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Interactive comment on "Rotation of floating particles in submesoscale cyclonic and anticyclonic eddies: a model study for the southeastern Baltic Sea" *by* Victor Zhurbas et al.

Anonymous Referee #4

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This is an interesting study of the reasons behind the predominance of cyclonic spirals of different substances in the marine environment of the northern hemisphere (equivalently, anticyclonic spirals in the southern hemisphere). The explanation is unexpectedly simple but clearly physically relevant. Another valuable finding is the explanation of one of the processes that systematically drives patchiness on the sea surface.

The study is sound and professionally performed. Even though Introduction provides perhaps too a massive flow of the information, it is well written and serves as a good summary to the experts from other parts of the world. The authors use an ocean model with a very high resolution that evidently is able to resolve a number of fine

(sub)mesoscale features. The simulated pattern of eddies fairly well matches the outcome of satellite remote sensing. Most likely this match partially reflects the high probability of having synoptic eddies in certain more or less fixed locations of the Baltic Sea because of the specific geometry of the sea and its shores. Even though this remark is just an observation and not critics, still I recommend making the claim on lines 220-221 a little bit weaker.

To my eyes, the use of words "linear features" (lines 57, 63 and in several occasions below) is misleading; mostly because in hydrodynamics the adjective "linear" is usually associated with properties of the underlying equations and their solutions. Thus, for many readers "linear surface features" would automatically connote "sinusoidal wave trains" even if Walter Munk used this expression in a different meaning of substances aligned into elongated patches or stripes (like mentioned on line 239).

I recommend to mention that a "sister" phenomenon of the quick regrouping of particles to cyclonic spirals (lines 302–304; Väli et al., 2018) occurs in the periphery of intense marine eddies. The associated almost explosive increase in the particle concentration in was first explored in detail in (Samuelson et al., 2012). The increase in the local concentration occurs in the rim of an anticyclonic eddy differently from that in cyclonic ones. It happens basically because of the interaction of outward motions of particles with the field of particles outside the eddy. A little bit outside of the scope of the manuscript is an attempt to quantify the associated systematic changes to the density of particles, with much lower resolution than the simulations in this manuscript, for a subbasin of the Baltic Sea (the Gulf of Finland) in terms of so-called finite-time compressibility (Kalda et al., 2014).

The entire study, in essence, signals that the well-known asymmetry of atmospheric cyclonic and anticyclonic eddies (all strong storms are cyclonic) becomes evident also in the field of ocean eddies. I guess that the reader would enjoy some comments on whether the established strong asymmetry of the rotation rates of eddies of different sign is a local property (of densely packed eddies?) or reflects a generic property of

marine eddies. This asymmetry may affect more widely the statistical parameters of surface flows (Heinloo and Toompuu, 2012) as in such occasions the average curvature of trajectories of water parcels is predominantly of one sign.

The use of English is clear and appropriate but may need at places minor corrections (e.g. on line 286 it should probably by “the radial distance” but simply “submesoscale cyclones” would do on line 292).

Minor comments

I recommend to be careful with the use of “rotation” of particles and to clearly distinguish rotation of particles around their own centre and (rotary) motion of particles along curved or circular trajectories. For example, the words “floating particles rotation” (line 66) could easily be misinterpreted. Similarly, “the rotation of a particle /—/ is accompanied /—/ by a shift” is ambiguous.

Some parts of the manuscript contain too long paragraphs that make it complicated to follow the line of thinking. The first paragraph of Introduction covers 27 lines that is far too much. Also, in several occasions the sentences could be split into parts for clarity.

Equation (4): it is not clear how w(0) is calculated; also there is no need for square brackets in the first expression.

Line 106: n.m. obviously stands for nautical mile but it is better to explain the abbreviation.

Line 169: perhaps it would be more exact to speak about divergence/convergence of the surface velocity field.

Line 219: use the Polish ń in Gdańsk.

The claim on line 232/233 is just a repetition of the same claim on lines 224-225.

Table 1 could be better placed in Appendix.

Line 261: “The statistics . . .” contains, to my eyes, too much jargon and simply “mean” (line 267) would do the same job as “ensemble mean” (but “ensemble mean curve” on line 282 has clear meaning).

References

Heinloo, J., Toompuu, A., 2012. A modification of the classical Ekman model accounting for the Stokes drift and stratification effects. Environ. Fluid Mech. 12 (2), 101–113, doi: 10.1007/s10652-011-9212-5.

Kalda, J., Soomere, T., Giudici, A., 2014. On the finite-time compressibility of the surface currents in the Gulf of Finland, the Baltic Sea. J. Mar. Syst. 129, 56–65, doi: 10.1016/j.jmarsys.2012.08.010.

Samuelsen, A., Hjøllo, S.S., Johannessen, J.A., Patel, R., 2012. Particle aggregation at the edges of anticyclonic eddies and implications for distribution of biomass. Ocean Sci. 8 (3), 389–400, doi: 10.5194/os-8-389-2012.

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