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 #1 that was 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 intermittent 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.

Dear Reviewer #1,

Thank you for your supportive and constructive feedback, which we believe has improved the paper. The most substantial changes to the manuscript based on your review are summarized here:

1. **Attention to the re-partitioning of particulate organic matter:** We added a new sensitivity test that did not re-partition the organic matter in the water column. Results from this sensitivity test showed that even when resuspended organic matter was not repartitioned in the water column,

oxygen consumption still increased due to resuspension events over timescales of days to two months (see item 5 below). Additionally, we improved the justification for our approach.

2. **Clarification of the model equations and methods**: We moved the supplement into the main manuscript, and more attention was given to the presentation of equations and variables.

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

Thank you again for your review.

Best Regards,

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

Specific comments

1. 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.
   It is possible to predict resuspension events based on wave and current shear stresses, but simple parameterizations often have a large amount of uncertainty. We have added text to Section 4.2, line 22 to suggest that such parameterizations predicting erosion and deposition could be combined with biogeochemical parameterizations for seabed-water column fluxes to estimate the effect of resuspension on oxygen dynamics in larger-scale numerical models in future research efforts.

2. p.4, l.21-22: What is the estimated seabed oxygen consumption at the study site itself?
   We have added text to Section 1.1 (pg. 4, lines 21-22) to clarify this.

3. 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?
   This equation estimates a flux of sediment across the seabed-water interface. This is now clarified in Section 2.1.1 (pg. 5, lines 13-14).

   The erosion rate parameter, M, is a rate constant representing how much sediment is entrained from the seabed into the water column per unit time as bed stress increases, i.e. the erodibility of the seabed. We now note this in Table

1, and repeat the reference to Warner et al., (2008) in Section 2.1.1 (page 5, line 14) for further reference.

4. 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?
   In this location POM only accounts for 3% of the combined inorganic + organic particulates in the seabed by weight (Pastor et al., 2011a). Because 3% is small compared to the uncertainty in estimating sedimentation rates and the uncertainty introduced by imposing a long-term sedimentation rate on a two-month time period, we chose to develop the model such that POM deposition does not contribute to the thickness of eroded or deposited seabed layers. Thus, the instantaneous inorganic sedimentation rate equals the rate at which seabed layers are shifted downwards (or upwards, during erosional periods). Also, our inorganic sedimentation rate of 10 cm y^{-1} (or 14 kg m^{-2} yr^{-1}) is equal to the total sedimentation rate used in other models of this site (i.e. Pastor et al., 2011a). In response to this question, we have clarified that we neglect POM's contribution to the total sedimentation rate in Section 2.1.1, p. 5, line 18.

   Note that neglecting POM's contribution to the thickness of seabed layers is also justified by sensitivity tests that indicated that even a 50% change in total sedimentation rate caused relatively small variations in seabed and bottom water oxygen dynamics (Figures 7, 8). We therefore expect a 3% change to be relatively unimportant.

5. 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.

   First, thank you for this, and other related, comments, which prompted us to include an additional sensitivity test and the inclusion of additional supporting information from the literature. Specific responses and changes to the manuscript are listed below:

(1) We added the information from Supplement S.1 to Sections 2.1.2 and 2.1.3 (pages 5-7).

(2) We significantly improved the justification for our approach in the Methods (in Section 2.1.3, pg. 7, near lines 5-21). Because it was a major concern for the reviewer, we also justify our approach here. As the reviewer notes, the model converts a fraction of resuspended refractory POM into labile POM upon entrainment from the seabed into the water column. This modeling approach is supported by laboratory experiments by Stahlberg et al. (2006) indicating that organic matter remineralization rates increased during and in the days following resuspension events, and that changes in remineralization rates were not only due to changes in oxygen availability. Literature pertaining to how resuspension affects the remineralization of particulate organic matter over days to weeks is limited, so we also considered related studies that focused on redox oscillations and remineralization (e.g. Gilbert et al., 2016; Sun et al., 2003; Caradec et al., 2004; Aller, 1994; Wakeham and Canuel, 2006; Arzayus and Canuel, 2004). Because guidance from this literature is inconclusive, we chose "repartitioning" for the organic matter that mimics the changes in remineralization described in Stahlberg et al. (2006), and is consistent with field data from the Rhone River (see bullet (3)).

(3) We added a 'no-repartitioning' sensitivity test that was the same as the standard model, but did not re-partition the resuspended POM in the water column. Instead, any labile organic matter in the model was assumed to stay labile, and refractory organic matter in the model was assumed to stay refractory.

