Dear Dr. Vladimir Ryabchenko,

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
… However, I have a few questions and small comments, the answers to which I would like to receive before finally recommending the article for publication.
Specific comments
1. Studying the eddy structures and features, the authors do not refer to the surface salinity fields anywhere. At the same time, salinity is a more conservative characteristic than temperature, especially far from river estuaries, and eddy structures will probably appear clearer in salinity fields. It would be nice if the authors showed salinity fields in Fig. 4,5,6 and commented on the results.

Fig. 6 supplemented by salinity field is given below.

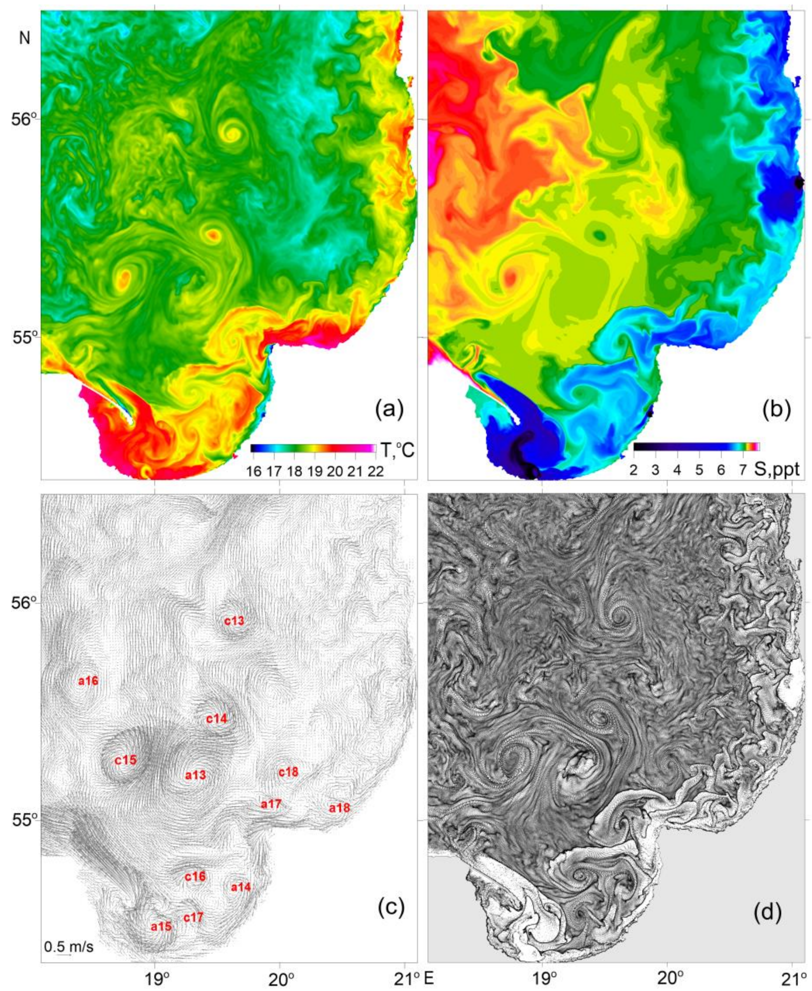

Fig. 6 supplemented by salinity field.

Despite the salinity is believed to be a more conservative tracer than temperature, the spirals in the temperature field seem more pronounced to those in the salinity field. Probably, the reason lies in the fact that the mixed layer under the conditions of the seasonal thermocline is characterized by small but noticeable vertical temperature gradients and vanishingly small vertical salinity gradients. Following Branningan (2016), it can be assumed that the spirals in the surface temperature field are associated with the alternation of upwelling/downelling cells with transverse wave length of the order of 1 km in the mixed layer of a differentially rotating eddy, caused by submesoscale instabilities.

In view of this remark, we will supplement Figs. 4-6 with the salinity panels and add to the revised manuscript the above comment/paragraph.

2. Lines 96-100. The depth field in the domain of the high-resolution model (0.125 nm) has a coarser resolution (0.5 nm). I would like to hear the authors' thoughts regarding the sensitivity of the calculation results to the accuracy of the representation of the field of sea depths, especially in the coastal zone.

The authors apologize for mistake in the manuscript – the BSBD data has original resolution 0.25 n.m and not 0.5 n.m, so the resolution of the data was better than what reviewer might have assumed based on the original text.

Regarding the sensitivity of the calculation results to the representation of the field of sea depths, the authors have the feeling it does not really matter if the original bathymetry had also been on 0.125 n.m resolution. As the GETM is so-called sigma-layer family model, which has the number of layers constant over the computational grid in contrast to the z-coordinate models, some smoothing of the bathymetry is required to reduce the possibility for pressure gradient errors and also to make it more stable numerically. Therefore, we also applied a weak smoothing for the topography and in the end, the impact of the resolution of the original bathymetry was reduced.

3. Line 119. "The high-resolution model accounts only for rivers that flow into the sea within the model domain." The meaning of the phrase is not clear. Indeed, in the high-resolution model, only rivers flowing into this area should and can be taken into account. And what else? The phrase can be deleted altogether.

Indeed, only those rivers that are within the model domain, can be included. The sentence meaning was that we took into account all the rivers also in the high-resolution model even for the short period. For instance Laanemets et al (2011) only used the river Neva in their model simulations. In any case, we will remove the sentence.

4. Line 120. The procedure for obtaining the initial thermohaline fields on the coarse grid should be described in more detail. Please, indicate at least the duration of the run in which these fields were obtained.

Indeed, we have not stated the initial conditions for the thermohaline fields of the coarse resolution model in the manuscript. They were obtained from the Copernicus Marine Service using the re-analysis product for 1989-2015. The corresponding text regarding the model setup, will be improved in the revised manuscript.

5. In the part 2 "Material and methods", the material is not located in accordance with the order in which the results in part 3 are presented. It would be logical to isolate paragraphs Lines 179-183 and 184-185 and modify them in the new section "Synthetic floating particles approach", which is placed after section 2.1 Model setup (after line 130). In this case, the general numbering of sections will change as follows (the title of the last section was shortened): 2.1. Model setup 2.2. Synthetic floating particles approach 2.3. Rotary characteristics of submesoscale cyclones / anticyclones.

In our opinion, the phrase "Synthetic floating particles approach" includes a wide range of problems that is outside the scope of this study, and therefore its use as the title of a subchapter of the manuscript does not seem appropriate (seems too generalized). We would prefer the old, more specific title "Application of synthetic floating particles approach to extract rotary characteristics of submesoscale cyclones/anticyclones", which athough a bit long, but fully consistent with the content of the chapter.

6. Line 240. Why, when analyzing the results of numerical experiments in section 3.3, anticyclone marked as a17 in Fig. 6 missing?

The anticyclone a17 was omitted because this eddy occurred to be too young: it could not be clearly identified two days before 3 July 2015 to seed synthetic particles on a line passed through its centre an therefore provide a numerical experiment on advection of the particles.

We will include the above explanation to the revised manuscript.