

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.*

The manuscript is a model study focusing on the effect of different N:P ratios in a biophysical model (ERGOM). The topic is interesting, and there are several publications and a lot of data indicating that the use of Redfield (as done in most models today) or other rates is an oversimplification and not a law of nature. With this background the authors has run their model with a number of different settings for nutrients uptake and remineralization in the Baltic.

Even if this is an important question to study, I am not convinced that the methodology used is appropriate to gain further insight in this. When choosing uptake rates based on the available data it is not a surprise that model performance is improved as these hardly can be said to be independent data sets. Also, by using the ERGOM model the authors fall into the same pitfall as they identify in the introduction (Page 386): ...phytoplankton are flexible in their overall stoichiometry, often matching their nutrient supply....., and,.....fixed elemental ratio is an oversimplification. As ERGOM are using fixed ratios for nutrient uptake and remineralization (the same ratio for both processes) it can not be used for a study like this. Nutrient remineralization from organic material should have different rates (N and P). This is done in most other models by adding at additional detritus pools. As ERGOM only have one detritus pool, it will always have problems with validation against nutrient concentrations especially during summer. Change in the ratio can of course change performance, but this is more like curve fitting. Therefore my conclusion is that the work should be rejected. Not because the topic isn't important and interesting, but since the methodology is inappropriate. I have a few additional remarks below,

- page 387, line 13. I have read this paper, and my conclusion were (as above) that the data suggest non-Redfield, but the study (using ERGOM) could not document this
- page 388, line 7. WOA01 is a 1 degree climatology and can hardly be used for the Baltic as initial field. This is also the case for the biogeochemics (even when adding data from ICES). The authors should refer to other model works in the Baltic (e.g. several publications by Neuman and co-authors that has also used ERGOM) on spin-up and adjustment of initial fields. This is crucial in the Baltic with it's long residence time. How long is the spin-up in the present study, and which years have been included? Is it only 2007+2008??
- section 2.5. This is interesting, but if the authors believe what they wrote in the intro (phytoplankton flexibility to match nutrient supply), I would question if this is relevant