Biogeosciences Discuss.,
doi:10.5194/bg-2016-50-RC2, 2016

Biogeosciences Discussions
Open Access
EGU

Interactive comment on "Modelling nutrient retention in the coastal zone of an eutrophic sea – a model study" *by* Elin Almroth-Rosell et al.

Anonymous Referee #2

Received and published: 29 March 2016

The model study of Almroth-Rosell et al. presents a sound model approach to improve our understanding of the nutrient retention in the archipelago oft he city of Stockholm. Through a combination of different models the authors estimate the retention capacities of nitrogen and phosphorous in different basins from nutrient sources to the Baltic Sea. The models which are combined here and the validation of the model is logical and well done, the results may be relevant for managers. The critical aspect here is the lack of a significant increase of our mechanistic understanding of processes leading to retention or the factors impacting retention. Moreover, the processes behind the retention are not clearly described and thus it remains difficult to fully understand how the retention works in this approach. The processes described are burial and for nitrogen also the process of microbial denitrification in sediments (probably also water column, when oxygen goes to depletion). Clarification of these aspects is required and some detailed

Printer-friendly version

Discussion paper

comments for improvement are given below.

The introduction needs more clarity and focus. The text jumps from general statements to specific Baltic Sea aspects e.g. hypoxia in the Gulf of Mexico is mentioned and next loads of nutrients to the Baltic Sea (p3 lines 8-14) or global eutrophication and loads to the Baltic Sea (p3 lines 18-22). I also did not understand why this example on retention (of nitrogen?) by plants is chosen (p4)? Why is the expression river outlet preferred over estuary (p4 line 18)? The description of the archipelago (p4 lines 23-27) is very brief and general. The more detailed description follows later. This should be combined or at least the reader should be referred to the later text. Further below the reduction of sewage is mentioned however again without details (line 31, reduction of how much N?). The a classification is mentioned (unsatisfactory eutrophic) but what is that really? What are high nutrient loads (p5 line 4)? On p.6 (lines 16-19) continue with somewhat vague site descriptions. Please add data on nutrient release over time and what the improvement of the treatment really means in concentration and load changes. Here again a combination of the text in the introduction with the study site description is would be better. What is the overall intention of this study – is it a retention estimate for managers, or are processes considered and their regulation by natural settings and anthropogenic impact? These are two different foci which impact the model set-up and the description of results. To me it seems that the authors mix both aspects with the results that neither aspect receives sufficient attention.

The model description is well done and clear in most cases. Minor requests for clarification are: The conversion of hydrogen sulfide into negative oxygen concentrations is sometimes used but seems not correct since it does not include the conversion of sulfate into hydrogen sulfide. What is the justification for this? How is the amount of N an P stored in sediments calculated (p 8 line 1)? How is nitrification and denitrification modelled (line 8)? Better focus the text lines 10-21 on the critical variables for this study, burial, remineralisation – assimilation and nitrogen fixation are less important. Is the atmospheric deposition indeed significant and deserves this much attention

Printer-friendly version

Discussion paper

throughout the text (see line 24)? Rivers hardly every supply nutrients with a N:P ratio of 16, usually the ratio is much higher (see p9 line 3-5). May be I misunderstand the calculation of the loads, but this needs conversion to true input ratios and concentration. The retention calculation (p11) is crucial for the manuscript. Please improve the description and explain R tot better, so that equation 4 becomes clearer. I could not understand the sentence p.12 line 22-23 about the validation of results and how representative stations are. Where are the cost functions from which are mentioned (p.13 line 25)? In case oxygen is not very well simulated by the model, then denitrification estimates cannot properly work in the model. How well is oxygen represented and how does that impact the results of the denitrification estimates? In this context the retention time of the water also plays a crucial role since longer retention times should lead to decreasing oxygen concentrations in the water. It would be good to dedicate a paragraph to the linkage of these variables and discuss the model results in relation to findings at the representative stations. In the present text the retention is qualitatively mentioned but quantifications are lacking (see paragraph 3.2.2). The paragraph on reduction scenarios is a pure description of model results but lacks aspects of a discussion – this would therefore need drastic shortening. It would be nice to significantly reduce number of figures.

Overall - as already mentioned above – the study would profit by a comparison of own results with other such model exercises. Although I understand why the paper was submitted to Biogeosciences it may be better placed in a model journal. As the paper stands now it does not explain the processes of nutrient retention or relate them to environmental processes (except the nutrient reduction scenario).

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-50, 2016.

Printer-friendly version

Discussion paper