Biogeosciences Discuss.,
doi:10.5194/bg-2016-482-RC1, 2016

Biogeosciences Discussions

Interactive comment on "The Roles of Resuspension, Diffusion and Biogeochemical Processes on Oxygen Dynamics Offshore of the Rhone River, France: A Numerical Modeling Study" *by* Julia M. Moriarty et al.

Anonymous Referee #1

Received and published: 20 December 2016

General comments

This manuscript presents the development of the HydroBioSed module within the ROMS-CSTMS framework. This module couples existing hydrodynamic, sediment transport, and water-column biogeochemical modules with a seabed biogeochemistry module, with the aim of investigating the role of resuspension on biogeochemical dynamics, most notably oxygen. This novel module is then applied to the Rhone Delta, showing that local resuspension has an important impact on oxygen dynamics on at least seasonal timescales. The paper also classifies systems where the impact of in-

termittent local resuspension on oxygen dynamics may be substantial.

I believe that this work is a necessary addition to benthic-pelagic coupling of reactive transport models, and an advancement in the field. The manuscript is well written and clearly organized, actually even too organized to my liking, by announcing even every subsection in detail. I do however believe that sometimes a little more detail might be nice in the method section, such that the reader unfamiliar with ROMS and/or CSTMS can also well understand the model development. Specifically, I would add the supplement to the main paper, as it contains details that are necessary to fully understand the method development, which is a major aim of this paper. Moreover, I only became aware of my biggest concern with the paper after having read the supplement. This major concern is related to the way the model deals with the lability of resuspended organic matter. I do not believe that it is valid to reclassify refractory resuspended organic matter into the labile pools, and believe it is the main reason why the re-deposited organic matter is enriched in labile pools (p.10, l.31-32), which I consider an unexpected and also unrealistic result that may highly influence the estimated seabed oxygen consumption. I will explain this concern in more detail below, but I believe this issue needs to be resolved before the manuscript can be published. Aside from that, and a few other comments related to the methodology, my comments are mainly technical and deal with how results are presented.

Specific comments

- p.4, l.9-10: is there a way that these resuspension events can be predicted, e.g. from tides or wind speeds? I am asking this in the context of potential model applications.

- p.4, l.21-22: What is the estimated seabed oxygen consumption at the study site itself?

- p.5, l.15: when looking at Table 1, it becomes evident that this equation calculates an amount of sediment in areal units, i.e. in kg/m2. Do I have to see this as a depth-integrated concentration from the active transport layer (so a concentration multiplied by za), or rather as sort of a flux across the sediment-water interface? What does the erosion rate parameter M represent exactly?

- p.5, l.18: In terms of mass, how much of POM is added relative to the number of inert particulates? I am asking this to see if adding POM as an additional particle source can lead to inconsistencies in e.g. estimated deposition rate compared to previous applications. Keeping this in mind, wouldn't it make more sense conceptually if the inert particulates pool would be split into a POM pool and an inert pool?

- p.5, l.22: The information of S.1 needs to be added to this section, especially since it is so important for the conceptual model. My main problem with the conceptual model is that the repartitioning of refractory resuspended organic matter into the labile pools does not seem a valid approach. I understand that resuspension may change the degree of aggregation, but I do not see why it would change the lability per se. The arguments given on p.2, l.5-8 of the supplement may be true, but can be implemented into the model in a conceptually better way. Faster OM remineralization under oxic conditions is already implemented in the model via the limitation factors and half-saturation constants. Higher remineralization rates in the water column can also be achieved by using different rate constants in the water column compared to the sediment, but in reality, I think this is due to the constant supply of fresh OM to the water column due to in-situ production and/or external loading. The former is already part of the model, whereas the latter might be implemented by an additional source term to the water column.

- p.6, l.1: to this section, the information of S.2 should be added. I was really missing this while reading this section, it is too important for the model description to be part of the supplement.

