Referee comments on the manuscript 'Numerical modeling of dynamics of Russian south waters within the framework of operational oceanography tasks' by A. V. Grigoriev et al.

General comments.

The paper is timely, it fits well into the scope of the EU ECOOP project and is aimed at an important issue of operational oceanography of the Black Sea. The authors have developed a nested numerical model where the parent medium resolution (about 5 km) model is a z-coordinate model used operationally by the Marine Hydrophysical Institute in Sevastopol, and the high resolution (1km) insert is based on the terrain- following version of the Princeton Ocean Model. The authors seem to have carried out a substantial amount of work and demonstrated their ability to upload their modelling results routinely onto a dedicated website (http://www.oceanography.ru/index.php/ru/чёрное-море/результаты-расчета-скорости-течений). My main concern is related to the following three issues: (i) Combination of the Black and Caspian Seas seems to be artificial, the section re the Caspian Sea is too brief and downgrades the quality of the paper; (ii) Too little information is given of the important building blocks of the nested model, and (iii) The model validation / quality control is insufficient. I also suggest that the manuscript is completely re-written in terms of English. The paper can be published in Ocean Science after the issues noted in this review have been addressed.

Specific comments.

TITLE.

What are 'Russian south waters'? In the Black Sea, the parent (medium resolution) model covers the whole sea (see Fig.1) , including territorial waters of 6 riparian countries. The high resolution insert also goes beyond the Russian territorial waters. In the Caspian Sea, the model covers the whole sea as far south as the Iranian coast. The title needs to be reworded to reflect the content of the MS, and to improve on English.

INTRODUCTION.

P1866,L18 and throughout the MS. References in the text are given by numbers (not the style used in OS), whereas the list of references is given by authors names. Referencing should be rectified.

In the Introduction or a literature review section the authors are usually expected to give some review of previous research and how their own research is seen within a wider context. Such section needs to be added. The author may wish to compare their approach to other modelling studies in the region, e.g.

V.L. Dorofeyev et al: 2001. Eddy-resolving model of the Black Sea circulation, Ecological Security of Coastal and Shelf Zone and Complex use of Shelf Resources, 71–82, (in Russian);

Stanev, E.V. et al, 2003. Control of Black Sea intermediate water mass formation by dynamics and topography: comparisons of numerical simulations, survey and satellite data. J. Mar. Res.;

C. E. Enriquez et al, 2005. Hydrodynamic modelling of mesoscale eddies in the Black Sea. Ocean Dynamics, 55: 476-489.

BLACK SEA. P1867,L8. 'Global grid' is probably the same as the basin -wide grid and 'coarse' grid. Clarification is needed.

P1867, 1868. Very little information is given on the important parameters of the model. What turbulence closure scheme was used to calculate coefficients of vertical viscosity/diffusivity? Were any sensitivity studies carried out to obtain the optimised parameters of the closure scheme and what are they? Same question about horizontal diffusion for scalars and vectors. How the vertical grid was set up? The authors use only 18 sigma layers which does not seem to be a lot in comparison with 1 km horizontal resolution. How well the CIL was resolved in the vertical (i.e. how many sigma-layers covered the depth range of the CIL)? It would be good to show the sigma levels on the cross sections e.g. Fig.7. The continental slope in the NE Black Sea is very steep which poses some problems in calculation of the pressure gradient forces in terrain -following grids. How this issue was addressed? The corresponding section of the MS should be extended to answer these questions.

P1867, L7,8. ' Values of parameters in nodes of regional models were calculated first with the use of horizontal linear interpolation of the values in the adjacent nodes of a global grid'. Due to the doming nature of the isohalines/isotherms in the region such method sometimes generate inversion of density (hydrostatic instability). Please clarify what you dealt with such predicaments.

P1867, L7-11. How the lateral boundary conditions were set at the open boundaries for the velocity vectors? The authors state that ' Total fluxes through the section border in regional and global models strictly coincided'. How was this achieved when the velocities were directed outside of the high-resolution domain? The authors further state (L10) that ' components of baroclinic speeds of currents in regional models were equal to corresponding components of global baroclinic speeds'. At the same time the barotropic velocities were different as it follows from Eq(2), and hence the full velocities were different. How can the fluxes coincide 'strictly'? Clarification is needed.

