

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 #2

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This is a very interesting paper on the effect of physical-induced sediment resuspension on the oxygen dynamics in the sediment and overlying water column in a shallow mediterranean area. As far as I am aware, this is the first time that such sediment dynamics are included in this detail into a mechanistic model. As clearly shown in this paper, such resuspension events may significantly alter sediment and bottom layer oxygen dynamics. Moreover, the correspondence of model and data suggest that resuspension in the area is very well represented by this model. The manuscript is very well written, and results are clearly explained.

C1

I tend to have somewhat different views on why the model displays what it does, which might be considered. Also I would suggest to slightly rewrite the model equations and setup.

Model results

The sequence of events that are invoked to explain the differences in oxygen budget are strongly focused on the physics. The authors write that resuspension increases the vertical gradient of oxygen in the sediment, which in turn increases the diffusive flux, increasing oxygen consumption through nitrification. A biogeochemical view would be that resuspension brings ammonium from deep layers more towards the surface, in close contact with oxygen. This would increase nitrification and increase oxygen consumption, resulting in stronger vertical gradients and a higher flux. Probably the truth is in between both ?

One of the sediment characteristics that has a significant effect on resuspension-induced O₂ dynamics in the sediment, is the abundance of labile organic material. This is quite surprising, considering that the increase in O₂ consumption after resuspension is largely due to nitrification. Based on the nitrification effect, I would guess that high ammonium concentrations at depth would increase the effect of resuspension of O₂ dynamics. And deep concentrations of ammonium are usually linked to deposition of refractory OM rather than of reactive organic matter. I do not understand what is causing this effect of labile OM.

I guess that the 20% increase in oxygen consumption of the seabed in case of resuspension results from the fact that the initial state of the diagenetic model has been estimated in the absence of resuspension events (although I am not sure this is how the model was initialized). Perhaps, for sediments where these resuspension events occur regularly, a better initial profile would be generated as a dynamic equilibrium that is established including these resuspension events? In this case, the average oxygen consumption would not be higher, but the variability would be increased due to

C2

resuspension.

Model description:

Several ways to describe the benthic model are quite confusing, not standard and sometimes inconsistent.

The formula for Eised is confusing as Eised is described differently in equation (1) (page 5 line 13-14) compared to its description in table 1. It is not standard as it features the Δt (timestep). Timesteps only determine how the model is solved, and should not feature in model equations. I suggest to give the equation for the erosion *rate* instead of the eroded mass, as is usual in sediment modelling studies. This removes the Δt in the equation, and changes the units of Eised.

A related question concerns the sedimentation rate. Does this only apply when there is no erosion?

In diagenetic models, the units of dissolved substances are typically expressed in mmol / m^3 *liquid*, and porosity features in the diagenetic equation because diffusion takes place in the porewater, while the mass balance needs to be written for bulk sediment. In this model, the units of O_2 , and NO_3 , (and of the parameters k_{O_2} and k_{NO_3} , etc. . .) are said to be mmol / m^2 . But what is this: mmol / m^2 bulk sediment or mmol / m^2 liquid? (my guess is that it is per m^2 bulk, but I am not sure). Moreover, the units are not always consistent. For instance, the $d\text{O}_2/dz$ is said to be in units of $\text{mmol} \text{O}_2 / \text{m}^4$ which suggests that O_2 is in mmol / m^3 . Finally, the units of the monod constants are said to be mmol / m^2 in table 1, but in mmol / m^3 in table 3. I suggest to represent the equations using concentrations per m^3 liquid as is custom in diagenetic modelling. How this is actually implemented in the model is less relevant.

Perhaps related to the previous comment: the equation for the diffusion across the sediment-water interface is very strange (in the supplement). I suspect -but could be wrong- that this is necessary to assure mass conservation and correct for using the

C3

wrong units (i.e. if units are per bulk sediment and not per liquid)? Also, I could not see that this is how Soetaert et al. implemented sediment-water exchange in their model.

It only becomes clear how resuspension is effectively included in this model based on the supplement. As this is the truly distinguishing feature, it should be included in the main paper. Related to that: how is this model integrated-i.e. which integration method is used? Many integration methods assume smooth dynamics, and one cannot just alter state variables directly.

There is no need for figures 7, 8 and 9.

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C4