Peer review on Neumann et al. "Non-Redfield carbon model for the Baltic Sea (ERGOM version 1.2) – Implementation and Budget estimates." By Tatsuro Tanioka

In this model description paper, the authors introduce non-Redfieldian C:N:P uptake stoichiometry into the biogeochemical model ERGOM v1.2 and study the model performance in the Baltic sea. By introducing a non-Redfieldian uptake ratio, the authors demonstrate that model estimates for pCO2, macronutrients, and oxygen improve compared to the previous version of ERGOM with strict Redfield C:N:P.

While I believe that the authors evaluated important issues for the modeling carbon cycle in the Baltic Sea, the study suffers from significant technical and model experimental design flaws. In particular, the assumption that cellular C:N:P of healthy phytoplankton is fixed at the Redfield ratio is not supported by either observation or previous modeling studies. Furthermore, almost identical issues in the Baltic Sea (underestimation of model pCO2) have been previously resolved by the model study (Fransner et al., 2018). It is not clear how the current study is a major improvement over the study by Fransner et al. and other numerous modeling studies (Kuznetsov et al., 2008; Kreus et al., 2015; Wan et al., 2011) that introduced non-Redfieldian stoichiometry to improve carbon cycle, nitrogen, phosphorus, and oxygen dynamics in the Baltic. In summary, this paper provides only an incremental advance in our understanding of biogeochemical cycling in the Baltic Sea, and I do not believe it is worthy of publication in GMD.

General Comments:

1. The assumption about fixed C/N/P of phytoplankton cells: the authors assume that "the stoichiometry of healthy phytoplankton cells follow the Redfield ratio." (line 45). However, this is likely not the case for phytoplankton in the Baltic, especially diazotrophs that thrive over summer following spring bloom (Larsson et al., 2001). C:N:P of suspended POM varies seasonally (Heiskanen et al., 1998). Previous non-Redfieldian models have considered these observations and described variable C/N/P of phytoplankton in the Baltic (Kuznetsov et al., 2008; Kreus et al., 2015; Wan et al., 2011). My enthusiasm for this study is dampened because the authors do not cite or show any evidence supporting their assumption that the C:N:P of phytoplankton in the Baltic is fixed at the Redfield ratio.
2. No rigorous assessment of how the model improved from the Redfield model: The authors do not fully quantitatively assess how incorporating non-Redfield C:N:P improves modeling nutrients and oxygen dynamics. In addition, a poor model fit of Alkalinity to observation by almost 200-300 uM raises concern about whether the carbon cycle model is correctly configured and parameters are tuned.

Other Comments:

1. L16: It may be more appropriate to call it "non-Redfieldian," not "non-Redfieldish" throughout the text.
2. L46: What is the justification that phytoplankton are healthy in the study area? Does healthy imply nutrient-replete? If so, please justify.

3. L47-48: The study by Martiny et al. (2013) clearly demonstrates significant deviation in C:N:P of suspended POM in the nutrient-deplete and nutrient-replete regions.
4. L56 “Strong observational evidence for evidence for an ER of DOM”: Please provide ER evidence in Baltic.
5. Figure 3 and L 123-125: What exactly are these relationships between DIN/DIP and C:N:P in terms of equations? How are they derived?
6. Figure 4: Where are the heterotrophic bacteria that feed on DOM?
7. Figure 6: Please define what “organic matter” means? Is it DOM + POM or POM only?
8. L187-L188: “We have to note that our model approach does not allow for C:N and C:P ratio below Redfield ratios in the DOM and POM fractions.” What is the justification for this? C:N and C:P in nutrient-rich regions such as the Southern Ocean are below Redfield ratios of 106 and 6.625 (Martiny et al., 2013).
9. Figure 13: It would be helpful to plot the Redfield case as a control.

<u>Reference:</u>

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