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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 #2

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General Comments:

Gutknecht et al. present a complex and powerful model for assessing productivity and nutrient transformations in the Namibian upwelling system. The model is impressive in it's inclusion of a complex nitrogen cycle that includes rate limited denitrification and anammox reactions. To my knowledge, few, if any, 3D regional models incorporate these processes in such a detailed way. In general, the authors do a very nice job comparing the model with observational data. The model appears to simulate temperature, salinity, NO3, and chlorophyll very well.

However, I do not think the authors spend nearly enough time explaining the substantial

changes they have made to the original biogeochemical model by Kone et al. (2005). Several new tracers have been added (O2, DON, N2O, NO2) as well as multiple step denitrification, nitrification and anammox rate-limited reactions. The equations for these reactions appear to come directly from Yakushev et al. (2007) and are never explicitly discussed, justified, or validated. To this point, the authors do not state whether any testing of parameter values was performed despite the fact that Yakushev et al. present a 1D model for a very different aquatic ecosystem with many additional model components. Because the use of this complex nitrogen cycle will be of major interest to other 3D biogeochemical modelers, I strongly recommend moving the governing equations from the appendix to the main text in a separate section dealing specifically with denitrification, anammox, and how the tracers NO2, NO3, and NH4 are calculated and sensitive to changes in the rate coefficients.

The need for additional N-cycle model evaluation becomes apparent towards the end of the paper when the denitrification and anammox rates calculated using the model are presented. The authors acknowledge that both rates are lower than observations, but do not point out that significantly more water column N is lost in their model through denitrification than through anammox, in opposition to the observational findings of Kuypers et al. (2005) in the Benguela upwelling. I very much recommend that they do some additional testing of the model sensitivity of the N-cycle rate coefficients. Also their N2O production parameterization could be significantly improved, but probably not by using the parameterization of Freing et al (2009) as they suggest, but with one that explicitly includes N2O production via denitrification. In addition, because anoxic sediments are an important feature of this region, including a sediment model that includes organic matter remineralization via aerobic processes as well as anoxic denitrification seems crucial for accomplishing the goals stated at the beginning of the paper: to investigate the full N budget in the Namibian sub-system of the Benguela Upwelling System.

Although this is the stated goal of the paper, this topic comprises a surprisingly small portion of the text due to a lengthy model validation section, which as mentioned previously, fails to validate or discuss some of the most interesting and complicated features of the model. I also have some concerns regarding the N budget. The authors never state whether the annual nitrogen fluxes through their region of interest balance. This would be a good indication that the model is in steady state. Using the numbers from Figures 18 & 19 for the top 100m over the slope, I calculate a net loss of inorganic N (-8.9 x 1010 mol N yr-1) and net gain of organic N (+2.38 x 1010 mol N yr-1). The magnitude of the net gain or loss is larger than some of the advective fluxes. I may have computed this incorrectly, but the authors should address this point and convince the reader they are presenting a balanced N budget.

On a more conceptual level, I'm undecided about whether this paper, even after revision, can be successful in its aim to provide a complete and realistic N budget for the Benguela Upwelling system given the lack of testing/validation of the denitrification and anammox modeling, it's lack of a sediment model, and weaknesses in the N2O parameterization. Their finding that EBUS can provide nutrients to ocean gyres is not terribly surprising and their number for this source will need to be revised as soon as a more complete model is available. In addition, although I think that evaluating the model is very important and requires some additional effort, it also makes the paper very long. The application of the model to answer a scientific question does not come until very late in the paper when many readers interested in this particular topic will have already lost interest. I would recommend publishing a separate model validation paper and a shorter, to the point paper about the N-budget of the Benguela Upwelling System.

Specific Comments

p 3539 ln 9 : "alleviate" seems an odd word choice here, perhaps "potentially diminish" would be more appropriate. Starting the sentence with "However, losses of ..." would also put the sentence in context right away.

P 3539 ln 18: awkward sentence - "over the first 100m over . . . over . . .". I recommend

changing the first use of "over ..." to "into the top 100m of the water column".

P 3540 ln 15: "of the global ocean, its estimated..." I recommend rewording to "of the global ocean, however we estimated it's N2O emissions using a parameterization based on oxygen consumption to be 4% of the ..."

p 3541 ln 20: "As for the other... equator: the Benguela ..." This sentence does not make sense. I believe it should read "As with the other EBUS, the trade winds maintain a horizontal pressure gradient along the coast associated with a coastal geostrophic current flowing towards the equator. In the BUS this coastal current is called the Benguela current and contains cold, nutrient-rich waters."

p 3541 ln 24: "under the form of eddies" should be "in the form of eddies"

p 3542 ln 4 & 6: These sentences are a bit unclear in their description of where and when suboxic zones, anoxic zones, and anoxic events occur.

P 3542 ln 14: "alleviate" again this is an awkward word to use, "mitigate" or "diminish" would work better.

p 3542 ln 19: "more efficient than CO2..." this is too vague- more efficient than CO2 at doing what?

