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Biogeosciences Discussions

Interactive comment on "Nitrogen transfers and air-sea N_2O fluxes in the upwelling off Namibia within the oxygen minimum zone: a 3-D model approach" *by* E. Gutknecht et al.

Anonymous Referee #1

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The manuscript describes results of a newly developed biogeochemical model of the nitrogen cycle (BioBUS) that has been implemented into a regional sigma-coordinate model of the Namibian upwelling region. The manuscript consists mainly of a comparison of model results with observations or observational estimates of biogeochemical tracer distributions. This comparison is very detailed and appears objective. Overall, the model seems to describe many features of the Benguela upwelling system much better than other models do, although this is not firmly demonstrated by the authors. The few examples where a comparison to earlier (box) model results is given, the results are very similar.

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Interactive Discussion

Discussion Paper

I value the paper as a fine example of a model validation. However, the paper neither demonstrates significant improvement compared to earlier model results, not does it address a clear scientific question. The main scientific result is the quantitative assessment of the lateral export of nitrogen out of the upwelling region into the open subtropical South Atlantic. I am unsure as to how new this really is. Presumably, this transport could be diagnosed from observational data bases and an estimate of the Ekman transport (e.g. Williams and Follows, DSR 1998).

Overall, I think that the manuscript is more a model description (though some details like the formulation of "burial" still need to be described in the manuscript) than a scientific biogeochemistry paper. Because of the lack of scientific questions (and answers) in the relatively lengthy manuscript, I cannot recommend publication in Biogeosciences but would recommend publishing this paper (after revisions) in a journal like Geophysical Model Development. I am not sure whether one can easily transfer papers from one EGU journal to another, but this would certainly be a very good opportunity to do so.

individual comments:

The model is a new configuration (the "Namibian configuration") of the ROMS model. The physical model is evaluated against hydrographic observations of T and S. It is not clear whether model has reached a seasonally cycling steady state, yet. On p.3548, lines 25-27 may suggest that there is still some substantial temporal drift in the model results. To convince the reader (and me) it would be good to show a time series of some properties like upwelling transport, NO3/O2/N2O concentrations.

units for O2 concentrations should be changed to umol/l (or umol/kg or mmol/m3. ml/l is difficult to assess in combination with all the other N and C fluxes that are given in molar units.

The treatment of the sediment and the way "sequestration" is computed do not become clear. Equations in the Appendix seem to be valid for the water column only. Does remineralization of detritus stop in the sediment? Is there a sediment layer below the last grid box of the water column? Otherwise, sediment material would be still subject to advection, wouldn't it?

p. 3540, line 26/27. N2 fixation is not necessarily restricted to the ocean-atmosphere interface. There is enough N2 gas resolved in sea water everywhere.

p 3542, l.4 & 6. "suboxic..." "During these anoxic events" not clear what is meant here. Are you referring to the same suboxic=anoxic events?

p3542 , l.14. "alleviate". This does not seem to make sense.

p. 3545, l22-24. Is there no N2O consumption at very low O2 concentrations? If so, this could perhaps be stated explicitly.

p.3546., l.12 ""is better simulated as compared to data". What is meant here? better than what?

p.3547, l.21, Are maximum growth rates given for a temperature of 0 degrees Celsius ?

p.3550, section 4. Please specify whether the model is interpolated onto the observed data or vice versa. Also, it would be good to say whether all data are assumed statistically independent from each other and whether there is weighting applied to account for the different volume of different model grid boxes.

p.3551, l.17ff. state whether you refer to salinity units (psu) or whether you refer to normalized biases.

p. 3553, l.4/5 "simulated salinity is weaker than measured salinity" ?

p.3553, l18-20. It would be good to see whether the model has been spun up sufficiently, i.e. how large the remaining drift is in physical and biogeochemical model fields.

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p.3554. It might be useful to quantitatively compare simulated and observed volumes with oxygen concentrations lower than, e.g., 50umol/l.

p.3556, l.19-21. Could the overestimate of O2 and underestimate of NO3 be explained by too weak upwelling?

p.3556, l.23-28. This reads as if there is good agreement everywhere. The authors should say that the satellite data show highest chlorophyll over a wider area in the north, which is in contrast to the model behavior that shows highest chlorophyll and a widest off-short extension in the south.

p. 3560, l. 29. I agree that the simulated values come closer to the in-situ values, but it should be said that there is still a factor 2 (or 100%) difference.

p.3561, l.25-27. Why should the more complex parametrization of Freing et al. help? AS I understand, Freing account for nitrification process only, and they apply their method to north Atlantic data only, i.e. they do not have to deal with very low oxygen.

p. 3563, l. 16 "a contribution decreasing towards the shore" l. 18 "represents a nitrate sink"

p. 3563, l.25. Is the high f-ratio of about 0.9 in agreement with observations?

p. 3566, l. 27. Is there any formulation of anammox that does not require the simultaneous presence of NO2 and NH4?? Why is the Yakushev et al. formulation to blame here?

p. 3567, l.2. Woebken et al. reference is missing. (there may be others, but this is the only reference I checked - bad luck).

p. 3567, l. 10 Please explain how exactly the "burial flux" is computed in the model without sediment?

p. 3568, l.23. These numbers are given for 22-24S, while figure 18 shows numbers for

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the entire latitude range of the mode. This is confusing. Why not show and discuss either the total region or the Walvis Bay alone?

p. 3569, l.15. Does this confuse mol N2 versus mol N in Fig. 19? Otherwise I do not understand.

p.3569. should "air-sea flux" read "sea-air flux"? It is somewhat difficult to understand that the model simulates N2O concentrations too low by at least a factor 2 and at the same time simulated air-sea (sea air? this is unclear in the text) are too high, at least off-shore. What could be the reason for this apparent inconsistency?

p. 3580, eq.A26/27. Is there no light inhibition of nitrification in the model? This might be useful to point out.

Interactive comment on Biogeosciences Discuss., 8, 3537, 2011.