

**Authors express gratitude to the Referee for a review and useful remarks.
We present our responses to each comment of Referee below.**

Anonymous Referee #2

Received and published: 23 November 2011

Review of the paper:

Numerical modeling of dynamics of Russian south waters within the framework of operational oceanography tasks

by A. V. Grigoriev et al.

This manuscript presents the results from the modeling of the Black Sea and Caspian Sea waters dynamics was conducted within the framework of the European ECOOP project and Russian project JISWO on the basis of the Princeton Ocean Model (POM). This issue is interesting and important, addressing the application of ocean models for nowcasting/forecasting of the Black Sea and the Caspian Sea.

There are, however, a number of issues, which are not quite clear, making a MAJOR revision of this manuscript necessary.

There are important general comments related to the present manuscript like:

(1) setting up a model is not explained in detailed (parameters, nesting);

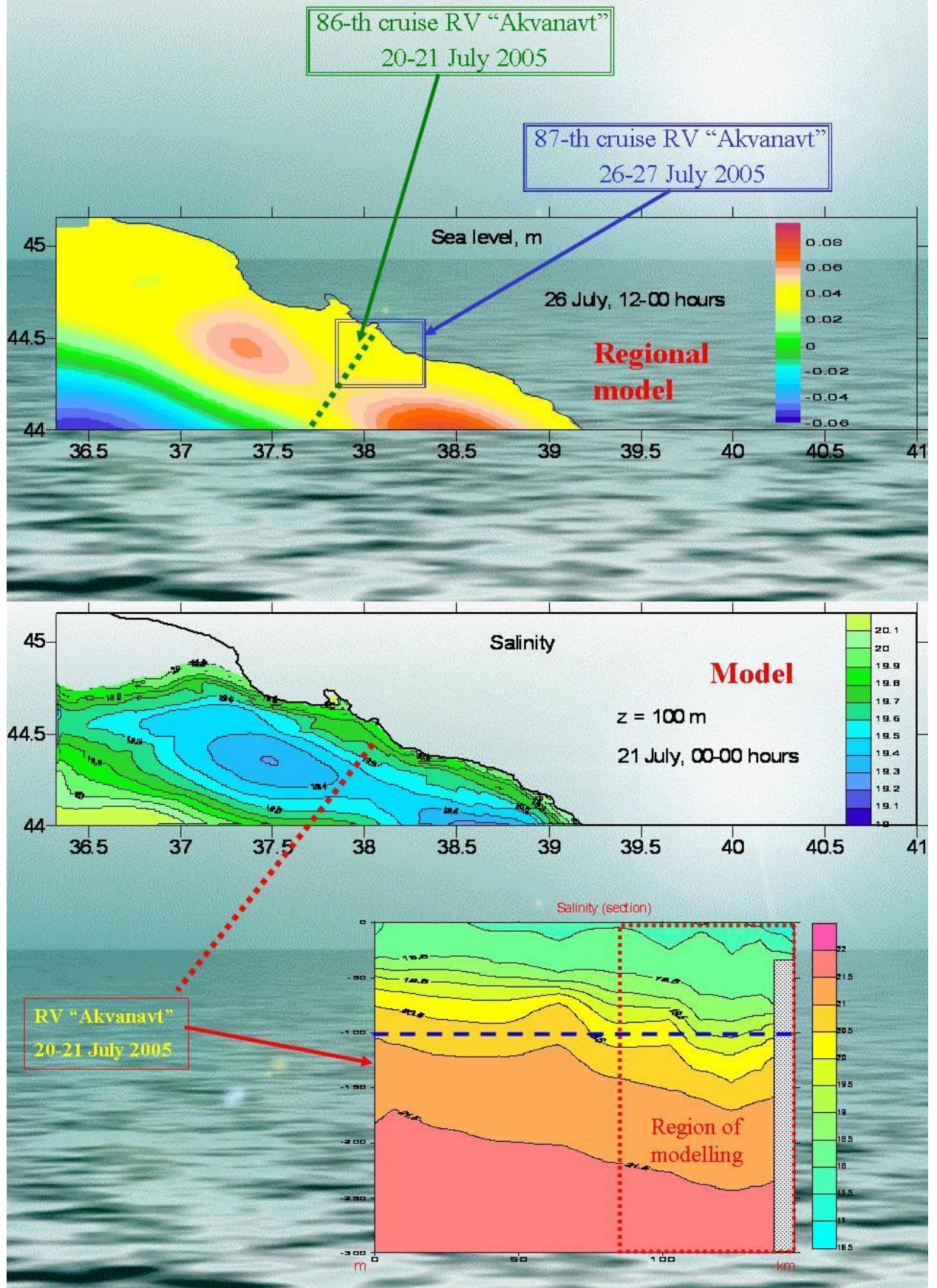
The basic parameters of the model are listed in Table 1. The description of nesting is presented at the page 1867. Also we could refer to the paper from MHI (submitted to the same issue) that contains the detailed description of the model. However we could expand the explanation of the model setting up if it is required.