- p.6, l.17-21: I believe that these equations are currently incorrect, when following the units of Table 1. The right-hand side of Eqs. 2-6, is in mmol/m3/d, whereas the left-hand side is in mmol/m2/d. I would anyway stick with mmol/m3 and mmol/m3/d

everywhere throughout the manuscript, even though the model stores concentrations in a depth-integrated way, as this is the conventional way to present rates and concentrations.

- p.7, l.1-2: My first thought when reading this was: "shouldn't resuspension and redeposition have a huge impact on porosity in the surficial centimetre of the seabed"? As far as I know, porosity can fluctuate a lot even in the top centimetre depending on the amount of deposition and/or resuspension. I wonder I the authors have tested the effect of a different porosity, even if kept constant with depth; perhaps this should be part of the sensitivity analysis.

- p.7, l.8-12: I might have missed this, but are sediment deposition and erosion mutually exclusive? I understand that erosion only occurs in case of high bed stress, but does this mean that there is no deposition at all during erosion, or that both processes cooccur but that erosion dominates?

- p.7, l.30: What are the units of the 5x6 model grid?

- p.9, l.4-6: It would be good to stress here that the bottom boundary layer is the layer below the pycnocline, as mentioned on p.7, l.32. Otherwise, this definition lacks a bit of context (i.e. why was 4 m chosen as thickness of the BBL?)

- p.9, l.7-9: Why are the rates of biogeochemical processes in the BBL not added to the code as output parameters?

- p.9, l.28: I It seems that, since zSWI and O2,OPD are by definition 0, O2,SWI is positive, and zOPD is negative (at least according to Figs. 3 and 5), Eq.8 would by default result in a negative value, which is not the case. So this needs to be changed to make it consistent.

- p.10, l.5-7: Have the authors considered implementing a loss term of the resuspended material, as a possible workaround to the uniform conditions in the horizontal?

- p.10, l.26-27: How were the erosion depths quantified? Is erosion depth more dependent on the duration of the event, or on the bed stress?

- p.10, l.31-32: This is a result I do not understand, and also do not believe to be an accurate representation of reality. I believe this is due to the parameterisation of the lability of the resuspended material, which I discussed above.

- p.11, l.22-27: I'm not sure if I understand this argument. Isn't this faster oxygen consumption during re-deposition driven by the newly-deposited OM being more labile, rather than additional oxygen availability?

- p.11, l.26: Wouldn't 'quasi-steady state' be a better term there than 'equilibrium'?

- p.12, l.1-2: Why would entrainment of nitrate increase the nitrification rate? Eq.5 does not show any NO3-limitation.

- p.12, l.10-12: This sentence seems to lack something. I was expecting to read "..compared to xxx" at the end.

- p.12, l.22-25: Fluxes of NH4 and ODU are presented here, it would be nice to also present them in a figure or table (similar to Table 6).

- p.14, l.14: all 3 resuspension events presented here have a duration of days (grey-shaded areas of Fig 4). I don't see any events with a duration of hours. Were the shorter events not captured by the model, or did they not take place during this two-month period?

- p.15, l.15: The Almroth-Rosell et al. (2011) paper shows most resuspension at other locations in the Baltic Sea, though it is high in certain parts of the Gulf of Finland. Perhaps the authors can elaborate a bit on this in their discussion.

- p.15, l.28-29: I am perfectly fine with keeping temperature constant in the model, but I do think that the impact of a higher temperature should be tested by increasing rate constants according to the Q10 rule. That would be a very valuable sensitivity analysis.

- p.16, l.3-5: Wouldn't OM loading also be lower in winter and thereby impacting oxygen

consumption?

- p.16, l.13-16: What is the spatial scale of these other models as compared to this model? Could differences therein be an argument for their different handling of oxygen consumption due to resuspension?

