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Ocean Science Discussions



# *Interactive comment on* "Model study on horizontal variability of nutrient N/P ratio in the Baltic Sea and its impacts on primary production, nitrogen fixation and nutrient limitation" *by* Z. Wan et al.

**Anonymous Referee #1**

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The authors use a 3D coupled circulation-biogeochemical model in the Baltic Sea in order to investigate the sensitivity of model results to the use of a varying NP ratio in the formulation of the phytoplankton nutrients uptake and regeneration. Most of the actual biogeochemical models assume that the uptake of inorganic nitrogen (DIN) and phosphorus (DIP) occurs with a constant ratio (usually Redfield) and this hypothesis may be inappropriate in certain cases. Also, the general thematic of the manuscript is of interest. However, the way the authors handle this critical problem can be strongly criticized and I have strong reserve about the methodology followed.



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Interactive Discussion

Discussion Paper

First, it is well known that a wide diversity of processes may lead to an unbalanced (compared to Redfield ratios) of DIN/DIP ratios in the water. For instance, there are: 1) Processes that affect both DIN and DIP cycles like phytoplankton nutrients uptake, hydrolysis of detritus, heterotrophs excretion and bacterial DIN/DIP uptake .. and all these cited processed may modify the DIN/DIP ratios in the water compared to Redfield, 2) Processes that affect only DIN or DIP and thus may change the DIN/DIP ratios. This is the case of denitrification (important in the Baltic), phosphorus adsorption, nitrogen fixation, chemical reactions. . . , 3) DIN/DIP unbalance can also be due to river and atmospheric inputs.

It means that the DIN/DIP ratio is governed by a wide variety of processes and is certainly not only governed by phytoplankton uptake. Also, the approach to try to correct phytoplankton uptake in order to obtain a better representation of DIN/DIP ratios in the medium is inappropriate because there are a lot of other processes that need to be investigated. For instance, the model does not involve an explicit representation of the microbial loop which is a severe limitation when one wants to investigate DIN/DIP ratios since this ratio is for sure affected by bacterial excretion and uptake in some cases, detritus decomposition (different rates for ÂÍPOP and PON), . . .

Based on the analysis of in situ data of DIN and DIP concentrations obtained before and after the spring bloom (spatial averages) the authors estimate the DIN/DIP consumption ratios and linked it to the phytoplankton stoichiometry. These DIN/DIP spatial data (one average per area) are then used to constrain the phytoplankton uptake of DIN and DIP and regenartion considering that the external DIN/DIP ratio is a proxy of the ratio of DIN and DIP uptake. For the arguments mentioned above, I do not agree with this assumption. We can not make the hypothesis that only the phytoplankton uptake is responsible for this unbalanced ratio in external DIN and DIP. In the abstract and elsewhere in the text, the authors mention that "In addition, variable N/P ratios greatly affected the model estimated primary production, nitrogen fixation and nutrient limitation, which highlights the importance of using an accurate N/P ratio" Again, the



OSD
9, C56–C59, 2012

Interactive Comment

external DIN/DIP ratio can not be considered has an accurate N/P ratio of phytoplankton uptake.

Comments on the validation Section Results, the authors mention that the model has been completely validated in Wan et al 2011. However, looking at this paper, it appears that the validation exercise is quite limited. This paper shows model results obtained for three values of the NP ratio of nutrient uptake 16, 10, 6 and compared these different simulations with some data (surface values and averaged vertical profiles at 3 stations). The validation of the model could be further substantially before pointing out one deficient process (phytoplankton uptake). Indeed, from the validation exercise this is not shown that phytoplankton uptake was the deficient process that need to be updated. Important: Looking at Figures 4, 5, 6 comparing the simulations of chlorophyll a, DIN and DIP with adapted NPratio, the improvement are not clear at all and for instance in Station A the agreement is worse and station P for phosphorus. Also, model simulations do not justify the use of a variable NP ratio derived from external nutrient data. The statistics shown in Table 3 confirm that , these statistics are absolutely not convincing and the "improvement" of using a variable is not significant at all contrary to what is sad by the authors (line 188, page 9, "The statistic results clearly show the model performs better in Scenario V2 than in Scenario NP10 (Table 3). Âż Even if the results were better it would not be due to the good reason.

Comments on the imposed NP ratio This is not clear how the DIN/DIP ratio varies during the year. It is estimated very roughly from some data sets collected at 9 stations before and after the spring bloom and then the obtained ratio is imposed throughout the year to a wide area ignoring the spatial variability of DIN and DIP dynamics. In laboratory experiments, the NP ratio of phytoplankton nutrient uptake may vary during the bloom starting with almost redfield values and then showing an unbalanced growth. Here, the same ratio is used during the whole year as I understood and this is a strong hypothesis. Since the manuscript has the aim to investigate the impact of the NP ratio of nutrients uptake this uptake representation should be refined. How do the authors



OSD

9, C56–C59, 2012

Interactive Comment

impose the spatial variability of NP ratios? Again errors may arise when deriving maps of NP ratios from 9 stations points and this is very questionable that the derived NP ratios will be better than the constant Redfield one for describing phytoplankton uptake. Finally, the conclusions of the manuscript are that the NP ratio of phytoplankton uptake is not Refiled. This is in disagreement with the findings of Osterroht and Thomas (2000)for the same area (as mentioned by the authors) and the justification brought by this manuscript is not convincing.

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Interactive comment on Ocean Sci. Discuss., 9, 385, 2012.

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9, C56–C59, 2012

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