Pg 3543 ln 7: Questions two and three should be more specific – what is goal? Estimating the magnitude of the N loss and the N2O production in this area? or the nature of the N loss? its seasonality? Etc...

p 3543 ln 15: Just stating that an N budget will be presented is again somewhat vague. Since this appears to be the main scientific goal of the paper a few more lines describing the approach, for instance, including which biogeochemical and physical mechanisms are included in a flux analysis performed to obtain the budget, would be helpful.

Section 2

General Comment:

I recommend describing the Namibian model configuration directly following section 2.1, which describes the hydrodynamical model. Then move onto the biogeochemical model is a larger change in topic.

General Comment:

It appears that some substantial changes were made to the biogeochemical model for this study. DON, NO2, N2O, and O2 state variables were added as well as the rate-limited nitrification, denitrification and anammox processes. I think greater explanation and the equations governing these processes and the new state variables should be presented in the main text. Many biogeochemical models parameterize nitrification and denitrification more simply (dependent on O2, N, and detritus concentrations) and ignore anammox (PISCES, BEC, HAMOCC). Therefore it is a major acheivement of this study that it uses a more complex and detailed representation of these processes.

However, the study cited as a description of the complex nitrogen cycle (Yakushev et al. 2007) is a 1D model designed to simulate a number of redox processes in the Black Sea. Simply citing this paper does not provide an adequate explanation of how this model works or these processes were incorporated into BioBUS. The Yakushev et al. model includes several other variables and processes which were not incorporated into BioBUS with rate coefficients obtained by tuning to model to produce observed concentration profiles. This is fine for that application but the authors must discuss what (if any) rates were changed from Yakushev et al., where those coefficients were obtained to begin with, and at a minimum a basic explanation of the reactions and equations. This is not trivial as multiple steps are involved due to the addition of the state variable N2O. An entire section in the methods on the addition of new N-related tracers and the calculation of denitrification, annamox, and nitrification rates should be provided.

P 3545 ln 8: It's interesting that a DON tracer what added. Often models use a slowly remineralizing, sinking, large detrital pool and a small more labile detrital pool that may

or may not sink at all and is functionally a DON pool. Since we now have three pools of non-living organic N to consider within the context of a complex nitrogen cycle, a sentence or two comparing them in terms of remineralization, sinking rates, and their interactions would be helpful. Figure two shows large detritus being remineralized to NH4, Small detritus becoming DON, and DON being remineralized to NH4. Is this correct? Why does large detritus become NH4 directly but small detritus first becomes DON? A more detailed explanation and rationale for this complexity is warranted.

P 3545 ln 16: A bit more explanation of the addition of O2 as a state variable is necessary, at a minimum please provide a citation. [→ I just saw that this is addressed in short appendix, not noted in this section of the paper. I recommend removing the appendix section and adding the relevant sentence here.] Also O2 appears to be calculated in mmol O2 m-3 (Table 1) which would be a much more intuitive unit to use when discussing O2 concentrations in the text, as it's more easily comparable to the nitrogen units reported.

P 3545 ln 21: Figure one contains some confusing arrows that are not explained. Large phytoplankton become small detritus directly, but what process do the arrows branching off of phytoplankton as they flow toward zooplankton that are re-routed to detritus signify?

P 3546 ln 16: In the parameter adjustment experiments, was each parameter changed independently? And why were certain parameters chosen for sensitivity analysis and not others? There are so many interrelated processes occuring in this model it seams that changing some together or those that are the least well known or constrained by observations would make sense. For instance, when you compare changes in the DON mineralization parameter KND4 to the distribution of NO3 and O2, wouldn't it make sense to also consider some of the other parameters affecting DON on it's path to becoming NO3 (DON → NH4, NH4 → NO2, NO2 → NO3). Testing values for either of the nitrification rate parameters could be just as useful KND4 as they are relatively uncertain and affect both NO3 and O2 directly. Just a sentence or two explaining why some parameters were tested and others not (if they were not) would be appreciated.

P 3553 ln 12: Once I searched and finally found the paper referred to (Monteiro and van der Plas 2006) I found the comparison of the mooring data and figure 11 quite good. But this in situ data should be presented and printed in THIS paper. The reader should not have to search and obtain a relatively obscure article and flip to a figure in the middle of the article to see if the what the authors say about how their data compare to observations is reasonable. If it's not possible to publish a figure with the mooring data in this article, the paragraph should be reworded to simply describe the temporal dynamics in the model data and state that this is in good agreement with mooring data published in (Monteiro and van der Plas 2006). Also, why is there a jump in figure number from 7 to 11. Figure 11 should become figure 8.

P 3554 ln 27: Could slightly too high oxygen and too low NO3 between 200-40m (Fig. 8) be due to underestimated nitrification? Your parameterization doesn't allow it to proceed in the euphotic zone, but there have been several studies that have observed nitrification in low to even moderate light. However, it doesn't sound like NH4 concentrations are high enough here to increase nitrification rates.

P 3557 ln 24: Although your model produces a deep chlorophyll maximum, the gradient between the surface and deep chl max is not nearly as steep as in the observations. This is fine but should be noted in this assessment section.

