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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.

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Answers to Dr. Thomas' IC

We thank Dr. Thomas for his interesting comments and for his overall positive judgment on the manuscript. His recent publications have inspired some parts of the work presented in this paper.
We are also grateful for the careful check of spelling and typos. They will be amended in the final revision.

Detailed answers

Page 221, l15: We agree that the sentence is unclear. The exact phrase should have been "response to physical and biological perturbations".

Page 226, l13-15: Dobson and Smith have proposed an equation to compute astronom-

ical irradiance at the top of the atmosphere as a function of latitude and time of the year. Therefore, it is independent from the specific year, and annual variability is included by correcting for cloudiness, a parameter that was available in the BASYS dataset.

Page 235, l25-29: The sentence will be divided in two parts in the final revision. *The spring bloom and the nutrient draw-down are satisfactorily modelled, but this model (as most of the deterministic ecosystem models) has substantial approximations in parameterising the subtle processes that link organic matter to the local biogeochemical transformations. In fact, nutrients can be locally recycled from organic matter, making them readily available for local production, or particulate organic matter can sink to the benthic pools, thus introducing a further time lag in the re-supply of mineralisation products.*

Page 236: We thank Dr. Thomas and will follow his suggestion in the final revision. This section will be split in two sections: 5. Discussion and 6. Process oriented experiments, the latter with 2 sub-sections (formerly 5.1 and 5.2). The other sections will be re-numbered accordingly.

Page 238, l7: This point has also been addressed by Referee #2 (see answers in Author's Comments http://www.cosis.net/members/journals/df/article.php?a_id=999). Extensive model comparisons with ammonia data were not shown because of the large scattering in the observations, which is indicative of the transient nature of this compound as correctly pointed out by Dr. Thomas. We just shown ammonium in the surface layer in Fig. 12b because it was already showing some of the major issues. However, the model also overestimates ammonia in the bottom layer (http://www.bo.ingv.it/~vichi/BGD/amm-B1.png) and this is the explanation why it produces a winter replenishment similar to the other nutrients. This comment will be included in the revised version. Winter maxima are, however, visible sometimes in the dataset.

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Interactive Comment

We are aware that the coupling between ammonification, denitrification and nitrification is much more complicated than the model parameterisations, but it is also likely that the production of a more properly-qualified organic matter would contribute to improve model results (see answer below).

Page 238, l18: It is likely that one of the key to complete the picture of N-dynamics in the BIW is the transition of dissolved organic carbon into particulate without the intervention of the entire foodweb, which, as the model predicts, ultimately leads to nitrogen-enrichment of the substrate. This transformation from DOC to POC could lead to the production and sinking of N-poor detritus that would probably solve some of the discrepancies with nitrate and ammonium in the bottom layers as well. This was also suggested by Referee #2, and Dr. Thomas' comments are really helpful in better clarifying this picture. In this work we tried to simulate this flux by the introduction of the fast-sinking components of detritus, but now it's much more clear that this process is important, but does not introduce any change in the quality of the organic matter. We will include some of these considerations in the final revision of the paper, suggesting future lines of improvement in the model parameterisations.
Nevertheless, it is also important to clarify the role of bacteria in the BIW, because the model suggests that yet they are active remineralisers, while there are not many measurements to support or reject this model prediction. Measures of bacterial activity at 40-50m would help to quantify the extent of their contribution.
Concerning the issue of an earlier bloom, the model is already simulating a time-shift in the spring depletion of nutrients (p. 237 and Fig. 12a), which is an indication of an earlier bloom development with respect to observations. Making the bloom even earlier would lead to a faster removal of POM from the surface layers that would be segregated in the layers below the BIW during summer. On the contrary, we expect that a delayed phytoplankton bloom, or rather the occurrence of a series of small blooms instead of a single peak, might contribute to

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the maintenance of sufficiently high N levels in the surface layer (in the form of DON or ammonium). This is why we suggested the inclusion of a better parameterisation of light acclimation (see also Authors' Comments to Referee #2 http://www.cosis.net/members/journals/df/article.php?a_id=999).

Page 240-241 and Tab.1: We thank Dr. Thomas for his comments and we will put any effort to ameliorate and extend this section in order to make it clearer. The details on the different forms of inputs were given in Sec. 3.2, but we understand that they need to be reported here as well for a proper comprehension of the scenarios outcomes. Case S2 was discussed (p. 241, l13-16; p. 242, l10-13), but on page 241 there is no reference to the run name. This will be amended in the final revision.

Indeed, there are several important implications of the scenarios that could not be explained thoroughly in a paper that is also presenting the validation phase of the model. Most of the discussions related to the results of Sec. 6 are given in the final Sec. 7. We will make the final message more evident in Sec. 6 as well.

Finally, it is not completely clear what is intended with the sentence "how eutrophication might affect ecosystems". Usually, the term eutrophication refers to the effects on marine ecosystems of a generalised increase in nutrient inputs. We have addressed the importance of the different kind of inputs, and not specifically of eutrophication itself. Maybe, in this case, it would be more appropriate to cook up new terms like *autotrophication* and *heterotrophication*...

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