Response to '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.' by Anonymous Referee #2 that was received and published: 17 January 2017

General comments

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.

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.

Dear Reviewer #2,

Thank you for your supportive and constructive feedback, which has helped us improve the manuscript. As a result of this review, we propose changing the manuscript by including a more refined analysis, removing Figures 9a and 9b, and emphasizing resuspension's effect on ammonium, and how this affects seabed oxygen consumption. Additionally, the model equations and methods were clarified based on the reviewer's comments.

For further details, please see our response below. All page and line numbers refer to the original submitted manuscript.

Thank you again for the review.

Best Regards,

Julia Moriarty, Courtney Harris, Christophe Rabouille, Katja Fennel, Marjorie Friedrichs, and Kevin Xu

Specific comments

Model results

1. 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 ?
Thank you for this suggestion. We agree that resuspension-induced increases in ammonium concentrations and the related increase in nitrification is important for maintaining the 'erosional oxygen profiles' and increasing seabed oxygen consumption during erosional periods. We have added text to emphasize this (Section 3.2, p. 10, line 29; Section 3.2.1, p. 11 lines 27-28 & p. 12, lines 1-4).

2. 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.

   Yes, we agree that this was somewhat confusing. We have clarified why this occurs in the manuscript (Section 3.3.1, p. 13, lines 18-19), and note it below.

   Please recall that resuspension increases the amount of labile organic matter on the seabed due to the partitioning of organic matter in the water column (Supplement S.1, now in Section 2.1.3, and addressed in the other review), which increases rates of oxygen consumption due to aerobic remineralization, especially ***following resuspension events***. Please recall that resuspension-induced oxygen consumption equals oxygen consumption from the standard model run minus oxygen consumption from the no-resuspension model run. Thus, although this effect occurs during the depositional portion of the resuspension event, it still increases the overall resuspension-induced oxygen consumption. Overall, the increase in aerobic remineralization offsets the decrease in nitrification estimated by the model during depositional periods.

3. 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 resuspension.
   Yes, the initial state of seabed biogeochemical profiles was based on results from a no-resuspension model run that was run until the model reached a steady state. This is noted in Section 2.2 (pg. 8, line 20).

For the Rhone delta implementation, the seabed profiles in surficial sediments (which have a largest effect on our results) adjusted within days to new environmental conditions. As a test case, we re-ran the model with initial conditions that equaled the model results from the last timestep of the standard model. These new profiles caused resuspension-induced seabed oxygen consumption to decrease slightly, from +20% to +15%, but primarily affected the first resuspension event. Because this 5% change was small compared to other sensitivity tests, and did not change our main conclusion, we did not run additional tests. This is now noted in Section 2.2 (pg. 8, line 20).

Model description:

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

   a. 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 delta t (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 delta t in the equation, and changes the units of Eised.
   Thank you for catching this error. We have corrected the discrepancy between equation (1) and table 1, and have switched to the 'standard' formula for erosion. (See Section 2.2.1, pg 5, lines 13-15).

   b. A related question concerns the sedimentation rate. Does this only apply when there is no erosion?
   Both reviewers noted that this was confusing, and so we have clarified this by adding text to Section 2.2.1 (pg. 5, lines 13-14), and by including the equations for deposition and net fluxes across the seabed-water interface, in addition to the formula for erosion (Section 2.2.1, pg. 5, lines 15).

   The background sedimentation rate of 10 cm yr^{-1} (equivalent to 14 kg m^{-2} yr^{-1}) was applied throughout the model run, but instantaneous rates of erosion and deposition varied, depending on hydrodynamic conditions. For example, when bed stresses were high, more sediment was eroded from the seabed than was deposited, causing net deposition rates to be negative. Similarly, as bed stresses decreased, less sediment was eroded and more sediment settled to the seabed, causing an increase of the net deposition rate to >10 cm yr^{-1}.

