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Biogeosciences Discussions

Interactive comment on "Modeling dissolved oxygen dynamics and coastal hypoxia: a review" *by* M. A. Peña et al.

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We thank the reviewer for comments and for providing suggestions on how to improve our review manuscript. The main concern of this reviewer is that the paper is too long, involving a lot of subjects, repetitions and details that lack integration and analysis. Similar concerns were raised by reviewers #1 and #2. Basically, we agree with the reviewers that the paper will benefit from better reorganization to avoid overlap among sections and to improve readability. Thus, we have reorganized the text so each section is better focused to avoid repetitions, added figures and tables to support the main text, and carefully revised the paper to provide more integration and analysis of the topics.

More specifically:

Abstract: As it is now, the abstract lacks of concision. This is a succession of sentences dealing with different subjects (e.g. lack of 3D models, lack of data, model complexity). I would recommend summarizing the abstract in order to highlight what is the purpose of the present review and what are the most important conclusions about the review on modelling DO.

Response: We have modified the abstract in the revised version so it highlights the purpose of the review paper and the most important conclusions about the review on modeling oxygen dynamics and the case studies discussed.

Introduction (page 9197-9200): I found some repetitions for instance in the page 9198 lines 7-8 page 9198 and 22-23; page 9200

Response: The text has been revised to avoid these repetitions.

Figure 1: Could you please explain why you have a double arrow for primary production although you consider separately the respiration by phytoplankton? I would add in the legend that this scheme is for an oxic water column. Besides, this representation is an oversimplification because the degradation of detritus goes directly to ammonium instead of having detritus decomposed by the bacterial loop. So I would add that this is a representation of DO dynamics as represented by a NPZD model.

Response: We agree that Figure 1 is an oversimplification and depicts oxygen dynamics as represented by a NPZD model. Thus, based on the suggestion of this reviewer and reviewer #2 we have replaced this figure by a schematic representation of a fully coupled model.

Page 9200, section 2: The authors listed the processes that governed the dynamics of DO, they do not mention the impact of chemical processes such as the oxidation of ammonium and in general of reduced substances that may also occur in a water column. Afterwards, I understood that this is done in section 3. I suggest to merge these two sections as I mentioned below.

Response: We agree that section 2 and section 3.1 are closely related. We have merged Section 2.2 and section 3.1 in the revised manuscript.

Section 2.2, page 9202: 1) the consumption of oxygen during the degradation of detritus is due to bacteria respiration (this is included in your point “respiration of living organisms”). 2) In anoxic water column such as in the Black Sea you also have consumption of oxygen for the oxidation of reduced species such as dissolved iron, manganese, and hydrogen sulphide (you mentioned that point afterwards).

Response: As mentioned above, Section 2.2 has been merged with section 3.1 so now both of these processes consuming oxygen are discussed in the same section.

Page 9204, line 25: In fact, the respiration of marine organisms is the sum of a basal term which is a function of the biomass and an activity term which is a proportion of the growth. Besides, when you have a nitrogen based model, respiration is represented based on the excretion of ammonium using an appropriate C:N ratio.

Response: We agree with the reviewer that: 1) respiration of marine organisms is the sum of a basal term and an activity term and 2) in nitrogen-based models, respiration is represented as the excretion of ammonium. We have modified the text to clarify these points and mention that in most models only one respiration term is usually considered.

Page 9209, line 15 I suggest replacing “by vertical diffusion” by “by vertical processes which are strongly reduced due to the presence of stratification”. I suggest summarizing the paragraph between lines 13-25.

Response: We have replaced and shortened the paragraph as suggested by the reviewer.

Page 9210: The part on the description of the Richardson number is for me not relevant to the subject of the paper. In this section, once more the authors repeat that the presence of strong stratification may lead to hypoxia.

Response: We disagree with the comment that the Richardson number is not relevant

to the subject of the paper but we realize that too much details are given, so we have shortened its description to make it consistent with the rest of the section.

Section 3.1, Pages 9212-9213, the authors give the complete set of chemical equations governing the degradation and chemical reactions in oxic, suboxic and anoxic environments. I do not think that this is necessary because they are not used afterwards.

Response: We agree that too much details are given in this section and have removed most of the chemical equations in the revised manuscript.

Page 9214, line 7, the authors say:” The models correctly reproduced observed biogeochemical cycles when configured with appropriate initial and boundary conditions”. Which type of models? If a model is dependent on the forcing used this is not good, this is better to have a model that is able to reproduce observations due to its representation of the dynamics of the system and not due to the data you used to force it.

Response: We agree with the reviewer comment that “it is better to have a model that is able to reproduce observations due to its representation of the dynamics of the system and not due to the data you used to force it”. However, in this sentence we referred to the model dependency on initial and boundary conditions but not the forcing used.

Page 9214, line 8-12, what do you mean by “Organic matter formed during phytoplankton bloom periods determine water column biochemical structure” you mean the oxygen profiles? If yes, you do not mention the production by photosynthesis.

Response: Yes, we mean oxygen and nitrogen profiles. We have modified the text to mention oxygen production by photosynthesis.

Page 9214, lines 14 to the end of the section: the authors describe with a lot of details the characteristics of the Black Sea chemical structure (e.g. conc. of NH4, S_, H2S). This is a particular case and this is not a general rule applied to the other environment

deficient in oxygen. I found this part irrelevant because there are no conclusions about the ability of formulations of certain processes to represent the reality. This is just a description. I suggest that this section is included in section 2.

Response: We agree with the reviewer that this is a particular case and have moved this section to the Baltic Sea case study subsection.

Page 9215, section 3.2: Lines 14-17, I do not understand why primary production may be stimulated when increased sediment denitrification causes NO3 to be removed as NH4. You mean water column primary production?

Response: Yes, we mean water column primary production. This point has been clarified in the text.

Section 3.2.1, page 9216, the authors speak about the problem of phosphorus remobilization. We understand that the presence of hypoxic conditions may enhance the release of phosphorus from the sediments and hence delay the recovery from hypoxia but I do not think that all the details given to describe the P remobilization. We are away from the main aim of the paper which is to analyse models that assess DO dynamics. The same apply to the next section.

Response: This section has been moved to the section dealing with "Modeling the effect of hypoxia on biogeochemical cycles and ecosystems". Since P remobilization is an important response to hypoxia, we think that it is important to retain this section.

Section 4: a lot of repetitions with the sections before: impact of eutrophication, impact of stratification,

Response: The intent of this section is to focus on models applied to the case studies presented. The text has been revised to avoid repetition by moving some of the material presented in the previous sections to the particular case.

Section 4, 4.2, lines 16-25, this part is not particularly relevant for the Black Sea. It may apply for any model.

Response: We agree with the reviewer and have removed the text from this section.

Interactive comment on Biogeosciences Discuss., 6, 9195, 2009.