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Biogeosciences Discussions

Interactive comment on "Nitrogen cycling in the Central Arabian Sea: a model study" *by* A. Beckmann and I. Hense

Anonymous Referee #2

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The manuscript submitted by Beckmann and Hense on "Nitrogen cycling in the central Arabian Sea: a model study " uses a biogeochemical model to investigate nitrogen and oxygen cycling in the suboxic layer of the Arabian Sea along a 2D transect. In particular the contribution of various processes of nitrate and nitrite transformation rates to the nitrogen budget of the suboxic zone is estimated and compared with other published values. The scientific questions raised in this study are of key importance in the region since they are still a lot of uncertainties on the nitrogen budget in the area. However, the methodology followed by the authors and especially the modeling exercise that is done presents serious weak points that may compromise the publication of the work.

My main criticisms can be summarized as follows:

1. This is not clear how are derive the physical fields used to force the model. The

authors use 2D mixing coefficients and advection velocities to force the biogeochemical model. From what is said, it seems like the authors pick up in different models (e.g; Miyama et al 2003; Lee 2004) physical fields without being sure that the obtained field is afterwards dynamically consistent. They seem to adapt the structure of this field in order to simulate gradients in the biogeochemical fields that agree with the data (shown Figure 1). We would expect that the adjusted fields are compared afterwards with some other modeling initiatives in order to check if the obtained fields are still acceptable. The authors may envisage that an inaccurate parameterization of some processes (too low rates) may also lead to a "bad" distribution of biogeochemical variables. What about the temperature? This is an important variable that affect biogeochemical processes and no information is given on how this field is estimated.

2. The hypothesis of considering an invariant physical field is a strong limitation and would deserve some more justifications. The authors consider that the seasonal time scales are not important for the subsurface distribution. This is an hypothesis that seems to be contradicted looking at Figure 1b where we see that the oxygen and nitrate profiles show gradients at 100m. This is not obvious that the position of the oxycline is not affected by the seasonal mixing. Moreover, even if the suboxic layer is less directly affected than the surface layer by the seasonal cycle, the particle flux export to the suboxic zone is seasonal and this will impact the suboxic layer.

3. Even if the main focus of this manuscript is on the suboxic layer, the ability of the biogeochemical model to simulate the upper layer biogeochemistry is necessary since this is the flux of detritus that will drive the nitrogen and oxygen cycling of the suboxic layer. I would appreciate to see a convincing assessment of the performances of the biogeochemical model. We can deplore the lack of validation exercise performed in this work, the authors are satisfied that basic characteristics of the oxygen and nitrogen profiles are reproduced by the model and afterwards they use the tool to derive novel hypothesis. A variable that can be a good candidate to appraise model performances is N2 (adding a new state variable). I have some comments on the parameterization

used (see my detailed comments).

To summarize, there are too many degrees of freedom in this study: the physical fields are badly constrained and are adapted to reproduce the oxygen and nitrogen profiles although this is not sure that the misrepresentation of these profiles is due to a “bad” physics, parameters used to express processes like DNRN, DNRA, N2RN, ANAMMOX, A-DENIT are also poorly constrained, the biogeochemical model is poorly validated especially in the upper layer, the influence of lateral boundary conditions on the quality of model results may be important since data profiles are imposed, the imposition of a surface and lateral flux of nitrogen and oxygen for which very few information are available (the surface conditions for oxygen is notably very crude and this is not clear how nitrogen fixation is parameterized, this process is mentioned in the text but does not appear in the equations). Due to these large number of poorly constrained processes and the lack of a thorough validation exercise I do not think that this model can be used afterwards for diagnostic purposes.

Detailed comments

Abstract

Section 1, page 2 Line 4: incomplete sentence

Line 9-10: please add a reference after system

Line 10: What do you mean by “in both denitrification process”?

General comment on page 2 and 3: This is sometimes confusing to understand the exact process to which the authors are referring. For instance, is annamox not a denitrification process? The authors list the processes that may occur in suboxic conditions affecting the nitrate and nitrite contents. All the processes listed between lines 5-13 lead to a loss of nitrate or nitrite. Can not they all be considered as denitrification processes? For instance, line 12-13, the authors mention autotrophic anammox as an example of denitrification (in both denitrification processes . . . as mentioned at line 10).

Please give some clarifications.

Line 20 DNRN has already been defined.

Page 3

Line 6: anammox is also ammonium oxidation.

Page 6: please give some details on how the atmospheric inputs and lateral inputs are imposed and how they have been estimated. Besides, how are model results sensitive to the values imposed? The values imposed will probably strongly conditioned the amount loss by denitrification and export processes. Therefore, the way they are constrained is essential. This is a crucial point that the authors need to address.

Lines 5-9: The authors mention that “they exclude as many complicating aspects of the physical environment as long as the main phenomenology is capture and general quantitative agreement is obtained”. This is not a trivial task to identify what are the main processes that will influence the dynamics of the suboxic zone. It may require to start first with a complex framework and performing sensitivity studies in order to appraise what are the driving mechanisms. I would like that the authors clarify how they deal with this complex question.

Besides, it would be helpful that they clarify which complicating aspects they ignored, what are the main phenomenologies that need to be captured. . .

Section 3.2 page 6

Line 7: I would suppress the first sentence since it is repeated afterwards

Page 7, lines 6-8: it is not clear why the oxygen budget is not balanced in the model. Could you please show a scheme with the oxygen flows (including air-sea interactions) and transports?

Line 28: this is a very subjective choice that would deserve some comparisons with other modeling work considering the importance this choice may have on the solution.

Page 8

Line 1: please add a reference

Line 2: Please add a reference

Line 8: do you mean an inhibition by oxygen?

Equation for oxygen: I suggest to use parameters instead of directly 0.5 and 1.5 as done with the other terms.

Page 9

Line 10: why the authors are not using a classic monod function and 1-Monod to describe the limitation and inhibition by oxygen? The chosen formulation makes the mode really dependent on the value selected for "Theta".

The surface boundary condition for oxygen is very crude. Normally the flux is computed from a saturation concentration

Page 10

Line 20: the authors consider that the seasonal time scales are not important for the subsurface distribution. This is an hypothesis that seems to be contradicted looking at Figure 1b where we see that the oxygen and nitrate profiles show gradients at 100m. This is not obvious that the position of the oxycline is not affected by the seasonal mixing.

Page 11 Line 9-11: This is not clear what was the physical model that provide the physical fields to force the biogeochemical model. From what is said, it seems like the authors pick up in different models (e.g; Miyama et al 2003; Lee 2004)

Line 22: this is not clear how nitrogen fixation is modeled in this work.

Interactive comment on Biogeosciences Discuss., 9, 13581, 2012.