**Ocean Science
Discussions**

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Interactive comment on "Modeling long-term changes of the Black Sea ecosystem characteristics" *by* V. L. Dorofeyev et al.

Anonymous Referee #1

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The MS represents results of a comprehensive study of the long –term variability of the physical and ecosystem parameters of the Black Sea using a three dimensional coupled physical-biological model from 1971 to 2002. The ability to simulate the state of the Black Sea ecosystem is very important for providing background scientific information for the policy makers and achieving a Good Environmental Status. However modelling of the ecosystem is a difficult task and the manuscript is a welcomed step to building a predictive capability in the Black Sea. The MS is worth publication subject to addressing the comments (mostly about the presentation and discussion style) as specified below.

General comments.

It seems that assimilation of remotely sensed SST and SLA data is crucial for the

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model performance. For the period 1971-1993 only the occasional in-situ data were assimilated. I have got an impression that during this period the model was capable to represent seasonal/interannual variations but not the mesoscale variability despite the model was eddy-resolving (7x8 km). The model resolves eddies in the 1994-onwards simulations, when high-res satellite data was assimilated. It suggests that the model skill is heavily dependent on data assimilation. For this reason it will be beneficial if the authors presented results for 1994-onwards without data assimilation for comparison.

Specific comments.

Abstract, last phrase. 'The ecosystem model is able to simulate successfully main observed features and trends of the intense eutrophication phase (from the early 1970s to the early 1990s), but points to its modification to simulate better the ecosystem conditions of the post-eutrophication phase.' It should be stated more clearly what the model can and what cannot simulate. The last statement probably means:' the current model is not capable of reproducing the recovery stage after 1990s'. Please clarify.

1.Introduction.

Page 2041, line 16-18 Check the grammar: 'The period 1971–1994 involved relatively dense hydrographic surveys and was replaced by the availability...' What was replaced? – the period?

Ibid, lines 19-21. 'Reanalysis of the Black Sea dynamics for 1971–1993 performed by Moiseenko et al. (2009) indicates that the seasonal and interannual variability of temperature, salinity and current fields are well resolved'. What does it mean 'well resolved'? What was the metrics? As Moiseenko et al (2009) was published in a difficult to find journal, it is worth to give these details in the MS.

What is the difference between reanalysis performed by Moiseenko et al (2009) and this study?

2. Methodology of simulations

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Page 2042 last para ' ERA-40 . . . for the period 1958–2002 was used' . This section is about the period 1971- 1993, why did the authors 'use' the data for 1958-1970 and 1994-2002? More careful editing is required.

Page 2043, lines 4-15. The text does not provide a clear description of what and how assimilation was done. As it follows from the paper, assimilation is a crucial component of the model and should be better described in the MS. 'The monthly climatic arrays of temperature and salinity . . . were first interpolated on the model grid and then temporally for each day of a year by means of harmonic functions of time.' What does it mean? How many harmonics? An equation will be helpful here.

'They were then assimilated in the model'. What was assimilated- the climatic data or ' . . . three to ten monthly hydrographic surveys per year. . .' (see line 4)?

'The simulations were carried out on time period of 15 yr using climatic atmospheric forcing (Staneva and Stanev, 1998).' The length of the 1971-1993 period is 23 years, not 15. Please clarify. On the previous page the authors state that they use ECMWF 6-hourly reanalysis data, here it is monthly climatic forcing. Please clarify what meteo forcing was actually used.

'The model fields demonstrated periodic oscillations at the end of integration and we have considered them as the climatic one'. Not clear what the authors mean- please clarify.

P2042 line 22-24. 'The coefficients of turbulent exchange of momentum...' ->' The co-efficients of horizontal turbulent exchange of momentum...' 'The coefficients of vertical turbulent viscosity KM and diffusion KH are expressed through the turbulence kinetic energy and the stability parameters which are the functions of the Richardson number.' As the parameterization of the vertical diffusion/viscosity is critical to the accuracy of simulations, the equation and parameters used or a reference should be given.

Page 2044, line 20-22 '. . .the climatic sea surface topography obtained from the as-

similation of the climatic temperature and salinity arrays in POM. . .'. Are the 'climatic temperature and salinity arrays' based on observations or these are the simulated climatologies as in (Moiseenko and Belokopytov, 2008)? Please clarify.

3. Interannual and seasonal variability of circulation dynamics

Page 2046, line 21-25 This section discusses intricate variations sometimes as small as 0.05 degrees of temperature. It would greatly benefit if the uncertainties of simulations are estimated (e.g. by error bars on the graphs) and validation against observations is provided. This in contrast to the biochemical component, which is validated against SeaWiFS data. All information in this section is based on the model output.

At least some comparisons with in situ data measurements are required to assess the accuracy of the model results. Temperature and salinity data for the Black Sea over the period 1971-2002 are available from the Med Atlas and other databases.

Minor issues: Page 2070, Fig 11. Harmonise the spelling (UK/US) :'Modelling' in figure 11 but 'modeling' in the caption and further in the text.

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