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Biogeosciences Discussions

Interactive comment on "Modeling the nitrogen fluxes in the Black Sea using a 3D coupled hydrodynamical-biogeochemical model: transport versus biogeochemical processes, exchanges across the shelf break and comparison of the shelf and deep sea ecodynamics" *by* M. Grégoire and J. M. Beckers

Anonymous Referee #3

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General Comments

The Black Sea studies during the last decades indicated a strong degradation of the basin environment caused by polution of terrestrial origin. The observational programs during the 1980s and the 1990s collected a large amount of data about the biochemical variability at different parts of the sea, which allowed to diagnose the state of the ecosystem and its evolution. However due to the limited temporal and spatial resolution of the available data, some important scientific and practical aspects of the ecosystem variability remain open. In particular there is not still a reasonable quantitative esti-

mation of the intensity of the processes of nutrient transport and their transformation by the biochemical processes inside the basin which determines in a big extent the character of the reaction of the ecosystem to the fluxes of terrestrial origin and their variability.

The paper addresses important problems related to the quantification of the nitrogen fluxes in the Black Sea. These fluxes are strongly 3-D, which on one side have a strong across-shelf component and on the other they interact in a complex way with the local biochemical and sediment processes. The existing data and simplified (1-D or box) models have strong limitation in the study of these problems. Thus the authors apply a novel approach, based on the use of a 3-D coupled hydro-dynamical and ecosystem models. This approach was invented during the recent years in the ocean biochemical studies and its application to any ocean basin is related to specific methodological and numerical problems.

I think the paper has at least two major contributions to the studies of the Black Sea ecosystem. Firstly it describes clearly and properly the solution of numerous methodological problems related to the coupling of the GHER ecosystem and hydro-dynamical models for the specific case of the Black Sea. Secondly the paper presents results from model estimation of the nitrogen fluxes in the Black Sea and critical assessment of the obtained values. The errors in the estimations, which the authors determine by comparison of the simulations with the available data, are discussed and shortcomings in the model design and parameterizations, which may be the source of these errors, are indicated.

The major comment I have about the methodology used by the authors is related to the forcing used in the hydro-dynamical simulations. As discussed also by the authors the monthly mean winds and surface restoring towards climatological values of T and S may result in relatively weak vertical mixing and shallow mixed layer. I would recommend the work presented in the paper to be extended in future studies by using high frequency surface forcing.

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I think that the paper is a complete study, which is a major contribution to the scientific efforts in developing coupled hydro-dynamical and ecosystem models for estimations of the state of the Black Sea ecosystem.

Specific Comments

I have several specific questions, recommendations and comments mainly on the methodological part of the paper, i.e. on the way of how the two models are coupled and forced:

1) Page 111, second paragraph: *Ĕ with the aim of understanding the macroscale (i.e. time scale of a few weeks to months) Black Sea ecohydrodynamics and more specifically: (1) estimation of the transport*. For the transport variability an important contribution at these time scales have the meso-scale variability. How the authors distinguish between the meso- and macro- scales?

2) Page 112, first paragraph: *Ĕ general marine weather model by averaging over a time scale of several weeks.* The GHER model is an explicit free surface model, i.e. it resolves time scales of the fast surface gravity waves, which for the deep part of the sea with resolution of 15 km should be about 2 minutes. How the model equations are then averaged over a time scale of several weeks?

3) Page 115, line 21: *Using the results of the tenth year of integration of the physical model, Ĕ* It is not clear from the text with what frequency were stored the hydrodynamical data used as input for the ecosystem model (hourly, daily, weekly etc.).

4) Paragraph 2.4. Even though not mentioned explicitly, from the text it comes out that the coupling between the hydrodynamic and ecosystem models is one Ű way, i.e. the output from the hydrodynamical model is used as input for the ecosystem model. Some observations (for instance Aubrey at al., J.Mar.Syst., 1997) suggest that the feedback between the phytoplankton concentration and vertical mixing and the mixed layer depth may play an important role and especially in the area of the Northwest shelf.

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The observations show in particular that the high productivity in this area results in decreasing of the attenuation length for the solar radiation and thus strongly influences the stability of the water column.

5) Page 122, lines after the line 9: The application of vertical K-l mixing schemes in a model forced by annual mean surface momentum fluxes and relaxation of surface temperature and salinity to the climatology may (as discussed by the authors) not give resonable results for the surface mixed layer depth. It is not mentioned in the text however whether a daily variability of the short wave solar radiation is also included in the forcing. If not that may be additional source for errors in the model mixed layer depth.

Technical corrections:

The language in the paper is fluent and precise. I would recommend only the following minor changes in the text:

1) Page 114, line 25: *Ě scheme is completely instable Ě* I would use unstable

2) Page 115, line 28: The right bracket is not put at correct place.

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