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 italic.

General Comments

• *The abstract could use rephrasing. As written it appears that generally it's accepted that the rotary characteristics are between cyclonic and anticyclonic eddies are different, and this paper seeks to confirm that. However, in the introduction the authors do point out that their approach is different than most previous work, and this should be highlighted in the abstract. In addition the last sentence of the abstract should be written more clearly to define the three characteristics measured. The way it is worded it was hard to follow until after reading the manuscript itself. Perhaps a numbered list or commas would clarify.*

We will rephrase the abstract to highlight the novelty of the approach. A numbered list of the three characteristics assessed will be added to the last sentence.

• *The introduction could be made stronger by including the importance of these cyclonic spirals alongside underpinning the mechanisms for their prevalence. A good deal of space is dedicated to describing their existence and previous mechanisms of formation, but not much is provided to describe the relevance. For example, what are the biological impacts given that cyanobacteria trace eddies out so well or are there implications for eddy tracking with tracer fields? It would also be helpful to include the distinction between helicity and vorticity here, and what they tell you about a flow (e.g. for the helicity, line 170). The differential rotation parameter could also use a descriptive sentence. This will help the reader understand why you have chosen these particular aspects to compare, as well as connect to the mathematical descriptions provided later. Lastly a more thorough literature review is needed with respect to others investigating the impacts on tracer fields in anticyclonic and cyclonic eddies. For example Brannigan 2016 or Brannigan et al 2017.*

We will add to the Introduction some sentences on biological/ecological impact of the spirals and provide more thorough review of recent literature on tracer fields in anticyclonic and cyclonic eddies including Brannigan (2016) and Brannigan et al. (2017). Since the helicity and differential rotation parameters are introduced later in Material and Methods chapter, we do not think it is worth to discuss them in Introduction. The distinction/relation between vorticity, horizontal divergence, helicity and differential rotation will be discussed in more detail in Material and Methods chapter.

• *The manuscript should more clearly state that the authors are not seeking to explain the skewed tails of the vorticity distribution, only the dominance of the cyclonic spirals seen in tracer fields from satellite images. The previous studies cited provide the mechanisms favoring cyclonic spirals, and also explain why tracers highlight them over anticyclonic spirals. However in this manuscript, although Line 65 does a good job highlighting the objectives, throughout the remaining text the wording leaves it somewhat ambiguous what the authors exact intentions are with respect to both aspects of the problem. For example in the abstract and elsewhere using 'formation of spirals' is slightly misleading since the spirals are already there with respect to the velocity field. Perhaps including references to the tracer field when using this description would help clarify.*

To highlight the objectives more clearly, we will move paragraph containing Line 65 and the objectives to the end of the Introduction chapter (the last read statement is better remembered). Also we will supplement 'formation of spirals' wording with 'in the tracer field' throughout the manuscript.

• Section 2.2 could be clarified more, specifically the rotary parameters. These should be connected to the introduction as well to give the reader intuition into the authors interpretations of them. Is _$\delta = r_2 - r_1$? This would help clarify the sign dependence of Helicity. How is $\omega(0)$, the vorticity at the center of the spiral, diagnosed here given that particles presumably tend towards stationary at the exact center? A more precise definition is needed. Does this model have the resolution to produce such results?

Right, $\delta = r_2 - r_1$, where r_1 and r_2 are the radii of two consecutive loops of a synthetic Lagragian particle, we will clearly state it in the revised manuscript. The modelled velocities were bilinearly interpolated to the current position of the particle within the grid cell. Therefore if the initial position of the particle was taken close enough to the exact centre of the eddy, the radius of the loop r would be sufficiently small, e.g. smaller than the grid cell size $dx, dy = 232$ m. The frequency of particle's rotation at $r \approx 0.5dx \approx 100$ m was taken for $\omega(0)$.

To clarify the definitions of δ and $\omega(0)$ we will include the above paragraph to the revised manuscript.

