

## ***Interactive comment on “MOPS-1.0: modelling the regulation of the global oceanic nitrogen budget by marine biogeochemical processes” by I. Kriest and A. Oschlies***

### **Anonymous Referee #1**

Received and published: 3 April 2015

This paper describes modifications to a previously published 3d global biogeochemical model, resulting in what is now called ‘MOPS’. The paper also discusses a few specific sensitivities of the model.

I am glad to see further development of the authors’ very-promising matrix-based platform, and there are some interesting results in the sensitivity study. However, I found the paper somewhat hard to access. It wasn’t obvious to me at the outset what the paper was about, or how it fits into the context of the literature. I think it would benefit from an edited abstract and introduction that more clearly lays out the purpose, and that highlights the key results of the sensitivity tests.

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Details regarding these suggestions, as well as further comments, are outlined below.

– General comments –

The paper tests a few very specific uncertainties: the vertical distribution of particulate remineralization, and the half-saturation constants of O<sub>2</sub> and NO<sub>3</sub> in remineralization. The sensitivity of the model to the remineralization half-saturation constants is particularly useful. However, these targets of investigation are somewhat buried in the article - they should be clearly stated in the abstract and introduction, and perhaps even the title. A couple of other complexities - such as anammox - are discussed in the paper, which is helpful, but are not a focus since there are no experiments to explore them. At the same time, some other processes are left out entirely, such as the iron cycle, benthic denitrification, and variability in organic matter stoichiometry. That's fine, but in view of the specificity of the uncertainties tested, the title and opening of the paper seem unnecessarily vague about what the paper is actually going to address ('marine biogeochemical processes'). Along with this, it would be great to include some reasoning for why these specific uncertainties were chosen for this study, and to better highlight the results.

It would also be helpful to have some motivating statements about the model. Why was MOPS made? How does it compare to similar models, and what niche does it fill? Who should be rushing to download and test this model?

Benthic denitrification is not included in the model, even though it probably accounts for at least half of the total fixed nitrogen lost. I didn't see a scientific reason for not including it, it seems to be more because it simply hasn't been coded - the reason should be clarified on page 1949. I don't think its absence is a big problem, but it should be pointed out as a caveat in a couple of places where it will certainly impact the results. For example, the global distribution of nitrate concentrations would definitely be altered by the inclusion of benthic denitrification, given its different horizontal and vertical distribution relative to pelagic denitrification, and significant contribution to

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overall N loss. I would expect the nitrate concentrations to decrease in most of the ocean if this missing process were included, shifting most of the volume frequencies to lower concentrations. The conclusions based on nitrate concentration comparison with data should be reworded slightly in order to reflect this fact. The use of constant N:P stoichiometry should also be mentioned as a caveat, given that it has been shown to be important globally (e.g. Weber and Deutsch, 2012).

The paper talks a lot about ‘particle sinking speeds’. However, the parameter  $b$  reflects both sinking speed and remineralization rate. Thus, the results can be viewed equivalently as sensitivity tests of remineralization rate, just as much as sinking rate.

– Specific comments –

**Abstract** The abstract uses a lot of vague language. It would be more helpful to make it more specifically focused on the methods used and the results.

p. 1950: ‘... thereby parameterizing some form of “implicit denitrification” without explicitly accounting for other oxidants beside oxygen...’ I don’t think it’s fair to say it’s implicit denitrification, since this would imply a change in nitrate limitation. Better to say ‘implicit non-oxygen oxidants’.

p. 1952: I don’t understand how a relaxing of  $\text{NO}_3$  towards an N:P of 16:1 is ‘based on’ Breitbarth & Laroche. Also, the authors in this reference are listed backwards throughout.

p. 1967: The idea that the final state could depend on initial conditions has not been previously introduced in the paper. Why would anyone expect multiple equilibria in this model? There must be some literature on the factors that produce multiple equilibria that could be cited somewhere?

Section 4.4 doesn’t seem to add much to the paper - I think it could be removed.

Appendix

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- How does the matrix deal with physical mixing within the mixed layer? A couple of sentences about this would be nice, given the importance of mixed layer dynamics for biogeochemistry.

- Temporal discretization should be described. Is the circulation annual mean? What are the timesteps?

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Interactive comment on Geosci. Model Dev. Discuss., 8, 1945, 2015.

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