Biogeosciences Discussions

Interactive comment on "Link or sink: a modelling interpretation of the open Baltic biogeochemistry" *by* M. Vichi et al.

M. Vichi et al.

Received and published: 29 August 2004

AC to ICs by Anonymous Referee #2

We are grateful to Referee #2 for his/her positive comments on the manuscript. We also are convinced that ecosystem models should be used as tools to investigate and verify conceptual hypotheses on systems' functioning and not as just "numerical exercises".

We are therefore happy that the Referee sensed this attitude in our work. These open discussions are really useful to involve scientists from different fields, and can help authors in making their manuscripts more accessible to a wider audience of both experimentalists and modellers.

The model sources and simulation setup, as well as all the necessary software for running and analysing the models, are available to any scientist interested in reproducing our experiments and comparing them with their own findings.

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

© EGU 2004

Detailed answers

Pages 226-227: We are aware of POM limitations in describing momentum transfer at the boundaries. Particularly, it is well known that mixing rates in shallow waters are overestimated and this was a major reason for applying a restoration of the water column vertical structure. Without this parameterisation it was not possible to maintain the observed permanent stratification at all. At the same time, it has been shown that POM produces weak cross-boundary fluxes and therefore we added the modification of the constant background diffusivity. The parameterisation proposed by Stigebrandt (1987) and Omstedt & Axell (1998) have substantially increased the diapycnal exchange of nutrients during the stratified period, but apparently this phenomenon is not sufficient to explain the observed dynamics of nutrients and plankton in the BIW. Enhanced diffusion through the halocline was initially thought to be the main mechanism controlling phytoplankton growth in the BIW (Vichi, 2002), but now we tend to believe that the interactions between the remineralisation and the sinking rates of organic matter are key processes.

Our experience with this model indicates that the effects of these parameterisations are instead larger in the bottom layers, as also shown in the manuscript with the sensitivity experiments on the relaxation time scales (RTS) for salinity restoration (cf. Figs. 4,7,8, panel c and Fig. 5b).

We agree that profiles do give a one-to-one validation of model results, but as they lack the possibility to illustrate the time-evolution, we have omitted them from the manuscript. Moreover, in view of the long investigated period, we have the problem of choosing representative profiles. We provide here for completeness both a summer and a winter set of profiles compared with corresponding model results which can be included in the final revision.

We also confirm the binning depths used in Figs. 4-8 as requested by the Referee on page S80. Interfaces between homogeneous layers in the model are located at 0.0 -0.4 -0.7 -1.4 -2.9 -5.7 -11.4 -17.1 -22.9 -28.6 -34.3 -40.0 -45.7 -

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

51.4 -57.1 -62.9 -68.6 -74.3 -77.1 -78.6 -80.0 m and the model levels are located in the middle of each layer. Standard levels in the BED database are located at -1.25 -5.00 -10.00 -15.00 -20.75 -29.50 -40.00 -50.00 -60.00 -70.00 -80.00. Data nominally located at -40m have been compared with model results in the layer -40 and -45.7m.

Page 227, l20-23: The main reason for not including P deposition was related to the difficulty of relating it to precipitation in order to appropriately estimate the deposition flux. An option might have been to use the climatological mean annual precipitation, but this value is not representative for the large variability observed in the Baltic. We could not find the original data used in HELCOM (1987) to compute the reported annual load, and therefore decided to ignore this forcing.
As suggested by Referee #2, we performed a simple simulation with the addition of a background daily atmospheric deposition of PO_4 derived from

$$F_P = \frac{L_P}{365 \cdot A}$$

where L_P is the mean reported annual load (6500t) and A is the Baltic Proper area (211,070 km^2). The resulting flux is about 2.7 10^{-6} mmol P m^{-2} d^{-1} which is more than three orders of magnitude lower than the mean freshwater load (4.2 10^{-3} mmol P m^{-2} d^{-1}). This additional flux has just small effects on the simulation results, but it might easily be that a better time-resolved timeseries could lead to more substantial changes.

Page 233, l14-: The main point of applying Geider's formulation is that it reproduces the reported phytoplankton behavior of not maximising production in the light-saturating phase. The parameterisation in ERSEM (Ebenhoeh et al., 1997) leads to a linear regulation of phytoplankton production towards the maximum productivity at any depth with an irradiance larger than the minimum value prescribed in the model (25W m^{-2}). It is expected that a more refined parameterisation would

lead to a better resolution of phytoplankton vertical distribution. Some prelim-
inary simulations with Geider's formulation have shown that summer dynamics
are modified as a consequence of the spring bloom. Particularly, the location of
the subsurface Chl maximum is moved more into the BIW, which could also con-
tribute to better match the nutrient observations at that depth.