• The Lagrangian particle simulations and the comparison of gridded to linearly seeded particles to understand the spiral formation should be expanded on. Can you provide justification for using surface constrained particles to understand a 3D tracer field. Do you have an idea of which mechanism for creating cyclonic spirals is most prevalent? That is submesoscale fronts are ubiquitous, what percentage of spirals tends to come from advection of particles into a strong eddy field versus reshaping of linear tracer features?

We realize that a scenario presented in Chapter 3.3 where the spiral in the tracer field is formed from synthetic floating particles seeded on a line passed through the centre of a mature submesoscale cyclonic or anticyclonic eddy is barely realistic because one cannot imagine a natural phenomenon that could provide such kind of seeding. However, the two other scenarios, i.e. when the spirals come from advection of uniformly seeded floating particles into velocity field of a mature eddy(see Chapter 3.2) and from reshaping of a linear tracer feature aligned to the density front in the course of development of a kind of frontal instability (the Munk's hypothesis), seem quite realistic. In our opinion, depending on the specific conditions of the ocean environment, either the first or second of two realistic scenarios may prevail.

We will add the above discussion to Discussion and Conclusions chapter.

As to the justification for using surface constrained particles to understand a 3D tracer field – we are not ready to discuss the issue which seems to be outside the scope of this study.

• Do you think a seasonal pattern could be isolated using these methods? For example, with an intense eddy field in winter perhaps the differences between cyclonic and anticyclonic statistics are more prominent.

We do not exclude that there is some seasonality in the differences between cyclonic and anticyclonic statistics; it deserves a separate study and right now we have nothing to add to the manuscript on the issue.

• The tables should be moved to an appendix.

Table 1 will be moved to Appendix.

Figure 8 should be described more thoroughly as it is the most compelling evidence in support of the hypothesis. Are the confidence intervals based on the three days of model output combined into one and are they from bootstrapping or some other method? It would be helpful to explain how these days are included. Additionally why did you choose these snapshots? Do other snapshots show similar statistics?

We will add an intuitive explanation of why mesoscale cyclones rotate faster than anticyclones and why helicity in cyclonic(anticyclonic) eddies is negative(positive) as follows.

"The physical intuition for faster spinning of cyclonic eddies vs anticyclonic eddies can be gained from conservation of potential vorticity in a fluid parcel (e.g., Väli et al. (2017): $(\zeta + f)\rho_z = const$, where ρ_z is the vertical gradient of density. If the parcel undergoes ultimate vertical stretching ($\rho_z/\rho_z(0) \to 0$, where $\rho_z(0)$ is the initial value of ρ_z) given that it does not spin initially ($\zeta(0) = 0$), it will acquire unlimited cyclonic rotation: $\Omega = \zeta/f = \rho_z(0)/\rho_z - 1 \to \infty$. On the contrary, if the parcel undergoes ultimate vertical squeezing ($\rho_z/\rho_z(0) \to \infty$), it will acquire anticyclonic rotation limited from above: $\Omega \to -1 + 0$. The above considerations make it clear why in Fig. 8 in all cyclonic eddies $\Omega_0 > 1$, while in all anticyclonic eddies except one the rotation speed is within $-1 < \Omega_0 < 0$. As to the positive(negative) value of helicity in anticyclonic(cyclonic) eddy, it can be intuitively understood taking into account that the related upwelling (downwelling) implies potential energy loss and, therefore, relaxation of the eddy."

The confidence intervals are based on processing of 18 cyclonic and 18 anticyclonic eddies identified on three snapshots related to 3 days of model output; the 18 items are considered as a sample of a normally distributed quantity. The standard statistical method of assessing the confidence intervals based on the Student's t-distribution is used, and we will point it out in the revised manuscript. The three snapshots were chosen just because there were satellite images available for the dates – we will point it out in the revised manuscript too. We did not process other images because it was a time-consuming job, and we did not see any reason it would show different statistics.

• The conclusion should include a paragraph at the end with a summary and the thesis of the paper reiterated.

We will include such a paragraph with the summary reiterated.