P1867, L12.' ... water flowed into the settlement area...'. The authors probably meant '...into the high-resolution area...'.

P1868, L5-7. What meteo-forcing do you use for your regional high-res model? Same as MHI? How many rivers do you take into account and what data (operational, climatic?) do you use for the fresh water influx and temperature? Did you use the same bathymetry as MHI? Please clarify.

P1868, L10. ' Before the year 2009, calculations for the Black Sea were carried out in the test mode for debugging of technology. ' Have you made any optimisations for the model parameters during the test mode and if yes then which ones? Are results presented in the paper from the 'test mode' only or a mixture? If the latter, how different were the models in the 'test' and 'operational' simulations?

P1868, L15 ' As seen in Fig. 2, the model reproduces both anticyclonic vortexes (vortices?) located on the shelf-slope zone with a characteristic horizontal scale of ~100 km (Az1)...'. The shelf in this part of the Black Sea is very shallow (a few km), so that the eddy AZ1 is located OUTSIDE the shelf-slope zone. Please correct or explain.

P1869, L15-18 ;25-28 and Fig 6. The authors have performed only a qualitative comparison of model results and observation and focus on salinity comparison in their discussion. Salinity is a more inertial quantity in this area of the Black Sea as there are no major rivers and the effects of precipitation/evaporation are relatively small, so one could expect that during the short period of simulation (up to 3 days) salinity would not have changed significantly. Temperature, particularly at the surface is much more responsive. As far as I can infer from Fig.6 (despite the axes labels are tiny) the observed SST at st5 was 9.15 degC , whilst the model value was 7.85 degC, i.e.1.3 deg lower. Some clarification should be given why such discrepancy is considered 'adequate' (P 1874, L11).

P1870, 1871. The model validation is based on qualitative similarity of the eddy patterns, comparison with a single CTD cross section and a comparison with a single map of satellite derived SST, which does not seem to be sufficient to assess the quality of the model. The author state that on the 2 Luly 2009 the RMS (SSTobserv-SSTmodel) was as high as 1.1 degC. Is it a typical value? What about the bias? Have the authors used any other standard methods (e.g. Taylor diagram) to assess the quality of the model output for any extended period of time?

P1871, L19-25. The authors suggest potential reasons for the mismatch between observations and the model . However they do not put their results into a wider context. For example, are the results of the nested model significantly (or at all) better that that of the basin -scale model of the MHI interpolated to the location of observations? If yes , how big is improvement?

P1872-1873. The section related to the Caspian Sea is too brief. It should be either significantly extended or removed from the MS.

P1874.CONCLUSIONS. The conclusions are not substantiated by the discussion in the body of the MS. The authors state that proposed modelling technology ' can adequately monitor...'. What is adequately? What kind of error in T/S/V is acceptable and for what purposes?

P1874. REFERENCES.

Abbreviations have to be explained, e.g. SORBIS.

FIGURES.

GENERAL. Figures are too small, font is mainly illegible .

Fig2. does not have labels (co-ordinates) on the main chart. The scale on the SST image and modelled currents are mismatched. What is the red contour on the SST image? Why there are no currents in the area of the domain near the words ' Model (velocity)'?

Fig.4. Resolution of the picture is too low. The website shown in this Fig. has a strange temper. When I clicked on the icon labelled 05.02.2011, it brings upfront the chart dated 11.11.2011. As the paper describes the website in much detail, the website itself should be corrected.

Fig5-6. The axes labels are too small to read. Please enlarge.

Fig.7. Please show the computational grid. What is the difference between (b) and(c)?

Fig9,10- It is impossible to see any writing on the inserts. Please increase the font.

Fig10. The main figure and the insert have different colour scales so it is difficult to compare. Please use the same scales.

Fig.11. Time=0 (nowcasting). Does it mean that the T/S data were obtained by interpolation of the MHI model without any runs inside the high-res domain?

Fig 12-13. No comparison with observations/ other model is presented so it is impossible to judge the quality of results.

MINOR COMMENTS.

English is substandard and needs attention.