Biogeosciences Discussions, 1, S154–S165, 2004
www.biogeosciences.net/bgd/1/S154/

Biogeosciences Discussions

BGD
1, S154–S165, 2004

Interactive Comment

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

M. Grégoire and J. M. Beckers

Received and published: 14 September 2004

Dear reviewer, Here are my answers to your remarks. In bold, I have repeated your remarks. We would like to insist that we know that the biogeochemical model is maybe a crude oversimplification of the real Black Sea ecosystem which includes the development of gelatinous groups, 87 % of anoxic waters with complex chemical processes and a rich diversity of bacteria at the interface between the oxygenated and anoxic part, the development of coccolithophorids, the possibility of anoxic conditions in the shelf bottom at some periods of the yearĚ. The physical model simulates the general and synoptic circulation. We agree that the exchanges at the shelf break may

Full Screen / Esc
Print Version
Interactive Discussion
Discussion Paper

be strongly influenced by mesoscale processes which are filtered out in our study. However, 3D mesoscale coupled ecosystem-hydrodynamical models are often used to simulate short term events and simulations are rarely performed on several years.

The coupled model described in this paper was used as a tool to quantify the effects of the 3D physics on the biology and notably, to quantify the vertical transport of nutrients by advection/diffusion, lateral advection between the coast and offshore, the influence of the rivers and notably, the spreading of the Danube's outflow (with notably its reversal at the end of spring which is extremely important for the biology), upwellings/downwellingsĚ We may also argue that the nitrogen budget of the model is almost verified (as in complex models) and the different types of errors on the numerical discretisation and on the computation of exchange fluxes are totally acceptable considering the unavoidable errors on the data used to force the model. We hope to answer most of your questions. Thank you very much for your opinion on our paper.

The most important point is the cyclic instationarity of the biogeochemical model. In Fig 3 the nitrogen input onto the system is about 40 % higher than the output. Due to the corresponding successive enrichment of the system the model simulates unrealistic fluxes which are interpreted as natural phenomena

The total nitrogen input (Danube+Bosphorus) = 797 000 tons/year. The output of nitrogen (denitrification +Bosphorus outflow) = 450 000 +150 000 = 600 000 tons N/year. Thus, a difference of 197 000 tons. You have also to consider that 40 000 tons of PON leave the upper layer ecosystem an reach the anoxic waters and are definitely lost for the upper layer ecosystem. Indeed, this PON will be remineralized into NH4 and this Nh4 will diffuse upwards into the transitional layer and will be oxidized by nitrate and oxygen into nitrogen gas. This last process is not represented as mentioned in the model description. Indeed, since the ammonium pool in the anoxic layer does not have any impact on the upper layer ecosystem and since we are interested in the upper layer, we did not represent the deep ammonium pool. That can also partly explained why denitrification may have been underestimated in our model, because we do not consider this production of Ngas due to the oxidation of the ammonium diffusing from the anoxic part. Also, the difference between the inputs and outputs of nitrogen on an annual scale equals roughly 150, 000 tons of nitrogen. This represents about 2% of the total nitrate content of the whole basin. Also, the nitrate content increases "unrealistically" by only 2%. Nevertheless, it is far from being sure that the nitrate content of the Black Sea is stationary. Indeed, field data have suggested an increase of the nitrate concentration from about 4 mmolN/m3 before eutrophication to 12 mmolN/m3 in the eutrophication period. We start our simulations with initial conditions of 8 mmolN/m3 at the maximum. If you look at our paper (Gregoire et al, 2004, JGR), you will see that the simulated nitrate profile has a structure and values totally typical of the Black Sea. So, although the nitrate is not stationary in our model (the other variables are roughly stationary), its profile remains totally in agreement with observations. That is why we are quiet confident in model outputs and we use it for diagnostic purposes. Concerning the other sources of errors than the structure and parameterization of the model, we have estimated the error associated to the non-perfect conservativity of the numerical scheme used to integrate the biological equations and to the interpolation in time and space of fluxes presenting a high space-time variability. The order of magnitude of these other sources of errors were found totally acceptable regarding the margins of errors you have on the data used to force the 3D model at open sea boundaries (surface, rivers, bosphorus)

It is essential to quantify the ratio of organic vs inorganic material exported from the shelf into the open sea. Because of this relevance the authors should discuss this point more intensively in their manuscript.

This point is discussed in details in another paper : Exchange Processes and Nitrogen Cycling on the shelf and continental slope of the Black Sea basin. Grégoire M. and Lacroix G., 2003. Global Biogeochemical Cycles, 17(2), 42-I - 42-17. In this paper, the ratio of organic vs inorganic material exported from the shelf into the open sea is quantified as well as the export of refractory organic matter discharged by the Danube.