- p.30, Table 3: How has this sedimentation rate been estimated? If e.g. by 210Pb dating, wouldn't it be more correct to say that this is the total sedimentation rate, that then needs to be partitioned into an inorganic and an organic part? (see also comment on p.5, l.18)

- p.31, Table 3: What does the fraction of ODUs that are solid and inert represent? Since denitrification is modelled separately, does N2 also belong to the ODU pool here? If not, this number may be quite high.

- p.33, Figure 2: I do not find this figure very clear. What I would be interested to know is how suspended sediment, organic aggregates and organic particulates from the water column biogeochemical module are interlinked. I.e. a schematic of supplement S.1, showing that e.g. labile water-column organic matter is the sum of phytoplankton, detritus and labile aggregates, and that this is part of the total suspended matter.

Technical corrections

- A general comment: I get the impression that the terms 'resuspension event' and 'erosional period' seem to be used interchangeably (e.g. in the caption of Fig. 5), is this correct?

- p.3, l.24-26: I would leave out the "other factors" from the research question, it sounds too vague and results of other factors are not presented (only mentioned to be negligible in the case of ODU oxidation rate and the diffusion scheme)

- p.3, l.28: Section 1.1 fits better as the first section of the Methods.

- p.3, l.32: 'located at ~25m water depth'

- p.3, l.33: I would prefer ‘model validation’ over ‘model evaluation’ here

- p.8, l.28: the ODU oxidation rate sensitivity test is mentioned here as not being presented further, but it is part of Table 4. It should be removed from the table.

- p.9, l.10: ‘it’s’ should be ‘its’

- p.9, l.29: Table 5 does not show this; remove the reference here.

- p.12, l.21: The contribution of ODUs is virtually 0, as Table 6 shows, and negligible compared to the other processes, so remove it here from the text.

- p.16, l.10: change ‘produce’ to ‘reproduce’

- p.32, Table 6: The total oxygen consumption is higher than the sum of the separate processes. Maybe this is because minimum and maximum values are presented, but also for the average, this is not the case.

- p.14, l.30: “reduced species for oxidation”. This is not shown by the model, as the contribution of ODUs to oxygen consumption in the BBL is really minor. Remove this statement.

- p.15, l.25-26: Too many references here.

- p.16, l.19-23: More appropriate than what?

- p.17, l.12-17: This section might better fit in Section 4.1.2, when discussing temporal variations

- p.17, l.32-33: Something seems to be missing in this sentence.

- p.28/29, Table 1: carefully check the units, especially in relation to Eq. 2-5.

- p.32, Table 6: Add vertical lines between the three different categories as specified on the upper row. Also, why aren’t averages presented of the depositional (white parts of Figs. 4 and 6) and erosional (grey parts of Figs. 4 and 6) periods presented, rather than only an average of the total two-month period? That seems clearer and more

meaningful to me. Finally, clearly specify whether the maximum and (especially) minimum values presented are from the whole two-month period or from the erosional parts only.

- p.34, Figure 3: Define SSC in the figure caption. Add the dates to the oxygen microprofiles.

- p.35, Figure 4: Add a horizontal line to panel a showing the critical shear stress. Panel b could use a differently scaled y-axis, to make temporal variations clearer.

- p.36, Figure 5: Why are different times chosen here than in Figure 3? For clarity and consistency, it would be nicer to study the same erosional event in both figures. The depth scale of the O2 profile may be changed for clarity. Also for clarity, the captions ‘pre-resuspension’, ‘during resuspension’ and ‘post-resuspension’ (or something equivalent) could be added to the three columns.

- p.37, Figure 6: I don’t see any oxygen sources in panel b, only sinks. So I would change the caption here. Also, I would replace ‘fluxes’ in the caption with ‘sources and sinks’.

- p.38, Figures 7 and 8: These plots would be clearer if the black and purple parts were presented on different y-axis. Currently, it is quite difficult to observe differences between different scenarios. Also, in the caption of Figure 7, ‘it’s’ should be replaced with ‘its’.

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