Overall, results from this no-repartitioning model run indicate that estimates of *seabed oxygen consumption* were sensitive to this repartitioning, but estimates of *water column oxygen consumption* were not. **Since water column oxygen consumption is the dominant component of the total oxygen consumption, our overall results were insensitive to the re-partitioning parameterization.** However, as noted above, *seabed oxygen consumption* was sensitive to the repartitioning of organic matter. Compared to the standard model where resuspension increased seabed oxygen consumption by +20%, resuspension had a negligible effect on seabed oxygen consumption in the "no-repartitioning" model run over timescales of two-months.

However, note that results from the "no-repartitioning" model conflict with observations from Toussaint et al. (2104; their Fig. 7), who observed no significant change in seabed oxygen consumption (i.e. no reduction of diffusive oxygen fluxes into the seabed) following erosional periods like that estimated by the 'no-repartitioning' sensitivity test. This implies that the model with repartitioning better describes the observations on the Rhone prodelta compared to the model without repartitioning.

Results from the no-repartitioning sensitivity test are presented in Section 3.3.1, Figures 7 & 8, and are discussed in Section 4.2 (first paragraph) and Section 4.3 (p. 17, lines 26-28 & p. 18, line 2).

6. 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.
   We agree that this information should be in the main manuscript, as opposed to a separate document. We have therefore moved this material from the Supplement to an Appendix (located below the Conclusions). We think this information is better suited for the Appendix, rather than the Methods section, because although this technical information will be informative to numerical modelers, it may not be as interesting or relevant to other readers.

7. 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.
   Thank you very much for pointing this out. We have changed all units to mmol m^{-3} d^{-1} throughout the paper, including on p. 6, lines 17-21.

8. 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.
   Yes, we agree that porosity can fluctuate in surficial seabed sediments. Given that we expect the most significant effect of changes in porosity to be changes in diffusion, and that the manuscript already includes sensitivity tests to diffusion within the seabed, however, we suggest that this sensitivity test should be included in a future paper that more thoroughly investigates how seabed and bottom water dynamics are affected by different combinations of environmental conditions.

9. 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 co-occur but that erosion dominates?
   Sediment fluxes across the seabed-water interface are assumed to equal the difference between deposition and erosion as estimated for each time-step, so

they are not mutually exclusive and may co-occur. This is now noted in the text, and we have added the equation for sediment deposition (Section 2.1.1, pg 5, line 15).

10. p.7, l.30: What are the units of the 5x6 model grid?
    Units are 'grid cells'. This is now noted in the text (Section 2.1, pg. 7, line 30).

11. 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?)
    We have edited the text per the reviewer's suggestion (p. 9, lines 4-6). To reduce confusion about our definition of the bottom boundary layer (BBL) versus other definitions, we have also revised the entire manuscript to refer to this region as "bottom water", rather than the bottom boundary layer.

12. p.9, l.7-9: Why are the rates of biogeochemical processes in the BBL not added to the code as output parameters?
    We have revised the model code so that this information is saved. This has simplified the manuscript and affected text in Section 2.2 (p. 9, lines 7-9), and slightly changed the model estimates given in the Results.

13. 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.
    Thank you for noticing this discrepancy. We have added a factor of (-1) to equation (8) (p. 9, l. 28) so that increases in dO_2/dz_{OPD} correlate with sharper oxygen gradients, and have clarified the definition of z_{OPD} in Table 1 (pg. 28, last line).

14. 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?
    We also believe the fate of eroded sediment may be important to quantify to better understand seabed & BBL biogeochemical dynamics. However, for this paper we wanted to focus on the vertical exchange processes. Instead, we believe the issue of resuspended sediment that is transferred in to, or out of, the local area is better addressed by implementing the model in three dimensions. We are planning to implement the model for the Gulf of Mexico, and expect to address this issue in that publication.

15. 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?
    We have now clarified how erosion depths were calculated in the Methods (Section 2.2, pg. 9, line 12). Depths of erosion depend on both bed stress and duration of event.,.

16. 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.
    Please see bullet (5) of this response to reviews.

17. 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?
    Yes, we agree that the rapid consumption of oxygen during depositional periods occurs due to (1) re-deposition of POM, and the conversion of some POM from refractory to labile, as the reviewer suggests, as well as (2) resuspension-induced rates of nitrification (see Figure 6). During erosional periods, process (2) dominates oxygen consumption, and therefore the maintenance of 'erosional oxygen profiles'. Both (2) and (1) assist the destruction of 'depositional oxygen profiles'. We have edited the text to clarify that both processes were important.