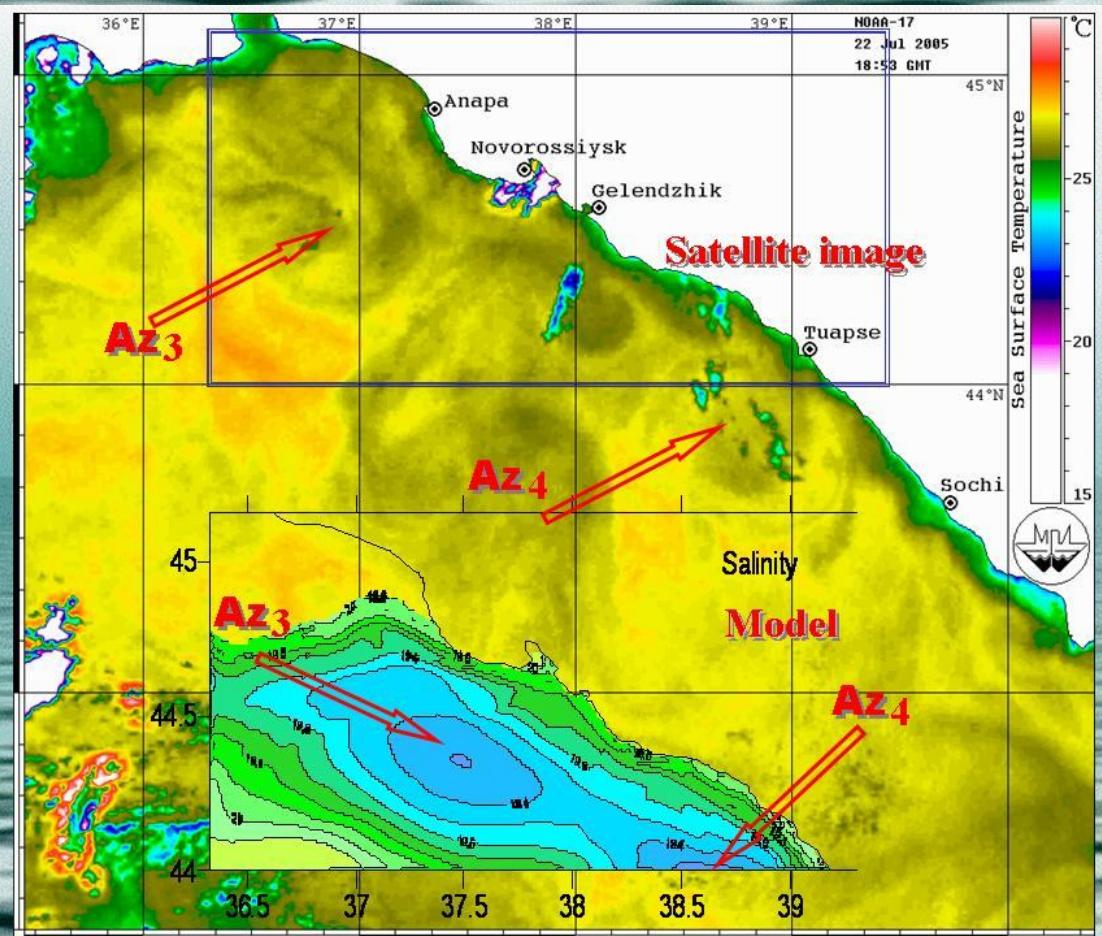
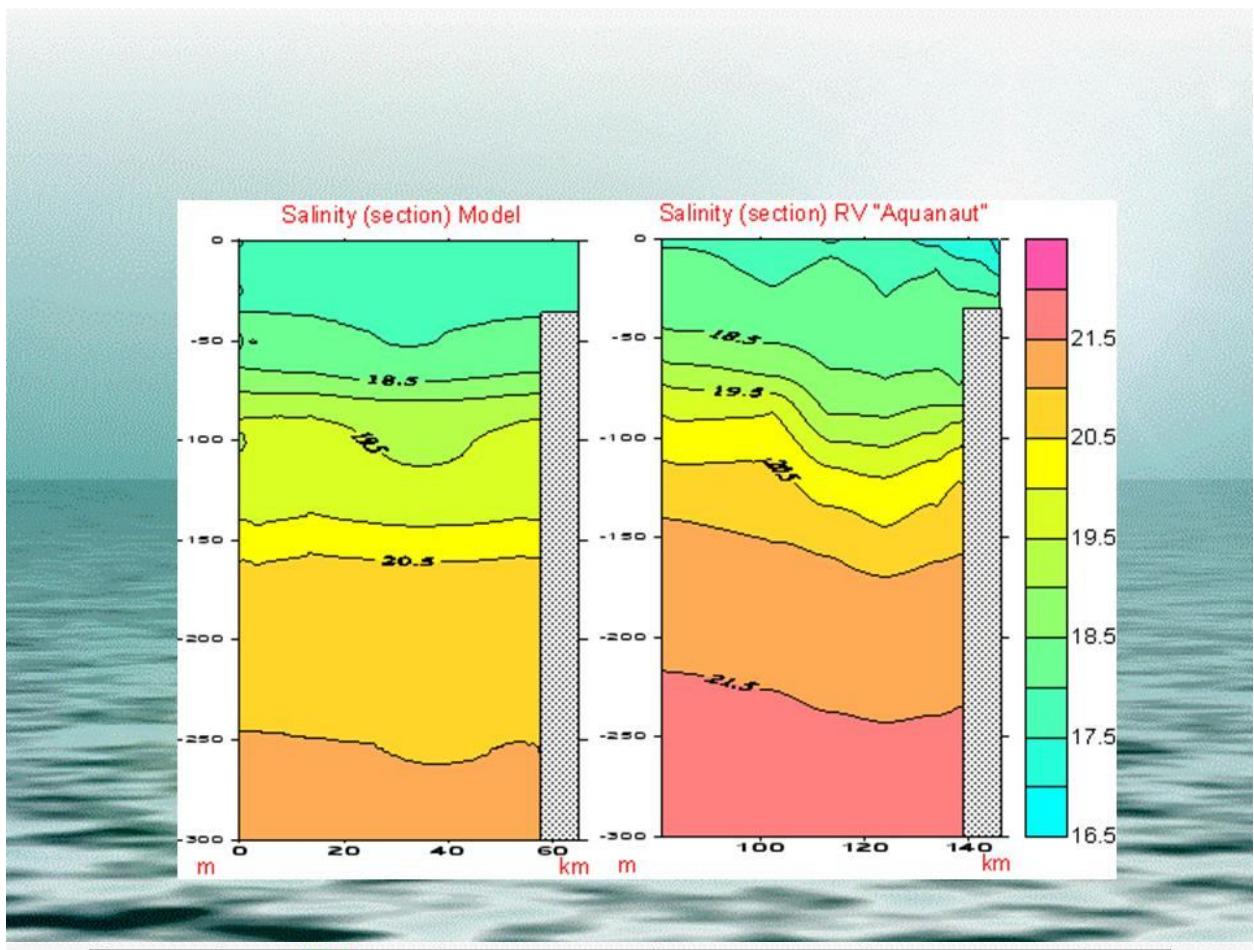
(2) model validation is not sufficient;