P 3558 ln 19: "Spatial variations are important." An additional figure would be really helpful for this paragraph comparing model and the observations described. Also, why not compare with a satellite algorithm for primary production?

P 3561 ln 12: Over what time scale is the modeled data presented? Is this an average for climatological December, or the year, or a shorter time-scale? Temperature agrees well in Figure 17 a, but none of the other variables agree well.

P 3561 ln 21: "Simulated N2O concentrations have similar values as compared to data

for waters with O2 > 2.6 ml L-1". To me it looks like N2O agrees well only at O2 levels above ~5 ml L-1 and that ignores the high N2O throughout the surface waters close to the coast.

P 3561 ln 27: I don't know if using the Freing N2O parameterization will improve the modeled N2O profile very much. It also only takes nitrification into account. A simple parameterization based on Suntharalingham et al. (2000) that accounts for N2O production via both nitrification and denitrification can be found in Dutreuil et al. 2009 (Biogeosciences, www.biogeosciences.net/6/901/2009). Another problem could be that in the current parameterization no nitrification can take place in the euphotic zones, though this is sometimes observed (Dore and Karl 1996, Wankel et al. 2007). Maybe altering the light/depth dependence on nitrification rates would help reproduce observed distributions.

P 3563 ln 17: "poleward undercurrent..." Why not specify if the meridional advection has a net flow to the north or south? From the description is sounds like the alongshore Benguela current is a net flow from the south into the budget box, and the 100-600m box over the slope is a net southward flow.

Also what is meant by "sink for the studied area with a maximum value"? Do these fluxes balance to a net zero over a year? After a calculating the sum of the organic and inorganic fluxes into and out of the top 100m of the slope box it appears there is an imbalance (-8.8 x 1010 mol N yr-1 inorganic, + 2.38 x 1010 mol N yr-1 organic). Whether the fluxes balance over an annual cycle for each box should be addressed.

P 3564 ln 1. The areas used in calculation of PP in these comparisons are not well explained. The area of the Walvis Bay used in the budget (I think this is the area used for the first two numbers presented) seems to be smaller and more productive than the "entire Walvis Bay" referenced a bit later (ln 9), but how does this compare to the area of the BUS used to calculate PP by Ware, Carr, Tilstone and Brown? Even some rough estimate of the approximate differences in areas would be helpful here. Or maybe a figure.

P 3566 ln 12: The reference to Kuypers et al. (2005) is very confusing. It appears the authors are providing 0.075 – 0.25 mmol N2 m-3 d-1 as the in situ rates of N2 formation associated with denitrification. However, I cannot find any denitrification rates in Kuypers. In fact, Kuypers et al. finds little to no evidence for significant denitrification, attributing the majority of fixed nitrogen loss to anammox. Lavik et al. (2009) does present but not discuss one denitrification measurement but the provenance of the cited rates is not clear. Also, it should be noted that the BioBUS model predicts significantly higher denitrification rates than anammox rates for fixed removal from the water column in contrast to Kuypers et al. This seems to warrant some testing of parameter values used in the complex N cycle.

P 3567 ln 10: Some conclusion should be given as to whether the PON/POC reaching the sediments is reasonable compared to observations. Lots of numbers with different units are given and it gets confusion. It appears that the BioBUS model overestimates PON/POC burial on the continental shelf, but some clarification should be provided about which areas should be compared directly between the model and observations.

P 3568 ln 25: Why assume a horizontal surface for the S. Atlantic gyre equivalent to that of the N. Atlantic? Is the same area being used or just the same approach for estimating the area? Why not just specify the area you estimate and use for the calculation.

P 3570 ln 27: Why compare your modeled N2O fluxes with observations if the conditions are not similar (oligotrophic)?

P3570 ln 15: An important deviation? Section 4.5 does not clarify what is meant here. It refers to figure 17 where a large difference between modeled N2O and measured N2O is shown, but I don't see why surface fluxes cannot be calculated as they would be at the other stations.

Pg 3571 ln 15: The model values seem on the low side of the other estimates mentioned. Especially the ones from the Mauritanian upwelling.

P 3571 ln 21: Why not area-normalize these modeled fluxes as well as the fluxes by Nevison et al.? This would reduce the amount of information presented, which is confusing, and be much simpler and to the point.

P 3572 ln 11: I'm puzzled because the model results show that the N2O fluxes from Walvis Bay are quite high compared to other upwelling areas. But N2O formation associated with denitrification is not included in the N2O parameterization and modeled N2O fields are much lower than observations (50%) in low O2 regions (Fig 17). Are these high fluxes reasonable given that N2O seems to be underestimated in the model?

P 3574 ln 28: Again, Freing et al (2009) does not include denitrification in its calculation of N2O, which I think is what is implied by "...N2O formation process associated with suboxic processes" in the previous sentence. I recommend looking at Dutreil et al. (2009).

p 3575 ln 1: Making the N cycle even more complex in your model may not be the most effective way to increase anammox rates. What about testing changes in the rate coefficients governing NO2 production (1st stage of nitrification, etc)?

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