Specific Comments

• What are the minimum and maximum vertical resolutions? Does this adequately resolve the helicity?

GETM belongs to the family of terrain-following models, so the vertical resolution is spatially varying. In the shallow region, the cell thickness in the surface layer was less than 0.5 m. In the study area (Gdansk Bay, SW Baltic Proper) the uppermost cell thickness did not exceed 1.8 m and could be regarded as relatively high resolution. Therefore, the authors feel that the helicity was adequately resolved by the model. Also we see no reason to argue that spatially varying vertical resolution could bias estimates of helicity.

In view of this remark, we will add info on the vertical resolution in the surface layer to the revised manuscript.

• D'Asaro 2019 might be a good reference to include as an in-situ observational compliment.

We will add D'Asaro et al. (2018) to the References and a short description of their main finding to the Introduction.

• Please fully write out dates so there is no ambiguity between Europe and the U.S. etc. For example, the way the date is presented in the caption of Figure 4 is ideal.

Everywhere in the manuscript we will adhere to the European/British style of writing dates: dd-mm-yy, for example, 15 May 2015.

• *Can you describe the physical intuition for the rotary characteristics of the spirals? For example why does it physically make sense that cyclonic eddies would spin faster?*

The physical intuition for faster spinning of cyclonic eddies vs anticyclonic eddies can be gained from conservation of potential vorticity in a fluid parcel: $(\zeta + f)\rho_z = const$, where ρ_z is the vertical gradient of density. If the parcel undergoes ultimate vertical stretching ($\rho_z/\rho_z(0) \to 0$, where $\rho_z(0)$ is the initial value of ρ_z) given that it does not spin initially ($\zeta(0) = 0$), it will acquire unlimited cyclonic rotation: $\Omega = \zeta/f = \rho_z(0)/\rho_z - 1 \to \infty$. On the contrary, if the parcel undergoes ultimate vertical squeezing ($\rho_z/\rho_z(0) \to \infty$), it will acquire anticyclonic rotation limited from above: $\Omega \to -1 + 0$. The above considerations make it clear why in Fig. 8 in all cyclonic eddies $\Omega_0 > 1$, while in all anticyclonic eddies except one the rotation speed is within $-1 < \Omega_0 < 0$. As to the positive(negative) value of helicity in anticyclonic(cyclonic) eddy, it can be intuitively understood taking into account that the related upwelling (downwelling) implies potential energy loss and, therefore, relaxation of the eddy.

The above reasoning will be included to the Discussion and Conclusions chapter.

• ***Section 2.1*** *- Please include the method used to interpolate the topography and initial conditions etc.*

We will rewrite this section as follows.

Previous text on line 99: “The digital topography of the Baltic Sea with the resolution of 0.5 nautical miles was obtained from the Baltic Sea Bathymetry Database (http://data.bshc.pro/) and interpolated to the resolution required.”

will be replaced with:

“The digital topography of the Baltic Sea with the resolution of 0.25 nautical miles (approximately 500 m) was obtained from the Baltic Sea Bathymetry Database (http://data.bshc.pro/) and interpolated bi-linearly to approximately 250 m resolution.”

Please notice, that there was also a mistake in the original text, for which the authors apologize.

Previous text on line 104: “For the open boundary conditions the one-way nesting approach is used and the results from the coarse resolution model are utilized at the boundaries.”

will be replaced with:

“For the open boundary conditions the one-way nesting approach was used and the results from the coarse resolution model were utilized at the boundaries. Sea-level fluctuations with 1-hourly resolution and temperature, salinity and current velocity profiles with 3-hourly resolution were interpolated using the nearest neighbor method in space to the higher resolution grid. In addition, the profiles were extended to the bottom of the high resolution model.”

Previous text on line 120: “The initial thermohaline field was obtained from the coarse resolution model for 1 April 2015 and interpolated to the high-resolution model grid.”

will be replaced with:

“The initial thermohaline field was obtained from the coarse resolution model for 1 April 2015 and interpolated using the nearest neighbor method to the high-resolution model grid. In addition, as the adaptive vertical coordinates were used in both setups, the T/S profiles from coarse resolution were linearly interpolated to fixed 10 m vertical resolution before interpolation to the high resolution.”

