

Interactive comment on “Bottom RedOx Model (BROM, v.1.0): a coupled benthic-pelagic model for simulation of seasonal anoxia and its impact” by E. V. Yakushev et al.

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The authors present a rather complex model allowing simulation of biogeochemical processes in coastal systems subject to seasonal anoxia. The paper has a few strengths and many weaknesses. Most numerical biogeochemical models focus either on the water column or on the sediments and very few couple these domains. The presented BROM model does explicitly deal with the coupled system and is therefore of value.

Strengths of this paper include the (a) coupling of pelagic and benthic modules, (b) the explicit resolution of the benthic boundary layer (BBL), (c) the focus on seasonal hypoxia and (d) its linkage to/integration with the Framework for Aquatic Biogeochemi-

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cal Modeling. These characteristics make this an interesting paper and the presented model is potentially useful.

However, there are many issues to be resolved before publication of this paper and model. 1. The paper is poorly written in terms of organization, flow and use of English. A few examples of the latter (line 1: seawater and benthic sediments, benthic systems or sediments are fine, but not benthic sediments; the use of the term protolithic processes: do you refer to stone age processes? or do you simply mean equilibrium processes, etc. etc.). There appear native speakers and/or UK/Canada based scientists among the authors: perhaps they should have another look at it. The text is also not prepared with utmost care: many typos, wrong equations etc. (see below in the list of detailed comments).

2. The model could be much better put in context and existing literature is poorly incorporated. I was missing references to other papers dealing with coupled benthic-pelagic models, the more simpler ones (Lancelot, Soetaert, Fennel, Reed and co-workers) and the highly complex ones from the ERSEM family. Soetaert and co-workers (Soetaert and Middelburg, 2009; Meire et al., 2013) have published on seasonal oxygen issues with coupled pelagic benthic models. There is also a large body of knowledge on the effect of oxygen in early diagenetic models; that literature is not covered. Extensive work on the role of sediments as moderating the timing of return fluxes (delay in return of N, P, Si after bloom) and the memory provided by stored reduced sulfur, iron and carbon in sediments is poorly covered (see work on Gulf of Mexico by Nancy Rabalais and co-workers).

3. The model description is often incomplete and imprecise (see below for a far from complete list). The documentation is not sufficient. Boundary conditions of the model are not clearly described. Details about the coupling of the models are insufficient: e.g. the grid size is very likely changing, yet not provided. It is unclear whether particulate organic matter is modeled explicitly. Is it transported by bioturbation in the sediment. It is unclear how bio-irrigation and solid-phase mixing are treated. Sometimes parame-

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ters are introduced in the text, but appear as fixed value (hard-coded) in the tables.

4. The model is very complex and detailed (perhaps too much) in some aspects and very rudimentary in other aspects. Regarding the latter, many detailed Mn, Fe, S transformations are included, chemoautotrophy is resolved for aerobic and anaerobic microbes (or only bacteria?), but important processes such as methane generation and anaerobic methane oxidation coupled to sulfate reduction are ignored. Another example particle settling velocities is corrected for the formation of Mn-oxides in the water column but other carrier phases such as calcium carbonate are not resolved. Clearly, the presented model is a version 1.0 and represents a first step, but the priorities of the authors do not match those of the majority of the audience. At the minimum some motivation for their particular choice should be communicated to the audience.

5. Section 3.1 on model output discussion needs major revision. The link with the figures is unclear and the organization is suboptimal. You discuss the oxygenated winter period and then link to later periods or a few days later in the section on oxygenated winter period. There is no story. Try to limit yourself to a few findings and discuss those. The reader now has to digest all the computer output her or himself.

6. Section 3.2 is not useful or convincing. The link with data is very poor. This is indeed a difficult job, but here serious work has been done. A comparison with just three to four papers is made and the extensive databases on oxygen uptake, oxygen penetration depth etc are not consulted. A statement like line 10 on page 20: “further observations under anoxic and suboxic conditions are rare as field and experimental studies generally focus on oxic conditions” is close to unacceptable. Consider all the work done on the Eastern Pacific ocean margins (Washington and Oregon shelves, California basins, Mexico/Peru/Chile margin), Indian ocean and shelves and multiple European systems (Black Sea, Baltic Seas). There are nice seasonal time series in coastal systems with low-oxygen events during summer (Kiel bight, etc.)

Minor issues:

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Page 3: - Line 2: benthic sediments? - Line 3: directly affect and are impacted by.. - Line 7: fuzzy writing, rewrite

Page 4: - Line 3: enrichment with OM or do you mean deposition of labile organic matter - Line 26-27: animals can die, migrate or change their behavior: revise text accordingly

Page 5: - Line 4-15: additional literature incorporation required. How does your model differ from those. Where are the improvements etc. - Line 22: the benthic boundary layer is a major strength. Introduce it better. Delineate the features, etc. - Line 24: at the BBL: do you exclude the sediments here?