18. p.11, l.26: Wouldn't 'quasi-steady state' be a better term there than 'equilibrium'?
    This has been changed here (p. 11, line 26), as well as on p. 9, line 22, where it is also used.

19. p.12, l.1-2: Why would entrainment of nitrate increase the nitrification rate? Eq.5 does not show any NO3-limitation.
    Thank you for catching this mistake; we have removed this.

20. p.12, l.10-12: This sentence seems to lack something. I was expecting to read "..compared to xxx" at the end.
    We agree, and have rephrased this sentence.

21. 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).
    While we agree that this is interesting information for some researchers, the focus of this paper is on oxygen dynamics and introducing the coupled model. As the paper is already quite long, and the proposed table/figure does not directly support the main conclusions, we respectfully believe that including such a table/figure is beyond the scope of our paper.

22. 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?
    Yes, we agree that this is confusing, and have clarified the text (Section 4.1,p. 14, l. 14) to state that we mean short time periods, from hours-to-days, not just the entire resuspension event as a whole, which does occur over a period of days.

23. 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.
We thank the reviewer for directing us to this paper. The text on p. 15, lines 12-15 focuses on the criteria needed for **local** resuspension, i.e. when material is resuspended and then re-deposited, to affect oxygen dynamics in various areas. In contrast, the modeling study in Almroth-Rosell et al. (2011) primarily focused on the role of redistribution of resuspended POM, and its affect on oxygen dynamics, and not the role of local resuspension. This reference is therefore especially pertinent to our future work section, and we have added it to Section 4.3 (p. 18, lines 10-12). We also replaced 'resuspension' with 'local resuspension' to make the text clearer in Section 4.1.1 (p. 15, lines 9-17), and other places within the manuscript.

24. 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.
We agree that this analysis would be valuable, but believe it should be incorporated into a future paper that more thoroughly investigates how seabed and bottom water dynamics are affected by different combinations of environmental conditions. Here, especially because the paper is already quite long, we have chosen to focus specifically on a single time period at a single site, i.e. the Rhone subaqueous delta in Spring 2012.

25. p.16, l.3-5: Wouldn't OM loading also be lower in winter and thereby impacting oxygen consumption?
We agree that changes in organic matter loading can affect oxygen consumption, and we have added a paragraph about organic matter loading to Section 4.1.2 (p. 16, after line 7).

26. 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?
We do not believe that the spatial scale of these regional models should affect the scientific results, but acknowledge that inclusion of a full sediment model in larger-scale water quality models may be inefficient. This is now noted in Section 4.2, pg. 16, lines 19-22.

27. 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)
We estimated the sedimentation rate based on Pastor et al. (2011a), as noted in Table 3. Pastor et al. (2011a) based their estimate on the radioisotope observations of Zuo et al. (1997, 1998), Radakovitch et al. (1999) and Miralles et

al. (2005). As you note, these observations provide indications of the total sedimentation rate, which introduces about a 3% error in our estimate of 10 cm/yr. As noted above, in the response to comment #4, however, (1) this error is small compared to other sources of uncertainty, and (2) sensitivity tests indicate that even a 50% error in the sedimentation rate does not produce large changes in the results.

28. 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.
    This fraction represents pyrite and other materials that are assumed to be oxidized sufficiently slowly such that they do not affect model estimates over the timescale of our study (2 months), as noted in Soetaert et al. (1996a,b), which is cited by this paper. It does not include N_2. In our study, we assumed that 99.5% of ODUs were solid and inert. This value, 99.5%, is on the high end of the range of values in the scientific literature, but has already been justified for this study site by Pastor et al. (2011; see Section 4.3.1), as noted in our Table 3.

29. 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.
    Thank you for this suggestion. We have added schematics showing how particulate organic matter is partitioned in the model during resuspension and deposition for the standard model run and no-repartitioning sensitivity test.

Technical corrections

30. 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?
    We have clarified the definition of resuspension (event) by adding it to Table 2A, and have confirmed that we now use it consistently in the manuscript. Note that although the verb *resuspend* and adjective *resuspended* are synonymous with erode and eroded, the noun *resuspension (event)* refers to the entire cycle of erosion and deposition.

31. 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).
    We have deleted this phrase from p. 3, line 26, as suggested.

32. p.3, l.28: Section 1.1 fits better as the first section of the Methods.
    We have reorganized the paper, as suggested.

