

Interactive comment on “N/P ratio of nutrient uptake in the Baltic Sea” by Z. Wan et al.

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This is a little late but complete answer to Comment C306, along with those timely replies. A data analysis is added, which further supports the conclusion of the manuscript.

Q1: Why over time and space fixed N/P ratios were used and not a variable one (like in the work of Kuznetsov et al.)? A1: It is common to use fixed N/P ratios in ecological modelling. Otherwise, it is difficult to keep mass balance, especially for long time simulation of years.

Q2: Why did you ignore re-suspension of detritus. A2: It is just a simplification. This simplification will not cause a lose of detritus and its remineralized nutrients overall. It impacts only the remineralization rate and a spatial distribution of detritus. In fact, the remineralization rate is an adjustable parameter and the impact of spatial distribution

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of detritus on nutrients distribution is quite limited. Detritus is a less important state variable of operational purpose.

Q3: Why did you use WOA01 and not a more urgent database? To my knowledge ammonium is not included in the WOA-data – am I wrong? A3: The context described initial fields were generated on base of data downloaded from ICES website (through interpolation). WOA01 was used only to configure open boundary (English Channel and North Sea-Atlantic Ocean interface), where the data from ICES are limited. Ammonium at open boundary is ignored. The model has such a wide domain covering the North Sea so that the boundary condition has quite limited impact in the Baltic Sea.

Q4 (a suggestion): In chapter 3 I would still prefer to have one section for every N/P-scenario. A4: We would further consider your suggestion in revision.

Q5: The calculation of the DIN/DIP uptake on p. 1240/1241 is not convincing, since the DIP flux is missing. Furthermore the atmospheric decomposition is measured per m² (not m³). Why is the atmospheric decomposition allocated over 20m depth? Is this the mixing depth? Or do you need the 20m to get the N/P-ratio near 10? A5: First, the atmospheric deposition was measured in m². It was a mistype showing m³. I am sorry to say this comment does not make very good sense. If DIP flux and a deeper layer than 20 m are considered, the estimate of DIN/DIP uptake based on observed data would definitely be smaller than what was presented and it will more strongly supports the hypothesis that DIN/DIP uptake in the Baltic Proper during spring blooms is much less than Redfield Ratio.

Q6: The reference to Osterroht creates the question, why did you not run the simulation with a N/P-uptake rate of 16:1 but a non-Redfield remineralization, resp. what are your results worth if the remineralization is missing? A6: We cited this reference as a support for our analysis. A N/P-uptake rate of 16:1 but a non-Redfield remineralization is against the mass conservation law. No model can works with this assumption for a long time. I am presenting a further analysis on a rate of DIN variation to DIP varia-

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tion on base of nutrient data in Gotland Deep (57.33°N, 20.05°E) in years 2000-2009 (Fig. 1). The data were downloaded from ICES website. DIN variations in surface and averaged over upper 60 m or over entire water column are quite consistent with corresponding variations of DIP after linear transform. This documents that N/P-uptake rate is almost identical to N/P remineralization rate at yearly base, even if they may not be identical in one season at one depth. The linear transform coefficient of DIP is 6, 10 and 4 in surface, averaged over upper 60 m and over entire water column, respectively. None of them is even close to Redfield Ratio.

Q7: In fig. 5 I'm missing some curve of the wind stress, what about inflows of saltier and oxygen-rich water? A7: It was explained a bit last time. The circulation model was used for operational purpose in Denmark and it was intensively validated in Danish Waters, however, it was recently found that the simulated water in lower part in the Baltic Proper got fresher and fresher (salinity gets lower) and the simulated oxygen concentration was always much higher than observations. The cause was preliminarily attributed to physical model. This was stated in manuscript. The specific cause like either regarding to unrealistic mixing or improper advection is still under investigation. The added data analysis above shows the main conclusion of this manuscript is not misled by the model inaccuracy in simulating oxygen concentration in deep layers.

Fig. 1 Comparison of temporal variations between DIN and DIP. Black (red) dashed cycles for DIN (DIP). Panel A, for values in surface, DIP is linearly transformed through $6 \times \text{DIP}$; Panel B, for values averaged over upper 60 m, DIP is linearly transformed through $10 \times \text{DIP} - 3$; Panel C, for values averaged over entire water column, DIP is linearly transformed through $4 \times \text{DIP} - 3$.

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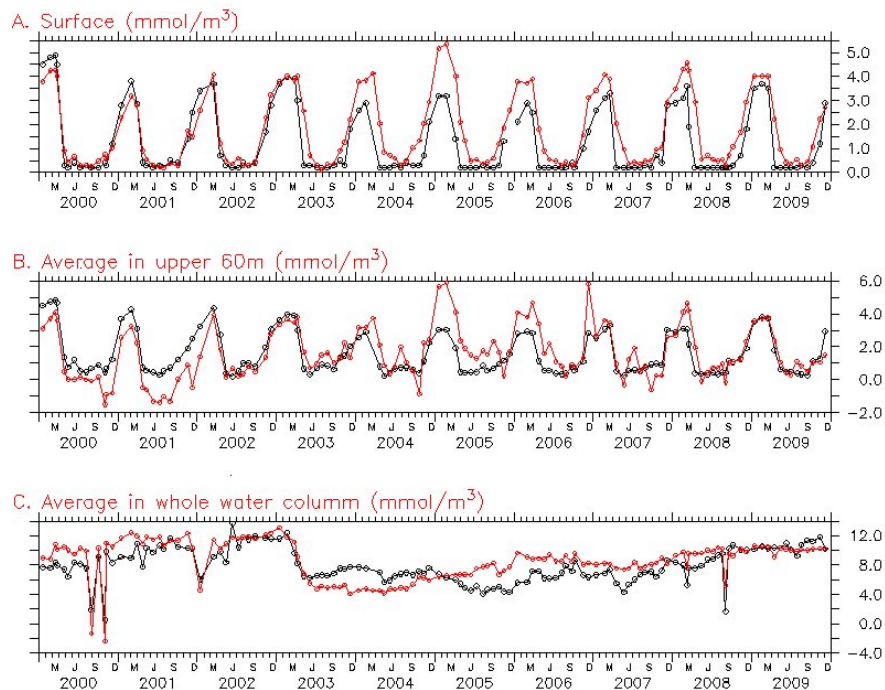


Fig. 1.

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