale of using the vertical component of the relative vorticity only in the presence of the frontogenesis processes.

- Sect. 3.3. Please justify the statement: "While in adiabatic flow, $\zeta > -1$ should hold, $\zeta < -1$ is probably a consequence of the diabatic contribution from the biharmonic diffusivity, caused by the extreme horizontal density gradient" This can be verified by comparing the contribution of the terms in the vorticity evolution equation. Is the "diabatic contribution from the biharmonic diffusivity" an artifact of the numerics? I do not quite understand this statement as the triple nest simulation was reported in Sect. 2 to have no explicit mixing? What is the (dynamical, theoretical) interpretation of the evolution showed in Fig. 6? The statement: "indicating a bimodal structure with extremely strong control of both vorticity and even more strain" is vague and brings in no quantitative information. One could present PDFs of vorticity/strain or vertical profiles of Kurtoses/Skewnesses instead and interpret the information. What is the meaning of comparing these values to the study in the Gulf Stream (Gula) - rings vs spirals and different dynamical regimes?

- Sect. 3.4. starts with: "In ROMS, the integral is actually computed as a sum from the bottom upwards and also as a sum from the top downwards, resulting in a linear combination of the two, weighted so that the surface down value is used near the surface while the other is used near the bottom (Hedström, 2018). Thus, the near-surface vertical velocity largely reflects the divergence pattern in the surface layer..." This information pertains to the Methods section. I do not quite understand how the sentence after "Thus" results from the preceding statement, and what the statement after "Thus" implies - what else should the near-surface vertical velocity reflect? Also I do not quite understand why (Hedström, 2018; technical manual for OCS BOEM study) is cited to support a statement about the vertical velocity computation in ROMS. If this paragraph is meant to cast a confidence on the model vertical velocity output, it has had exactly an opposite effect on me... The text that follows, and Figure 7 are too descriptive. Please quantify the "progressive decorrelation" or quantify by time scales w by e.g., spectral analysis and interpret. The spatial pattern of w is not quantified either. What processes create these patterns? Please see the cited references at the end of sect., 3.4. for tips on relevant diagnostics.

- Sect. 3.5 + Fig.8. Vertical stratification, focus on MLD. Hard to interpret the absolute numbers reported, can be skipped or information-compressed using statistical diagnostics and/or vertical profiles. What processes generate the variations of the MLD? I don't understand the immediate relevance of the statement: "The subduction of the 5.3 kg m⁻³ isopycnal in the center, the contemporaneous rising at the periphery of the spiral, and the leveling of the MLD are clear indicators for restratification by mixed-layer instability (Boccaletti et al., 2007; Fox-Kemper..." This should be verified for the particular regime relevant to the regional study presented and particular simulation with appropriate analysis (see cited references).

- Sect. 3.6. Vortex Rossby Waves. Again, the relevance and transferability of the other cited studies in the ocean and atmosphere that interpret the spiral patterns of various

quantities as VRWs is not immediate! Note that the interaction of the internal wave field with vortex field can also generate spiral features - the distinction between IWs and VRWs can be supported by e.g., estimation of the VRWs frequency from the dispersion relation and spectral analysis of w , in addition to analysis of the sources of w (see the cited references).

- Sect. 3.7. The origin of the secondary instabilities should be supported by appropriate diagnostics (e.g., energetics). This section can be also skipped unless its significance (and model realism) is discussed. A comparison based on visuals with results reported from the Gulf Stream not meaningful unless a full discussion of the dynamical regimes is given. A quantitative (model-data) comparison with observations from "Expedition Clockwork Ocean" is entirely missing (there is just a blurry figure 11 included).

- Section 4+5 (Lagrangian analysis). A more detailed information about the Lagrangian simulation technique (time step, in-line, off-line, volume/mass conserving or material points) missing which confounds the interpretation of the "isobaric" vs "isopycnal" property. Text in pPages 21-35 too descriptive and carry very little quantitative and quantitative information. This entire part could be removed and a decent analysis in terms of absolute and relative dispersion in 2 and 3 dimensions should be included to quantify the transport properties associated with the spiral eddy. Unclear statement on p. 38 ("floats in ROMS follow neutral surfaces" - aren't these actually s -surfaces in ROMS?

- Section 5. Should be rewritten after addressing issues pointed out above.

Presentation quality:

The 23 figures should be critically revised since they contain superfluous descriptive/visual information while statistical/analytical information is missing. Please keep to good practices on colormaps outlined e.g., here:
<https://www.nature.com/articles/d41586-021-02696-z>