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Biogeosciences Discussions

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Interactive Comment

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

Anonymous Referee #2

Received and published: 24 August 2004

This is a generally well written paper, making an appropriate use of modelling techniques to characterise and explore the process dynamics of the region in question. The quantification of processes seem to confirm and elucidate the generally accepted understanding of regional dynamics. The limitations of the techniques are also sensibly discussed. Aside from a number of minor discussion and editorial points, listed below, I would welcome this manuscripts rapid appearance in the literature.

The modelling methodology is sound and proven. The amendments to the model system are justified and well explained. I do miss a discussion of how known inadequacies in the 1D physical models vertical mixing could have contributed to the data-model mismatch in the intermediate layer and how the adaptations to the physical model described on pages 226-227 may have helped. The POM model is known for not doing a fantastic job in characterising boundary layers and cross boundary fluxes and this may be a significant source of error. On a slightly philosophical basis, presenting the model results exclusively at surface, 40m and bottom does not prove how well the model

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realises the three layer structure, although for validation purposes it is quite proper. Presentation of some vertical T, S, N and mixing parameter profiles with observations might provide insight and would add weight to the claim that the model does well resolve the observed three layer system.

Page 227, lines 20-23: A minor point, I do not follow the logic of omitting atmospheric phosphate inputs because no spatial temporal information is available. If we know that P is input via the atmosphere it would seem more correct to model this flux with accepted inaccuracy rather than omit it which is essentially more inaccurate. I do however accept that omitting this flux may not have a significant influence on the model results given that the model system appears N limited, but it would be helpful if the authors could back this up with a (simple) sensitivity study. It is hard to tell from the figures but the observations indicate that the surface system may also be P limited in the summer.

Page 233, line 14 on: The discrepancy probably could not be explained by the linear derivation of Chl from phytoplankton carbon in the model because it is unlikely to be improved by including the more realistic Geider model. Simplistically with the Geider model the lower Chl:C ratios in the summer will make the model predict less Chl than the linear Chl:C model, exacerbating the under simulation of Chl. This is not to say that the Geider model should not be included in ERSEM, it already has been in other versions to good effect. I suggest that the authors need to explore other possibilities to explain the under simulation of Chl. Do any phytoplankton biomass data exist that could shed light on this issue (HELCOM, Nommann & Kaasik)?

Page 233, line 27 on: Is there any evidence for inedible phytoplankton in the region to back up the model findings. The authors allude to a sensitivity analysis omitting P4 from the model, but this could be more clearly stated. If P4 are excluded does the observed September diatom bloom occur in the model? .. I see this is discussed on page 244, given the modelled discrepancies of surface N, the hypothesis that cyanobacteria are important mediators of nutrient dynamics is an interesting conclusion and would

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suggest a subsequent phase of model development.

Page 236, line 22 on: The recently published Engel et al Nature 428, p929-932 might contain discussion material relevant to the polysaccharide and export issues. It is informative to see the reporting of ŞunsuccessfulŤ model adaptations rather than just the success stories.

Page 237, line 7: Based on eyeballing the figure I suspect that the observations are too sparse to justify a 30d day interpolation which results in the unusual NH4 twin peaks. It does not look like there is enough winter data to make any statement.

Page 245, line 12 on: The statement about the importance of the benthic system is not backed up by information on benthic-pelagic fluxes and other benthic dynamics. This is an important omission.

Small points: Page 220, line 17: suggest Ş..that nutrient dynamics are linked..Ť Page 220, 28: suggest Ş..processes for final closure ofĚŤ Page 221, 8-12: The authors and cited references refer to Baltic investigations, so I am not sure the use of the word global is correct. Do the authors really mean a regional or basin average?

Figures: Can the authors confirm the depths / depth bins used for both model and data in the majority of the figures.

The x axis are very compact. These figures could be enlarged to make use of the full page width.

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

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