BGD

1, S154–S165, 2004

Interactive Comment

We also investigate the possibility of existence of a depocenter on the continental slope adjoining the shelf.

Detailed comments:

Page 5 Have you tried smaller values for the horizontal diffusion coefficient? Does this change the overall picture?

The horizontal subgrid scale viscosity for momentum is 500 m2/s and the horizontal subgrid scale diffusion for tracers is 50 m2/s. These coefficients have been computed according to the size of the horizontal mesh and have not been changed.

The relaxation to temperature and salinity in the upper layer should be mentioned in this chapter

This is a boundary conditions. This relaxation is only applied at the sea surface (first box) and not in the whole upper layer. Observed sea surface temperature and salinity were used to prescribe Haney-Type surface relaxation fluxes with relatively weak relaxation coefficients (corresponding to adaptation of a mixed layer depth of 10m in 10 days).

The authors argue Ě.Could you comment on this?

In Beckers et al, 2002 Special issue of EROS 21, we compared the water fluxes from the coast to the deep sea computed by the GHER hydrodynamical model with a horizontal mesh of 15km (as in the present study) and 5km. It has been shown that the variability of the export is higher with the high resolution model and the net export was also higher. However, the coupling of a high resolution model with a biogeochemical model for several years needs very long times of simulations. In addition, you need high frequency atmospheric forcings for the whole basin with a high spatial resolution. This type of forcing is unavailable at this time. The horizontal resolution of ECMWF data is 2.5 degrees.

Page 6, What is the reason for omitting O2 ? Ě.

BGD

1, S154–S165, 2004

Interactive Comment

In the paper Gregoire et al , 2001, we have explicitly modeled oxygen because we want to study the ventilation of the deep waters. It has been found that the explicit modeling of oxygen does not modify substantially the simulation of the biological productivity. In fact, in the Black Sea, the vertical profile of oxygen as a function of density does not show variability in time and horizontally. Also, the oxygen concentration at a given density can be diagnostically computed as a function of the density. It looks like a constant value at the surface, then a linear decrease in the oxycline which is located between more or less constant density levels and then very low concentrations in the suboxic layer and of course zero in the anoxic layer which starts at about sigma =16.2 whatever is the location and the time of the year (see also, the paper of Oguz et al, 1998 NATO ASI series). These characteristics of the oxygen profile make possible to use it diagnostically in the Black Sea biochemical models if oxygen is not a primary concern as a prognostic variable. In a 3D model, since the density field is well represented, we can use it to have the value of the oxygen concentration.

Could it be demonstrated that nitrogen is the most limiting nutrient in the Black Sea?

In the Black Sea central basin, nitrogen is the most limiting nutrient of the phytoplankton growth due to the intense transformation of nitrate into nitrogen gas by heterotrophic denitrification occurring in the transitional layer between oxic and anoxic waters. Low nitrate to phosphate ratio in the layer below the euphotic zone of the deep sea implies nitrogen as the most limiting nutrient (e.g Sorokin, 1983; Tugrul et al, 1992, Nature). This fact seems to be a well accepted feature and all the biogeochemical models applied in the central Black Sea are nitrogen cycles. On the shelf, the EROS21 expeditions organized on the shelf in summer 1995 and spring 1997 revealed that phosphorus may be also limiting at the end of spring but silicate was never limiting due to an intense recycling of silicate both in the water column and at the sediment-water interface (see the special issue of Estuaries, Coastal and shelf science of March 2002).

Neglecting the microbial loop with the remark that it works particularly efficient in the Black Sea is misleading as variations of nutrient availability may result in nonlinear compensating effect.

The present form of the model may be regarded as a first step to understand the first order biological processes, to assess the effect of the 3D hydrodynamics on the ecodynamics and to understand the basic nutrient and material flows controlling the overall productivity in the Black Sea. The bacterial loop has been short-circuited since the time scales associated to bacterial growth are usually shorter than the other modeled time scales. It is of course a simplification but this could be more or les justified. Moreover, this short-circuiting is often done in 3D models and gives very good results. In the Black Sea, this simplification can be justified by the high efficiency of bacteria.

Page 7 The use of a sigma-coordinate system cannot be justified by avoiding the rigid lid approach. Also z-coordinate models work with a free surface

I agree, it has been modified.

Gregoire(1998a) should be omitted for citation

I have replaced this reference by the paper Gregoire et al, 2004 published now in the Journal of Geophysical Research.

Page 8 : The implicit integration approach causes mass defects. It can not be avoided because explicit systems are not stable. This is not true, you only need an automatic time-step adaptation.