Page 6: - line 9-10: it is unclear whether organic C is also modeled or is it just inorganic C. - Line 12: No nitrogen transformations?

Page 7: - Line 2: delete consists - Line 26 and all through: replace protolithic processes with equilibrium processes/reactions or acid-base reactions

Page 8: - Line 11: provide the number of state variables to the reader

Page 9: - Line 1: chemoautotrophy is resolved, but overall secondary production is ignored. There may be good reasons for this, but communicate this then to the reader - Line 8-10: why is methanogenesis excluded? This is probably related to the way you model organic matter. Conceptually most simple is to turn all labile organic matter remaining after depletion of all oxidants into methane and carbon dioxide. - Line 15-25: the alkalinity equations as given are wrong: the phosphate alkalinity term should include a H_3PO_4 term, the ammonium alkalinity term should not include NH_4^+ , etc. Please check whether you have implemented it correctly into your model. Page 10: -Line 2: I guess you mean Atom was set to zero and not TOM.

Page 11: -Line 1-10: quite a number of the reactions are not balanced and inconsistent with Table 2: e.g. denitrification with hydrogen sulfide and the line above represent two reactions of which the latter misses a two before OH^- . Check carefully. -Line 11: Table

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2 not 3.

Page 12: middle of page: a distinction is made between settling velocity of particulate matter and of particles with Fe and Mn oxides. Why not write particle settling velocities (w) as a sum of various contributing terms. Why the focus on Fe and Mn? Just a Black Sea model heritage?

Page 13: - Line 8: the eddy diffusion coefficient was assumed constant in the BBL. As a first coupled model that resolves the BBL it may be done like this, but given the lognormal velocity profiles, would one not expect a depth profile in K_z as well. This can be incorporated quite easily. - Line 15-20: the description of bioturbation/bioirrigation is difficult to follow. As written above are solid phase and solutes transported separately or together? This is unclear. Bioturbation depth are very shallow for fully oxic conditions.

Section 2.3. The description of boundary conditions needs more attention. It appears that you use flux boundary conditions for the gases and constant or fixed (at least imposed) boundary for the others. This may lead to mass balance issues. The boundary conditions at the bottom (depth 12 cm in sediments) are not described: no flux or no gradient or fixed concentration? The assumed sulfate concentration is either close to zero or do you mean 25 10-3M? Basically all external sources such as atmosphere and rivers are added to surface layer.

Section 3: I stop making detailed feedback because there are too many issues and the referee already spent double the amount of time normally needed for an evaluation.

p. 26, line 5-9: chemoautotrophy indeed involves CO_2 consumption and thus has the potential to increase pH. However, the energy required for CO_2 fixation is obtained from oxidation of reduced products: usually an acid producing process. With typical growth efficiencies one would produce more acid linked for the energy than consumption of acid by organic matter production. Cable-bacteria spatially disconnect half reactions and can therefore cause a real pH increase. Without detailed model investigations, I

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suggest removing these sentences. The authors might be right because of the complexity of reactions and the many buffering reactions, but it is not convincing as presented here.

Table 1: it is stated that oxygen is presented in $\mu\text{M O}$, but sometimes it might be, at other places it is definitely in $\mu\text{M O}_2$.

Table 2: - Aerobic respiration and denitrification are treated different than Fe, Mn and sulfate reduction regarding DON and PON separation. - For Mn reduction where does the 0.5 come from (half saturation constant hard-coded?) - There are multiple typos which complicate checking. - Where is the $(1+ftD(t))$ term coming from. ftD is not defined. - Page 41: I guess that NO_3 dependence should depend on nitrate and not on ammonia?

Table 3: - I guess that $K_{\text{Mn_rds}}$ should be $K_{\text{Mnrd_HS}}$? - There are many values assumed, some literature citations would be helpful. I guess that the model is rather insensitive to most of these parameters and their value should therefore be based on literature values. - Why did you choose 2.7 for the Fe/P ratio and not the conventional 10?

Table 4: check carefully: e.g. for phosphate you have hard-coded 2.7 for Fe/P and 0.67 for Mn/P rather than a parameter. Taking stoichiometry as a constant is fine, but do not present

Table 5: this table is useless and I doubt whether the fluxes are all in the right units.

Table 6: could be deleted.

Figures. All captions need more documentation. For instance it is not even mentioned why some concentrations are presented on two different scales. As written above, reconsider to focus on a few results and elaborate the model results in another paper. The figures as presented now appear more like raw model output.

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