The model was validated by comparison with many observational results (see figures below). We could expand the description of inter-comparison by adding these figures and

the discussion on them, if this is necessary.

Contact measurements





Resume of intercomparison between the Russian coastal zone nested model data and the data obtained during the R/V “Akvanavt” cruises and sattelite data

Results of modelling are in general physically identical, increasing a spatial permit of processes allows reproduce in calculations the detail of hydrological structure, which do not find displaying in large-scale models. In particular, eddies with horizontal spatial sizes ~15 km.

Model calculations reproduce observed real dynamic structures.

Their spatial position not wholly well complies with observed data.

Quantitative features of calculate parameters have a good correspondence with a measurements.

Increasing a quality of modelling can be reach, in particular, by means of data assimilation. For instance, SST and sattelite altimetry.

The results from 2003 till 2005

(3) the extraction of scientific information from the results of simulations is somehow limited in the present form of the manuscript etc.

We agree to expand this section

I have the following general comments:

1. The abstract is presented in a very general way and needs improvement.

The abstract will be re-written in more details.

2. The introduction (page 1866) does not present in sufficient details the state of the art. A scientific review of the existing works/publications of the topic is missing and has to be presented in sufficient details the revised manuscript. The reasons for perfoming this work need also an explaination.

A required scientific review will be added.

3. Model set-up is not presented thoughtfully, for example a list pf the parameters used in the Black Sea model (POM) is missing. How those parameter set is being chosen and what were the sensitivities studies to make the existing set-up is not presented. More information about the one-way nesting is also needed as well.

Model set-up would be presented more thoughtfully.

4. Model validation is not sufficiently presented and very descriptive. It needs a substantial improvement in the revised manuscript.

We will improve this section of the paper.

5.Caspian Sea part is very short. Actually, the connection of the two basins is not well presented and seems a bit artificial. My suggestion is that the authors either remove the Caspian Sea from the revised manuscript or present it in sufficient details, including also some interconnections and comparisons between the two model configuration, results, validations, etc.

The modeling of Caspian Sea was provided in the frame of ECOOP. This is why the short description of this work was added to the paper. However we agree with the suggestion of the referee to remove it from the paper in order to put more strength to the description of the Black Sea modeling results.

6. Conclusion part is very brief and not sufficiently well presented and needs substantial improvement.

The conclusions will be presented in more details.

7. Quality of the Figures needs substantial improvement.

The quality of figures will be improved.

Specific comments:

1. Figure 1 – The structure of the presented “System of nowcasting and forecasting of Black Sea water dynamics” is not appropriate and fully informative. The figure needs to be re-arranges. Additionally, the Black Sea horizontal patterns are too small and difficult to rd.

We will work at this figure to make it more appropriate and formative.

2. What are the lateral boundary conditions for the high resolution coastal model? How have they been implemented in the model – page 1867?

The conditions at the lateral boundary: free slip for the flow and zero normal fluxes of salt, heat and momentum. They will be added to the text.

3. Comparisons of the model results with satellite images (Figure 2, and page 1688) is only qualitatively/visually presented and from the figure 2 itself it is totally not clear how well/bad the model compares with the satellite images. Are the patterns from the satellite images and model circulation at the same time? The eddies formation and development presented by the model is not clear.

The patterns from the satellite images and model circulation are taken at the same time. The physical mechanism of the formation and development of eddies is a complicated issue which is beyond the scope of the paper.

4. Screenshot taken by Explorer – Figure 4 (page 1880) is in Russian and could not be read by a wider scientific community. – Is there possibility to pre presented in English.?

Unfortunately, there is no English version of the screenshot.

5. Figure 7 presents qualitative comparisons between CTD data and model salinity . It is not fully clear neither from the test, nor from the figure caption. what exactly Figure 7c presents.

The comments on Fig. 7 will be specified in more details.

6 It could be useful to show als comparisons between the vertical sections for the temperature similar to Figure 7 for the salinity.

This could be done.

7. The circulation patterns on Figure 8 indicate some problems of the lateral boundaries. Please comment.

This is true. The boundary values for the parameters are taken from the basin scale z-model of MHI. There is no such problem when both basin scale and nested models are sigma-models.

8. Page 1870 - the analyses for figure 9 is not appropriate.

We will improve the analysis.

9. The comparison between the SST from the satellite images and model simulations (Page 1870 and Figure 10) is very general und fully insufficient to state about the model performance

Yes, and we made a comment on this in the text (page 1870). Really, Figure 10 illustrates only the qualitative similarity between the satellite image and modeling results. The quantitative estimation of differences between the satellite information and the model results are given in the text.

We will pay more attention to the description of the inter-comparison results.

10. The Caspian Sea description is very short. Either remove the Caspian Sea or present this part in the sufficient details

We already put a comment on that above.

11. The references are given with numbers in the text, but there are nor numbers in the presented References list.

We will specify the subject and use the correct form.

Conclusion: The paper can be accepted after major revision.

The major revision will be done. Particularly, the section on Caspian Sea will be removed from the paper. We are thankful to the referee for attention and valuable comments.

**Sincerely,
Authors**