Automatic-step adaptation does not solve the stability problem. Here it is not the stiffness related problem which is referred to (which can be controlled by adaptive steps), but the unstable character of an explicit Euler discretisation when applied to oscillating systems. In addition in practice using time-step adaptations would lead to unmanageable small time steps in the scope of a 3D model.

Page 9 It would be interesting to see local maxima of the artificial creation/destruction of matter due to the non perfect numerically conservativity.

The paper already contains 16 figures. This distribution is not primordial to the understanding of the paper.

Page 10 The input of nitrogen lies not only in the lower range of river input but also atmospheric N-deposition and other rivers entering the Black Sea are neglected. The artificial accumulation of nitrogen would be even larger when these additional sources would be switched on.

The Danube river alone is the greatest contributor, accounting for about 50% of the total river runoff. The total discharge of the Dnestr and Dnepr rivers are about three times smaller than the Danube, and the total discharge of the remaining rivers account for a fraction ($<$1/5) of the total river runoff (Ozsoy and Unluata, 1997). These remaing rivers are mainly concentrated along the Turkish coast where nutrients may be limiting. The accumulation of the nitrate discharged by the Danube of the shelf is due to the fact that the modeled primary producers, represented by only one box as the zooplankton, are not able to consume this large amount of nitrate discharged on the shelf due to the strong grazing control. This phytoplankton grows under saturation conditions in nutrient. If we considers the input of nitrogen by the Turkish rivers (which are however not available), I suspect that these inputs will promote new blooms because these regions are not oversaturated in nitrogen. Also, the nitrogen will be consumed and will not accumulate. Moreover, as concerns the atmospheric input of nitrogen, these inputs represent less than 13 % of the Danube input (see Appendix A2 of the paper). This N2 input can be consumed by a particular group of phytoplankton which is cyanobacteria. This group is not modeled.

Page 11 The model does not represent denitrification processes on the shelf even though the authors argue that this process is relevant. Why is this process not integrated in the model or at least : why is not any parametrisation used ?

From the analysis of the EROS-21 observations performed on the shelf in summer 95 and spring 97, it has been found that benthic denitrification may be a significant

BGD

1, S154–S165, 2004

Interactive Comment

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

process of nitrogen elimination on the shelf (see Gregoire and Friedrich, 2004). However, these estimations of benthic denitrification are punctual measurements realized at 5 stations during a few days in summer 95 (when temperatures are high and may increase denitrification) and in spring 97. It is really difficult to extrapolate these measurements over the whole shelf and year in order to have denitrification rates to be included in the model. In winter, denitrification is maybe not important.

Page 13 Both model results and observations Ě a reference to a corresponding figure would be helpful. Fig 6 I can not see the model results.

The results of the hydrodynamics model and the ecosystem model are described in extensor in Gregoire et al, 2004, Journal of Geophysical Research. In this paper, the seasonal evolution of the chlorophyll concentration computed by the model and obtained from SeaWiFS and in situ observations are described. In this paper, we only present diagnostic computations and we did not want to increase the number of figures (which is already high). That is why we did not show maps of simulated phytoplankton concentration. Please refer to the above-mentioned paper.

I do not understand why you could not see model results in Figure 6. I have checked on the web. The continuous line show the model predicted primary production. In bold, mean values for the central basin and in normal line, values for the eastern main gyre.

Page 16 A figure of the horizontal annual primary production would be helpful. It has been added as Figure 6. All the figures have been renumbered accordingly

Page 18 The error concerning the water budget for the shelf : When the artificial water would be added to the shelf water it would lead to an annual sea level rise of 0.28 m. IS this correct?

The water budget of the shelf is closed as well as the budget of the whole basin (the transport at the bosphorus strait is adjusted for this, see Appendix 2 about the boundary conditions). Hopefully, the sea surface elevation does not increase of 28 cm per year (maximum sea surface elevations in the Black are of about 16cm at the Danube's mouth in spring when the runoff is maximum)What we mean in the paper is that the error on the model computed water export is 14 km2. This error on the export comes from the fact that model results are interpolated along the section. If the export was exactly computed it should be equal to 241 +14 = 255 km3. With this estimation, we can have an idea of how reliable is the computed export. It has a relative error of about 6% which we found totally acceptable compared to the other sources of errors. So, the model computed export and its vertical structure and temporal variability can be considered as acceptable.

Page 19 The structure of the shelf open sea transport seems to be export into the open sea in the upper layer and import into the shelf in the lower layer. Could this interesting feature supported by observational data ?

To our knowledge, there has been no measurements of the exchanges between the shelf and offshore and on its vertical distribution. Only satellite pictures clearly illustrate the presence of filaments of shelf waters ejected at the shelf break. These filaments are particularly visible at the end of summer. Besides, it is well known that the shelf circulation is anticyclonic from the end of spring until the end of fall, and this is at the end of summer that the nutrients discharged by the Danube reach the shelf break and penetrate in the western part of the central basin.

