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Biogeosciences Discussions

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

Anonymous Referee #3

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The manuscript presents a review of mathematical models dealing with the representation of the dynamics of DO and hypoxia. Models developed for the water column, the sediments and both systems are described. The manuscript also describes the impact of oxygen depletion on nitrogen and phosphorus cycling in the sediment and on the ecosystem components. It also deals with the impact of climate change on hypoxic environment. The authors also describe for 3 particular oxygen deficient environments the model applied and the main conclusions derived from the simulations. The paper mentioned a lot of literature references. Since I am not expert of oxygen study, I can not judge whether the list of references is exhaustive and up to date. The amount of work done is impressive and the work is certainly valuable. However, I found the paper much too long involving a lot subjects, repetitions and details. The manuscript is mainly a succession of features derived from the literature with a lack of integration and

analysis. Additional appropriate figures or tables may be helpful.

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. Besides, the manuscript does not deal only with modelling issues but also on understanding the impact of hypoxia on ecosystems, sediments nitrogen and phosphorus cycling in a descriptive way. A particular focus is put on three case study in section 4 the northern gulf of Mexico, the Black Sea and the Baltic sea.

Introduction (page 9197-9200): the authors enumerate the potential causes and consequences of hypoxia, listed the different types of environment where hypoxia can be found and then for some environments they listed some models that have been applied. I found some repetitions for instance in the page 9198 lines 7-8 page 9198 and 22-23; page 9200

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.

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.

Section 2.2, page 9202: 1) the consumption of oxygen during the degradation of de-

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tritus 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).

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.

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. The authors cite conclusions of a very specific study. I would say something like this “ In areas with reduced horizontal transport (e.g.), the intensity of ventilation is strongly conditioned by the strength of vertical stratification. For instance, interannual variability of the intensity of ventilation can be found in connexion with variability of the vertical stratification as found by Bendtsen et al., (2009) using . . .

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.

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. IF for a given reason, the authors want to show these equations I would suggest to put them in a table for clarity. All these equations are not related to DO dynamics.

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,

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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.

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.

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.

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?

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.

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

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

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