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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 a constructive evaluation and for providing suggestions on how to improve our review manuscript. The main objection of this reviewer is that a lot of material is covered, resulting in a long and hard-to-read paper. The reviewer suggests removing the sections on modeling oxygen dynamics, in order to attain a shorter and more focused review. We agree that the paper is long and that some sections contain too much detail but rather than completely removing the broader topic of modeling oxygen dynamics, as suggested by the reviewer, we have revised these sections so they are better focused. We also agree on the need to include more figures. We have added the following figures to the paper: i) a map showing hypoxic areas where model studies have been carried out, ii) a schematic representation of a fully

coupled model, iii) a schematic figure illustrating major pelagic and benthic processes, and iv) one figure illustrating each of the case studies discussed in Section 4 (i.e. Northern Gulf of Mexico, Black Sea, Baltic Sea and Oxygen Minimum Zones). We hope that by reorganizing the manuscript, significantly shortening the sections dealing with the broader topic of modeling oxygen dynamics, and adding relevant figures, we have been able to address the reviewer's concerns.

Major comments:

Although common practice, I have always found it strange to equip oxygen with the superfluous term "dissolved" as an epitheton ornans. In my opinion, there is no confusion possible (at least, I never think of gaseous or solid oxygen when it comes down to seawater chemistry).

Response: Agreed. We have removed the term "dissolved" when referring to oxygen.

The introduction on p9197 mentions "natural and human-induced drivers of the oxygen balance. Improving our understanding of these drivers is of great importance. . ." But what about internal feedback mechanisms (e.g. legacy storage of organic carbon in the sediment, see Turner et al (2007) that can influence the oxygen dynamics of hypoxia?

Response: We recognize the importance of internal feedback mechanisms and have modified the introduction text accordingly and have added the proper reference to Turner et al.

Section 2 on modeling oxygen dynamics is actually an abridged version of a biogeochemical textbook. Overall, this section contains too much detail to be a good review, and not enough detail to be a good textbook. Section 2.1 on air-sea exchange discusses one particular possible parameterization of the piston velocity, but there are many other possible parameterizations. Section 2.2 provides a general discussion of model formulations of primary production, water column mineralization, and nitrification in the water column. These are standard formulations in biogeochemical models,

and the added value for this review is not clear to me. Section 2.3 essentially provides a two page introduction to early diagenetic models of sediments, with a specific focus on the Sediment Oxygen Demand. Again the space allocated is too short to have a proper description of early diagenesis, and one can ask, why the sole equation (9) is mentioned, and not five other equations that are equally pertinent to early diagenesis. There are a couple of interesting sentences that deal with how the SOD varies with the oxygen concentration in the bottom water.

Response: We have extensively revised and shortened Section 2 on modeling oxygen dynamics to focus on "Major physical and biogeochemical processes". In Section 2.1 we now briefly discuss other possible parameterizations of the piston velocity. Sections 2.2 and 2.3 have been revised to provide a general overview of processes determining oxygen dynamics in the water column and sediments. Section 2.2 now includes a discussion of biogeochemical processes originally included in subsection 3.1.

Section 3.1. This is again long and has a textbook character. Fig 3: Why discuss the output of one particular model in detail? Section 3.2 This is a very concise, integrative and interesting section. Section 3.2.3 very relevant section!

Response: We have shortened Section 3.1 and moved its contents to Section 2.1 to avoid repetition, following the recommendations of the other reviewers. Section 3.2.3 has been expanded and made into a separate subsection.

Section 4. This section sequentially discusses three basins (Gulf of Mexico, Black Sea, Baltic). This provides a good overview of the modelling work on these basins.

Response: We have added a discussion on Oxygen Minimum Zones (OMZ) to this section as suggested by reviewer #2.

Section 5. Generally interesting section, especially section 5.3.

Response: We have reorganized this section as suggested by referee #2.

Section 6. This again discusses the broader problem of O2 in the global ocean.

Response: Most of the text of this section has been moved to the section discussing the OMZ case study.

Section 7. This section contains quite some "general truths". I would prefer a concide bullet list with more focal points of attention. (see e.g. Doney et al (2009) review paper on acidification in Ann Rev Mar Sci)

Response: This section has been modified to include a bullet list of focal points of attention and a table listing specific research areas requiring attention to produce more robust models.

In addition, we have taken into account all the remaining points listed in the detailed comments. In particular we have modified/clarified the text where pointed out by the reviewer, checked for consistency of units through the paper and added reference to the model of Billen and coworkers.

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