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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 #1

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This is an exhaustive review of all aspects concerning the (mechanistic) biogeochemical modelling of both natural and antropogenically induced hypoxia. Coastal hypoxia is an issue of growing concern due to the increased nutrient supply to the coastal zone combined with the effects of climate change (decreased solubility of O2, enhanced stratification).

The manuscript is generally well written, prepared with great care and is nearly typo-free (the authors are to be commended for this). Undoubtedly, this review will be a great resource for future model developers, not the least for the impressive reference list that is compiled and discussed. Overall, the authors deserve ample credit for taking on this ambitious effort, but still, I have some important remarks and suggestions on how this review manuscript can be improved.

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Interactive Discussion
Discussion Paper

(1) More focus and a more stringent selection of topics. A lot of material is covered, which results in a long manuscript, which demands a great time investment of the reader (it has taken me a couple of 1 hour train rides to work trough the complete manuscript). In my view not all of the material incorporated is equally essential. Like the title says, two topics are covered: modeling oxygen dynamics and modelling coastal hypoxia. The topic of modelling coastal hypoxia is well covered, and this is clearly the homeground of the authors. However, the sections on the broader topic of modeling oxygen dynamics are longwinding and have little relevance in the overall story. I would suggest to remove the latter part, in order to attain a shorter and more focused review.

(2) The figures are scarce and do not support the main text. In fact, they appear haphazardly chosen. In a review, I would expect more schematic drawings that illustrate concepts, or at least, carefully chosen illustrations. Some of the longwinding sections in the manuscript could be easily replaced by a schematic drawing (e.g.approaches to modelling O2 dynamics).

(3) The review is at points very descriptive in the sense of "those authors have done this" and "other authors have done that". I would prefer a more synthesizing and integrative style. Some parts already adopt such a more integrative style (e.g. section 3.2)

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).

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?

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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.

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!

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

Section 5. Generally interesting section, especially section 5.3.

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

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)

Detailed comments

P9198 L8 “biogeochemical properties” this is a very vague term

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P9198 L8 “climatic gases” -> greenhouse gases

P9199 L17 bad sentence

P9200 L2 “it is difficult to use the same model everywhere” Models are simplified representations of reality designed to answer a certain question.

P9200 L28 These are balance equations, not conservation equations (O2 is not an conservative property, but one can write a balsnce equations for O2; in contrast, mass conservation implies that elemental O is conserved in chemical reactions)

P9202 L1 K is the piston velocity (usually denoted k_d)

P9202 L16. Chemolithotrophic bacteria.

P9205 L17. tolerance -> functional response

Section 2.3. Implicit in this section is a one-dimensional model approach to sediment diagenesis

P9207 L1 dependencies

P9209 L5 “mass balance models” What is meant by this term: reactive transport models?

P209 Eq (10). A generalized form of this equation governs O2 in every circumstances (both water column and sediment), and so it is strange that this appears so late in the section.

P9215 L16 increased sediment denitrification etc. -> not correct

P9215 L26 “Most coupled benthic-pelagic models. . .” Not true: Luff and Molll (2004) are an exception rather than a rule

P9218 L16 oxidation of ammonium -> nitrification

P9222 L2 “models of intermediate complexity” -> what does this buzz term mean?

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Interactive Comment

P9222 L22 total phosphorus -> I thought nitrogen loading is crucial to Gulf of Mexico

P9224 L21 add the RIVERSTRAHLER model by Billen and coworkers

P9232 L2-3 “the extension of anoxic bottom waters. . .average anoxic area” please clarify what is meant by this

P9234 “ml O2 L-1“ old fashioned unit. Consistency of units throughut paper

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