Biogeosciences Discuss., 9, C6721–C6724, 2012
www.biogeosciences-discuss.net/9/C6721/2012/

Biogeosciences Discussions

Interactive comment on "Coupled physical/biogeochemical modeling including O_2-dependent processes in the Eastern Boundary Upwelling Systems: application in the Benguela" *by* E. Gutknecht et al.

Anonymous Referee #1

Received and published: 21 December 2012

General Comments

This work describes the development and calibration efforts of a new biogeochemical model, focused on the O2 and N cycle in the OMZ of the Benguela Upwelling System. The development and testing of this biogeochemical model is an interesting and comprehensive work. The synthesis of processes and various parameterizations is well discussed and integrated in the model and sensitivity tests. An additional value of this work is the comparison with a broad set of in situ data, in particular related with the N cycle, although they are still sparse and rare.

The authors nicely show that physical terms (advection and mixing) are driving the oxygen content in the OMZ, and that biogeochemical processes are maintaining its level (Fig. 14). But since the physical conditions are so important, what about testing the boundary conditions, in particular the O2 concentrations introduced to your regional domain, and the physical parameters (mixing terms). In section 5.3, you mention that a more developed OMZ over the shelf would significantly improve your estimates of N2O production and outgassing. This could be easily tested with a sensitivity test about your boundary conditions, or even a test case of restoring the oxygen concentration only, and see whether the fluxes mentioned by Suntharalingam et al. are achievable with you model. A discussion about the relative importance between all your efforts about the biogeochemistry parameterization and the physical model parameterization (mixing coefficients) would be necessary. What is the sensitivity of all your calibrations relative to the physical settings ?

Overall, the development and calibration work of such a complex biogeochemical model is a lot of work that will be of interest for the modeling community. Nevertheless, I would encourage the authors to develop the concluding part about how to improve the future work and model development in regard to their own model caveats and advantages.

I would recommend publication of this work after a strong effort of revising these few comments and discussion points.

Specific comments

Section 3.5 and Figure 9 : The fig 9a and b do not seem to me appropriate to really compare model and data on the point to point basis. It would be worth commenting why the model produces much higher N2O concentrations (> 50 10e-3 mmol /m3) at low O2 concentrations (but still > 10 mmol O2 /m3) than observed (the rising trend along the y-axis). Is this realistic in other oceanic regions ? In situ data are rare, and it is therefore difficult to evaluate there significance. But can the model achieve reproducing data points like the two ones in lower left corner of Fig 9c, with very low O2 and N2O ?

section 4.2, line 16 It may be worth explaining that this solution may act on the fluxes and that if they keep balanced, it will not modify the concentrations. The question is: does the improvement about the fluxes also improve the concentrations of the various chemical species ?

section 5.1 p15085, lines 11-14 : please make clearer the volumes achieved in the model compared to the data. Is it really relevant to mention a factor of 65.9 when starting from almost nothing ? In Figure 13, most of the error bars for hypoxic water volumes reach very low values. Does this come from inter-annual variability or from a trend in the development of the OMZ along the 8-years of simulations considered ? In both cases, it is questioning. If this is a trend, I find it problematic for the validity of the interpretation of your sensitivity analysis. If it is inter-annual variability, this is hughe and would be worth commenting where such fluctuations are coming from.

In section 5.3, I am not sure whether the N2O production budgets over the studied area are relevant, since part of the shelf area (shoreward of 130m isobath) is excluded from budgets because of a lack of oxygen depletion. this could be discussed more precisely.

section 5.3 The authors could mention potential sources for parameter improvements, like derived proxies for existing communities (e.g. ladderanes).

Technical corrections

p15054, line 20: Please make clearer what kind of model you are referring to, and what you call bias.

p15054, lines 22-23: Any reference for these expected changes ?

p15056, lines 4-6. Over which period of time is this trend observed ?

p15061, line 18. In equation (18), f' is formally also dependent on NH4+: f'Pi(NO3-,NO2-,NH4+)

p15062, line 10. Please correct : "...phytoplankton is not limited..."

p15062, equation (19). Replace "avec" with "with".

p15070, lines 20-27. Please rephrase and indicate which symbols of the graph you are refering to.

p15070, line 29 and p15071 lines 1-2. It seems the terms "spring" and "autumn" are inversed as compared to the values reported in Table 3. Please check.

p15075, line 9. I suggest to rephrase as follow: "However, the amplitude between the extremes ... is lower..."

p15084, line 20. The reference (Hofmann et al., 2011) is not in the references

p15085, line 22. I guess you mean "to reduce NO3- and NO2- to NH4+" ?

p15086, line 2. replace "expect" with "except"

p15087, line 14. Do you mean Fig14f instead of 14e ? Indeed only the biogeochemistry consumption induces a oxygen sink.

p15089, line 13. As no data are shown in Fig12a, it is difficult to tell that there is improvement in the mean N2O concentration. Please rephrase the next sentence: replace "This improvement provides maximum N2O..." with "This increase provides improvement in maximum N2O..."

Caption of Table 6. Please add "(test 15)" after "reference simulation"

Caption of Fig. 14. Do you mean that fluxes are computed at the depth of minimum oxygen concentration ?

Interactive comment on Biogeosciences Discuss., 9, 15051, 2012.