5. In diagenetic models, the units of dissolved substances are typically expressed in mmol/ m3 *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 O2, and NO3, (and of the parameters kO2 and kNO3, etc…) are said to be mmol / m2. But what is this: mmol/m2 bulk sediment or mmol/m2 liquid? (my guess is that it is per m2 bulk,

but I am not sure). Moreover, the units are not always consistent. For instance, the dO2/dz is said to be in units of mmol O2/m4 which suggests that O2 is in mmol/m3. Finally, the units of the monod constants are said to be mmol/m2 in table 1, but in mmol/m3 in table 3. I suggest to represent the equations using concentrations per m3 liquid as is custom in diagenetic modelling. How this is actually implemented in the model is less relevant.
We changed units for seabed tracers to mmol m^{-3} *liquid* for dissolved seabed tracers as is customary for seabed biogeochemical models. We note this in the caption of Table 1, and have edited the manuscript and equations accordingly. We also clarified units for the other variables that use m, m^2, m^3, and m^4.

6. 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 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.
We chose this formulation because we felt it was a parsimonious, efficient method, not because it was necessary to assure mass conservation or to correct for units. Because the surficial sediment layer was so thin (0.1 mm), this parameterization produced the same results as a model sensitivity test run that used a Fickian Diffusion Law (noted in the Supplement, pg. 3, line 1).

   However, we thank the reviewer for helping us realize that we mis-interpreted the description of the seabed-water column boundary condition in Soetaert et al. (1996). We removed this reference to Soetaert et al. (1996a,b) in the Supplement, pg. 2, line 24. (Note that although I cite the page and line numbers of the supplement from the original submission, in revision, the supplements were moved to the main body of the manuscript, as requested by both reviewers.)

7. 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.
Following both reviewers' advice, we have moved the supplement to the main paper, adding the information from S.1 to Sections 2.1.2 and 2.1.3 (pages 5-7). The material from S.2 was added as an Appendix to the paper because, although it is less crucial to understanding the results of the paper, we agreed that it should be in the same document as the rest of the manuscript.

   The ROMS framework was written to handle additional tracers using a variety of integration schemes. Specific numerical schemes for our model are listed below and are now referenced in the revised manuscript:

i. For advection of water column tracers, we used the MPDATA second-order scheme (Smolarkiewicz and Margolin, 1998), which has been used in several sediment transport and biogeochemical models (e.g., Fennel et al., 2013; Feng et al., 2015; Fall et al., 2014; Xu et al., 2013). This is now referenced in Section 2.2 (pg. 8, lines 20-22).

ii. For vertical mixing, we used a GLS (generic length scale) approach (Umlauf and Burchard, 2009), which is also commonly used (e.g., Bever et al., 2013, McSweeney et al., 2016; Feng et al., 2015) and referenced in the manuscript (pg. 8 lines 20-22).

iii. Transport within the seabed due to diffusion was calculated based on Sherwood et al. (2016), who implemented a scheme based on Thomas' algorithm (Anderson, Tannehill and Pletcher, 1984; p. 549-550) that was mass-conserving, and appropriate for non-spatially uniform diffusion rates and seabed layer thickness. It was first-order accurate when implemented for typical regional modeling spatial and temporal scales. The manuscript now references this method in Section 2.1.1 (pg. 5, lines 20-21).

iv. Changes in concentrations due to biogeochemical reactions were estimated using first-order methods, consistent with other biogeochemical models included in ROMS (Fennel et al., 2006; Franks et al., 1986; Powell et al., 2006) and Soetaert et al. (1996a,b). This is now noted in 2.1.3 (pg. 6, line 7).

v. Changes in the seabed biogeochemical profiles due to advection (i.e., resuspension and deposition) were not estimated using numerical integration, but were estimated using the 'bookkeeping' methods used in sediment transport models, as described in Warner et al. (2008) and Supplement S.2. These references are noted in the text.

8. There is no need for figures 7, 8 and 9.
We agree that figure 9a-b could be removed, and have done so. We kept 9c as we believe it is useful to examine how accounting for resuspension affects estimates of seabed oxygen consumption over time. Given the reviewers' interest in the sensitivity tests, we retained Figures 7 and 8.