Some measured profiles of carbon-specific production rates during summer
would further help to understand the actual phytoplankton activity during this pe-
riod. Unfortunately, Nommann and Kaasik (1992) data are limited to the spring
bloom period, and carbon data presented in HELCOM do confirm the fact that
the model underpredicts summer phytoplankton stocks.

Indeed, nutrient limitation and not a simplified Chl-dynamics is probably the main
cause for this discrepancy in the model, and this is why we tested the new param-
eterisations discussed in Sec. 5. In this context, we recognise that the statement
is not completely clear and will be amended in the final revision.

Page 233, l27-: This question is linked to the above considerations. Modelled phyto-
plankton is N-limited in the BSW, in spite of the excess of N-compounds in the
BIW. Under these conditions, slow-growing, inedible phytoplankton are favoured.
We are aware that there are no indications of the presence of such a group in
the Baltic Proper, but $P^{(4)}$ characteristics are similar to the one of filamentous
diazotrophs as, for instance, *Nodularia sp*.

Results from the sensitivity experiment of removing $P^{(4)}$ are shown in http://
www.bo.ingv.it/~vichi/BGD/P4exp.png as "calendar" seasonal means.
Run B1 is compared with the same simulation setup but without $P^{(4)}$ dynamics
(B1-P4). Diatom autumn blooms slightly increase in the first years but afterwards
both simulations tend to converge. It is interesting to notice that autotrophic
nanoflagellates are taking the role of slow-growing phytoplankton, with a sub-
stantial increase during summer. Picophytoplankton is instead not affected by the
absence of $P^{(4)}$. The autumn bloom of diatoms is still not so high as reported,

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper

© EGU 2004

but it is also not visible in Chl data. Chl measurements hint at the presence of a broad maximum from spring to autumn, which is likely to comprise a succession of different species. Such a broad maximum could be only maintained in the presence of additional sources of N.

N-fixation by cyanobacteria was not included in the model from the beginning despite their attributed role in Baltic nutrient dynamics. This was done on purpose to clarify the role of the other phytoplankton groups and of the microbial loop remineralisation in general. Model results now suggest that summer phytoplankton biomass in the BSW cannot be maintained without an additional source of N which might be related to N-fixation. We welcome any collaboration with scientists that might be interested in testing their N-fixation parameterisation in this model set-up.

Page 236, l22-: Frankly, we cannot yet state that the inclusion of polysaccharide dynamics is a complete success. It did improve the simulation of after-bloom conditions, partially indicating that there is still quite some work to do in the parameterisation of excretion from phytoplankton. The provided reference (Engel et al., 2004) is therefore very useful, because it suggests additional mechanisms that might be important for the understanding of late spring-summer dynamics in the BIW. Some comments will be added in the final revision.

Page_238, l7: We agree that the number of data is not sufficient to support a 30d filtering, but this winter minimum is also found in other years of the time series when there are more observations available http://www.bo.ingv.it/~vichi/BGD/amm-B1.png. Indeed, ammonium is a difficult parameter to measure because of its large spatial and temporal variability, available data are very scattered and the model appears to not exhibit good hindcasting skills. Our statement was mainly supported by common knowledge of system behaviour and not by this particular year of the timeseries. Ammonium concentrations in the BIW and in the bottom layers are relatively high during wintertime, therefore we found

it quite puzzling that the data do not show a replenishment of ammonium as they do for the other nutrients.

Page 245, l12-: Due to the complexity of the model, there are quite a lot of model results that have not been shown, because the manuscript would have become too long and difficult to read. Unfortunately, most of the results of the benthic models cannot be verified because of the lack of site-specific observations. Our statement was just derived from model result considerations, because the sediment-water nutrient remineralisation rates have been found to substantially affect the concentration of nutrients in the BBW and hence the amount of nutrients that can be supplied to the euphotic zone.

This is why we concluded that it is important to verify the model with measurements of benthic fluxes, which are, to our knowledge, extremely scarce in the Bornholm basin. We are also planning to apply the model to the Gotland Basin, where more observations of benthic-pelagic exchanges are available.

Small points: all the suggested changes will be applied in the final revised form. The word "global" refers to the Baltic Sea basin average; it will be clarified in the revised version.

Figures: see answer for question on pages 226-227 above for the binning of data and model depths.

Figure axes will be stretched in the final revision as requested.

───────────────────

Interactive comment on Biogeosciences Discussions, 1, 219, 2004.

Full Screen / Esc

Print Version

Interactive Discussion

Discussion Paper