Page 20 Fig.12 The total export of 795,786 should be indicated by a dotted arrow, right?

You are totally right !

Figure 12 : the difference of N inventory does not fit with import/export; Have I used the wrong numbers?

If the model was fully stationary, the nitrogen budget of the shelf would be equal to zero. It means that river nitrogen inputs = nitrogen net export at the shelf break (since

BGD

1, S154–S165, 2004

Interactive Comment

we do not model denitrification in the sediments). However, as mentioned in the paper the nitrogen budget is not fully stationary.

The annual nitrogen budget of the north-western shelf implies an approximate balance between the variation of the nitrogen inventory of the shelf, the external input of total nitrogen by the rivers and the nitrogen transported at the shelf break. However, since it exists an error on the computed nitrogen export as well as an error due to the non-perfect conservativity of the numerical model (due to the use of an implicit scheme of integration for the biogeochemical interaction terms), this balance is not totally verified and one estimates the relative error on the diagnostic computation of the nitrogen export towards the open sea to about 5.6 % which is in agreement with the estimated relative error on the water export (6 %).

Page 21 The authors give a reference for observational data concerning PON fluxes into the sediment and benthic remineralization. These data should be discussed at this point.

We were very brief concerning this comparison since the nitrogen budget of the shelf inferred from modeling results and EROS21 observations is extensively described in Gregoire and Friedrich, 2004. However, we have added a summary of the results in Page 21 as required by the reviewer.

Page 23 : A short comment on arguments for neglecting the microbial loop would be helpful at this place

It has been added in the model description section.

Page 26. The export by sinking critically depends on the sinking velocity and the degradation rate. It would be very helpful to give arguments for the values chosen. A short study with different values and resulting export rates would give an impression of these dependencies.

The sinking velocity and degradation rate were chosen so that there ratio lead about

BGD

1, S154–S165, 2004

Interactive Comment

90 % of the organic matter rematerialized in the upper layer as observed in the Black Sea. Results provided with the calibrated values are totally in agreement to observations. Since we had a lot of observations to validate our fluxes, we did not make some sensitivity analysis using different values of the sinking velocity and of the degradation rates since one year of simulations takes about 4 days of computation. So to reach the quasi-equilibrium solution, it takes more than 10 days!

Page 29 One of the central statements of the manuscript is that river input of nitrogen more or less equals the nitrogen export across the shelf break. What does happen with this statement when a big part of nitrogen is converted into free N2 on the shelf?

At this time, there are a lot of uncertainties concerning the annual cycle of benthic remineralization. The best solution which is maybe unfeasible due to the difference of time scales to reach steady state and also due to the difference of length scale on the vertical, would be to fully couple a diagenetic model to the pelagic biogeochemical model n order to study benthic remineralization pathways and notably the importance of benthic denitrification and its seasonal evolution. Once this seasonal cycle is known, it would be possible to quantify benthic denitrification as a loss of nitrogen for the shelf.

Page 30 The discussion of free N2 production in comparison with other sea areas should be strictly done with fluxes per square meter.

In this discussion, we would like to estimate the contribution of the Black Sea in the global production of N2. For this, we have to integrate the denitrification rate over the total Black Sea volume. We do not want to compare the intensity of the denitrification rate between regions.

Page 56 what is g?

The density. has been replaced by for avoiding confusion with the sigma coordinates.

Since, the vertical profiles of chemical species (e.g. Nitrate, ammonium, oxygen, hydrogen sulfide) is independent of the season and geographical location when expressed

BGD

1, S154–S165, 2004

Interactive Comment

as a function of the density, all the chemical processes (denitrification, nitrification, redox reactions) are also computed as a function of the density.

Page 57 maximum detrital sing(k)ing

It has been changed

Some general remarks: The big rivers surely deliver suspended matter which is flushed into the shelf water. Is this effect (especially the light limiting co-effect) included in the model ?

Yes, it has been taken into account. The presence of POM contributes to the vertical extinction of the light. However, we did not find any value in the literature concerning the extinction of the light by particulate matter other than chlorophyll. Also, we took an extinction coefficient exactly the same as for the chlorophyll. It has been clarified in the Tables.

The comparison with observational data is pure. For Fig. 6 the positions of observations should be indicated. These are observations performed in the central basin during different years. The position of the stations varies from one year to another. This is a composite picture.

Interactive comment on Biogeosciences Discussions, 1, 107, 2004.

BGD

1, S154–S165, 2004

Interactive Comment