33. p.3, l.32: 'located at _25m water depth'
    We have changed 'in' to 'at' as suggested.

34. p.3, l.33: I would prefer 'model validation' over 'model evaluation' here
    This sentence refers to the use of data from both (1) Pastor et al. (2011a), which was used to validate the model (i.e. to confirm that the model was implemented correctly based on its ability to reproduce model estimates from Pastor et al. (2011)'s previously implemented Soetaert model), as well as data from (2) Toussaint et al. (2014), whose data was used to evaluate the model (i.e. to compare our model estimates to observations). To clarify this sentence, we therefore replaced "model input and evaluation" with "model input, validation and evaluation" (p. 3, line 33).

35. 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.
    We agree, and have removed the last two rows from Table 4 (pg. 32).

36. p.9, l.10: 'it's' should be 'its'
    Thank you, this was changed.

37. p.9, l.29: Table 5 does not show this; remove the reference here.
    Thank you for this correction, we have removed this reference to Table 5, and added it to the correct location (p. 9, l. 26).

38. 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.
    Thank you for this suggestion. We have removed the text describing the contribution of ODUs (p. 12, lines 19-29).

39. p.16, l.10: change 'produce' to 'reproduce'
    We have changed this.

40. 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.
    Yes, we do not expect the maximum seabed oxygen consumption and the maximum bottom water (i.e. BBL) oxygen consumption to equal the maximum seabed+BBL oxygen consumption. This is because the maxima of the seabed and BBL oxygen consumption may not coincide with each other. The same logic applies for the minima of oxygen consumption.

    For the averaged values and value on June 1, the sum of the total seabed oxygen consumption and total BBL oxygen consumption does equal the total seabed+BBL oxygen consumption, except that these values are sometimes off by one unit due to rounding error when using two significant digits.

41. 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.
    We have removed this statement (p. 14, line 30).

42. p.15, l.25-26: Too many references here.
    We reduced the references to only the most essential (from 5 to 2 references; p. 15, l. 25-26).

43. p.16, l.19-23: More appropriate than what?
    We have clarified this sentence so that it focuses on which types of parameterizations may be most applicable for the Rhone delta and similar environments (i.e. compared to other types of parameterizations).

44. p.17, l.12-17: This section might better fit in Section 4.1.2, when discussing temporal variations
    We agree, and moved this (Section 4.3, p. 17, lines 19-23) to a new paragraph at the end of Section 4.1.2. As a result, we combined the first and fourth paragraphs of Section 4.3 to suggest that future work could include applying the model (in 1D or 3D) for different environments (Section 4.3, p. 17-18).

45. p.17, l.32-33: Something seems to be missing in this sentence.
    This was a typo. We deleted the word ‘may’.

46. p.28/29, Table 1: carefully check the units, especially in relation to Eq. 2-5.
    Thank you, and we have done so.

47. 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.
    We have added the vertical lines to Table 6.

    We added model averages for depositional and erosional periods to Table 6. However, note that the grey shading indicates resuspension events, which includes the cycle of erosion and re-deposition, not just the erosional time period. This has been clarified in Table 2, and in the captions of Figures 4 and 6.

    We have clarified the caption of Table 6 so that it indicates that the maximum and minimum values were calculated based on the entire model run.

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

We have defined SSC in the caption, and added the dates to the caption (p. 34, Fig. 3). Adding the dates to the profiles made the figure look too crowded.

49. 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.
We have made the suggested changes (p. 35, fig. 4).

50. 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.
We have changed Fig. 5 so that it plots profiles for the same event that is the focus of Figure 3. We have also changed the depth scale for the O2 profiles and added the column-headers (p. 36, Fig. 5).

51. 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'.
We have made the suggested changes to Fig. 6 and the caption (p. 37, lines 2-5).

52. 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'.
To make the scenarios easier to compare, we made the lines and dots thinner. We also combined the two figures into one figure so that the panels could be taller, and thus, easier to read. We did not divide these 4 panels (2 in the original Figure 7 & 2 in the original Figure 8) into 8 panels because we tried to balance your comments with suggestions from the other reviewer, who suggested we remove a few figures, including Figs. 7 & 8.

    Also, one of the main points of the Figures 7 & 8 is that changing the parameters in the model generally does not have a large effect on the results, suggesting that our results are relatively robust. We now cite Figures 7 & 8 when we make this point (Section 3.3., p. 13, lines 31-32; p. 14, line 1).

    The apostrophe from "it's" was removed (pg. 38, line 3).