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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 #2

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The paper by Gutknecht et al. describes a numerical physical and biogeochemical model of the Benguela Upwelling system and validates it based on available observation data. A companion paper (Gutknecht et al., 2011; which I do not know) apparently uses this model to elucidate the N-cycle in the oxygen-poor environment, with a specific focus on N2O production and various processes eliminating reactive N. This current paper appears to be the "methods section" of the predecessor paper, although here significant emphasis is put on modelling the N-cycle as well.

The paper is very technical and has no question apparent to me which it attempts to an-

swer. Half of the text is devoted to describing, tuning and sensitivity testing the model. Not being a modeller, I cannot comment on the correctness of the assumptions and formulations that have gone into the model. But I asked myself who would find this interesting: Is the description and tuning of a model suitable as the content of a scientific paper? Is this paper written for other modelers, and is the added work that the authors have invested into improving the previous versions of ROMS and BioEBUS significant? Can the paper be published as a technical note? I see from the BGD web page that this is a contribution to a special issue on "Low oxygen in marine environments from the Cretaceous to the present ocean: driving mechanisms, impact, recovery". It is difficult to see how this manuscript contributes to this: There are no independent insights to be had from the paper, except that "more studies are needed to better understand the N cycle and improve its representation in biogeochemical models".

As it is written now, it is very long, often awkward in its formulations ("In the OMZ off Namibia, N2O emissions to the atmosphere are comparable with N loss" – do you mean quantitatively?) or outright enigmatic ("Thus, other more classical variables are also necessary as nitrites") and generally a difficult read for a non-specialist.

My advice would be that the authors consider cutting short much of the model descriptions and tuning, and concentrate on 1 or 2 questions that they can address with the model in experimental mode and that observations are likely unable to answer: For example, what is the quantitative role of the OMZ in eliminating reactive N over a typical year? Is denitrification indeed balanced by nitrification if integrating over the entire system?

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