Ocean Sci. Discuss., 8, C325–C328, 2011
www.ocean-sci-discuss.net/8/C325/2011/

Ocean Science
Discussions

Interactive comment on "Development of Black Sea nowcasting and forecasting system" *by* G. K. Korotaev et al.

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Received and published: 22 June 2011

General comments.

The paper describes a system which could have relevant outcomes concerning scientific understanding and environmental management of the Black Sea.

In the present form, the paper does not seem acceptable for diffusion to a wide public, first of all because it appears addressed to the Black Sea scientific community only; in particular:

several geographic locations are cited, but the paper lack of a geographical map reporting their positions and possibly other information like bathymetry and main circulation features already known;

some oceanographic features produced by the model are defined as correct, but in the

paper there is no indication about general circulation and oceanographic characteristics of the Black Sea neither correspondent sea-truth data.

E.g., Sec. 2.2 ends with: "The Rim current frontal zone can be clearly seen in the left part of the section. The deepening of the thermocline as well as the Rim current jet is attached to the bottom slope. Thus the simulated fields are in a good qualitative agreement with observations. General features of the basin dynamics and stratification are well presented by model results.". But in the text the cited observations are not shown, and nothing is said about what is known "of the basin dynamics and stratification".

Moreover, the quantitative model calibration section is quite poor in terms of quantitative analysis: mainly standard deviation is used, but wider use of other indexes (like Root Mean Square Error, Mean Bias, cross-correlation, etc.) would provide deeper indications. I know that ECOOP and/or MyOceans projects include a Workpackage dedicated to define and to distribute common methods and tools for model evaluation; it seems that no outcome has been used in this work nevertheless the described Black Sea system is part of such projects.

Also Sec. 4.1 (.1 is missing in the test) Calibration of Ecosystem model is very poor in terms of data comparison (a seasonal cycle as the one in fig. 14, but deduced by data, would be needed).

Considering what written up to know, at present state of the paper the following part of the Conclusion does not appear justified: "Together with the circulation model it allows describing evolution of the Black Sea ecosystem. The models have been subject to the qualitative and quantitative tests, which are the essential part of the system. Archive climatic, hydrographical surveys data and measurements from the drifter and profiling floats were used for the models calibrations. Calibration tests showed reasonable accuracy of the system products.".

The paper would also benefit of a Discussion section in which current weakness and possible future improvements of the system are treated.

Specific comments

page 4, second par: how are POM results used “for improving the upper layer thermodynamics in the MHI model”?

nothing is said about models initialization, apart a general statement in the Introduction which is not clear if it would be applicable to the Black Sea system;

the final phrase of Sec. 2.2 would need more considerations. If without assimilation of climatic profiles there is a “slow sliding of the model to its own climate”, a better model calibration would be needed. It would be interesting to know the order of such “sliding” and how it compares to possible deep waters natural trends;

in Section 3 is not specified which of the two models is examined;

why fig. 5 reports anomalies? Salinities should be preferred as for temperature, and in any case it should be specified how anomalies were computed.

Technical corrections:

project acronyms could be explained, also by inserting them in an acknowledgment section (whose addiction appears correct);

page 5, first par: “It is then assimilated into the using the optimal interpolation” something is missed after “into the”;

page 7, Sec. 3.1, first par.: some text is missing here before “Other”: “Analysis of the model salinity has to show that the model simulates the Other specific features for the model calibration are the reproduction. . ..”;

beginning of page 12: it is not clear how “Figure 7 shows, that the altimetry assimilation brings the most significant improvement in salinity field”, please provide some details;

some numbers and more comments in Sec. 3.2.6 would be useful.

Some figures and captions should be improved:

as anticipated, a figure with geographical indications and possibly main oceanographic features known in literature should be added;

the captions should indicate more precisely what is represented (e.g., I suppose that fig. 3 represents model output, but it could have been drawn by satellite data, too, and captions should specify to which model they refer);

fig. 8 and 9 seem do not respect the temporal order;

fig. 11 needs an explanation of drifter colors, moreover maps are too small for understanding the velocity field;

fig. 12 contains 2 plots, but nothing is written about what represents each one;

fig. 14 contains maps (and/or fonts) too small, so values and dates can not be read;

fig. 15 show SeaWIFS map of 11 August, the correspondent model map should be available, so why is 13 August showed?

Fig. 17 would need a better resolution; why do not show a more recent forecast?

Interactive comment on Ocean Sci. Discuss., 8, 917, 2011.