• ***Line Number 158*** *- Should this be* $Hel \ll 1$*? Why does this assumption mean you can write out the helicity with your given formula? Is this what you actually use to calculate Hel or that given in (3)?*

There was an inaccuracy in Lines 168-169 for which the authors apologize. In the revised manuscript we will replace

“If $Hel \ll 1$ in an axisymmetric eddy, it can be presented as $Hel = 2\pi V_r/V_\varphi$, where V_r is the radial component of velocity.”

with

“In the case of the axisymmetric eddy the helicity parameter (3) can be rewritten as $Hel = 2\pi V_r/V_\varphi$, where V_r is the radial component of velocity, and in the case of no differential rotation/divergence in the axisymmetric eddy it can be expressed through the ratio of divergence $D = 2V_r/r = const$ and vorticity $\zeta = 2V_\varphi/r = const$ as $Hel = 2\pi D/\zeta$. In view of continuity the vertical velocity W, which is responsible for upwelling/downwelling in the eddy, is determined near the surface by horizontal divergence D and depth z as $W = zD$.”

Actually we used (3) and not $Hel = 2\pi V_r/V_\varphi$ to calculate *Hel* (because real/simulated eddies are not exactly axisymmetric.)

• ***Line Number 220*** *- It is not clear why this would be a validation of the model.*

In view of this remark the previous text on line 220:

“The possibility to identify the observed vortex pair in the simulated fields can be considered as a validation of the model.”

will be replaced with

“The fact that a vortex pair of almost the same size and orientation was modeled in almost the same place and at the same time as the observed vortex pair can be considered as a validation of the model.”

Technical Comments

• ***Line Number 59*** *- "One may expect that the spirals could also be generated." Does this expectation come from observations? Please state your motivation.*

We will add to here a reference to numerical experiments on floating particles advection (Väli et al., 2018).

• ***Line Number 65*** *- Perhaps: "The objective of this work is to understand the dominance of observed cyclonic spirals by assessing differences between floating particles’ rotation in submesoscale cyclonic and anticylonic spirals using high resolution modelling of the Baltic Sea."*

Thanks a lot! We will replace the text on Line 65 with the above sentence.

• ***Line Number 71*** *- The word ’fabulous’ seems out of place here. Perhaps "The most illustrative optical images...’ would work instead.*

Ok, we will change “Fabulous” for “The most illustrative”

• ***Line Number 75*** *- "eddies, which will be investigated..."*

Line 75 says

“vortex pair consisting of coupled cyclonic and anticyclonic eddies, the latter located at about 30 km to”.

We do not understand the sense of using "eddies, which will be investigated..." to here.

• ***Line Number 150*** - *Please specify that the relation is for the vertical vorticity.*

We will change “vorticity” for “vertical vorticity”.

• ***Line Number 171*** - *Change ’It can be seen easily’ to just "Large values of Dif...’*

“It can be easily seen that the” will be dropped.

• ***Line Number 180*** - *This paragraph could be worded more clearly. Specifically, ’ we utilized’ instead of ’we addressed’.*

We will replace

“Apart from the above defined rotary characteristics of submesoscale eddies calculated from frozen velocity field, we addressed some numerical experiments with the deployment of synthetic floating particles in the modelled non-stationary (not frozen) velocity field, namely, when initially the particles were uniformly distributed on the sea surface, and when initially the particles formed a linear feature (i.e. a line) passing through the centre of a cyclonic or anticyclonic eddy”

with

“Apart from the above defined rotary characteristics of submesoscale eddies calculated from frozen velocity field, we utilized some numerical experiments with the deployment of synthetic floating particles in the modelled non-stationary (not frozen) velocity field, namely, when the particles were uniformly seeded on the sea surface, and when the particles were seeded on a line passed through the centre of a cyclonic or anticyclonic eddy.”