

## Reviewer 2

**The manuscript goes through a lot of considerations/effort to parameterize the proposed model. I understand the rationale for this, but I was not convinced that it actually improved the accuracy of several parameter assessments. In other words, I am concerned that all of this effort based on numerous assumptions gives a false impression of how well the model is constrained in reality, and thus, how robust its predictions will be. In that light, I find that several model assumptions should be validated better. Admittedly, this is not an easy task, but it would give much more credibility to the model if it was validated in greater detail than as described. Specifically, I suggest to validate it against a couple of existing comprehensive datasets produced by detailed site-specific biogeochemical measurements. In addition to sediment water fluxes and depth profiles of all simulated species, including ammonium, it should include parameters such as the ratio between oxic/anoxic organic matter degradation. Without realistic reproductions by the model of all included variables, future model predictions and scenarios can be seriously misleading. Some comprehensive datasets are described in the Wang and Van Cappellen (1996), Rysgaard et al. (1998), and Fossing et al. (2004 - NERI Technical Report, No. 483).**

We thank the review for his/her constructive comments on our manuscript. Regarding the point made above, we would first reiterate that the model has been quasi-validated against a global database of benthic fluxes from 185 stations around the globe. This, in our view, demonstrates that the POC degradation kinetics are adequately described. The opportunity to further ground-truth the model using fluxes of other solutes is limited by the severe lack of fluxes collected in situ. Porewater data and nitrate penetration depths were used as further comparisons. We do not claim that the model can perfectly simulate field data from site-specific locations. This is obvious from Figs. 3 and 4 and arises from the use of empirical coefficients (e.g. bioirrigation, bioturbation, CBE) that themselves carry considerable uncertainties. Further validation of the model against the shelf sites proposed by the reviewer would require more complex reaction network than in our simple model, including coupled Fe-Mn-S cycling. Our model is mainly tuned to the N cycle in the upper 20 cm of the sediment, and it is here that we have most confidence in our results. In our previous study, we also made the point that the model/approach is not well suited to sites on the continental margin due to the broad heterogeneity found there. In the revised version of the manuscript we emphasize these points more clearly.

**Because ‘losses’ of reduced species, for example via burial, are usually small, oxygen uptake of the sediment should match the input of degradable organic matter well. This point is reflected here in Fig. 3a. However, a total sediment depth of only 10 cm is used in the model and with the lower Neumann boundary conditions used, the results in Fig. 3 imply that all organic matter is degraded over that depth. This is in conflict with many published studies that have measured substantial degradation activity well below 10 cm. This hints that the chosen fixed rate constants for the slower organic matter degradation – a key-part of the model – are off, at least for some sites.**

Firstly, all the organic is not degraded: we have three fractions and the least reactive one is set to CBE; itself independently imposed according to the function provided by Dunne et al. We agree that the lower boundary condition in the model is uncertain. Repeating the comment to Reviewer #1, we note that the sediments were simulated to 20 cm, not 10 cm, and only the upper 10 cm was shown in Fig. 4 (will be clarified in the manuscript). Even so, anaerobic organic carbon mineralization below 20 cm will lead to non-zero concentration gradients at 20 cm. However, the overall contribution

anaerobic degradation below 20 cm is a small fraction of degradation in the upper 20 cm. For instance, log-log relationships of radio-tracer measurements of sulfate reduction versus depth in Aarhus Bay show that over 95% of OM is mineralized in the upper 20 cm of sediment (Holmkvist et al., 2011, GCA 75, 3581-3599). Imposing a zero-gradient condition for ODU and  $\text{NH}_4^+$  is justifiable. We also make the point there and in the revised manuscript that the kinetics of POC mineralization in deeper anaerobic sediments is not the aim of the study and hardly constrainable from the empirical data presented in the paper.

**The manuscript does a good job reviewing earlier modeling work that is similar. However, it is unclear to me how and why this new model stands out relative to those. Why is this model application better? What are the improvements and advantages? These distinctions should be made.**

The principal advantage is for Earth System model applications, and with temporally variable fluxes of POC to the seafloor. The over step forward is that we use the Martin curve that is water column biogeochemistry to constrain the reactivity of organic matter reaching the seabed. This will be clarified. Please see also the response to Reviewer #1.

**The manuscript is fairly well organized, but due to a dense writing style, the numerous details given, and many abbreviations use, it is not the easiest one to read. This could be lightened up, by tabulating some of the information and by placing some of the detailed derivatives in one or two appendices.**

We will include a glossary of terms in the revised manuscript.

**Finally, it is stated that one key assumption is that the “rate of POC degradation at the sediment-water interface,  $R_{\text{POC}}(0,t)$ , is assumed to be continuous with POC degradation in the water column (Arndt et al., 2013)”. In other words, the rate of POC degradation in the bottom of water column is used to parameterize the upper sediment POC degradation rate. This assumption seems to me to be a stretch given the substantial differences in microbial community compositions and densities between the bottom water and in top of the sediment. I may be wrong here, but I do not recall that the Arndt paper supports this crucial assumption.**

Firstly, the Arndt reference was mis-cited and will be removed. It is true that the specific nature of sedimentary environments is vastly different from the bottom water, and other uncertainties were listed at the beginning of section 4.2. However, we take the top down view that differences in the microbial community composition and densities between the bottom water and in top of the sediment are determined by the flux of POC deposited to the sediment. In other words, we would expect that the flux of POC to the seafloor is the main factor that will determine microbial biomass and POC degradation in the seafloor. With this assumption, the reactivity of the bulk material does appear to be broadly consistent with the water column reactivity (Fig. 3 to 6). We are still at a very early stage of understanding the relationship between the continuity of POC reactivity at the sediment water interface, not least because the reactivity of material in the lower water column is probably even less well understood than in the sediments. The assumptions made in the paper provide a basis for further research on this topic.

**Specific comments**

**Page 1, line 17: This seems to be in conflict with the statement starting on page 10, line 33.**

There is no conflict in these two statements, and this only requires clarification. Although our derived rate constants differ with the phytoplankton decomposition experiment by Westrich and Berner (1984), they are in perfect agreement with the experimental data (Fig. 7). This is the main point we wanted to make and the text will be rephrased accordingly.

**Page 2, line 4: I don't know the policies of BD, but to give 9 references to support a statement seems to be too much. Some journals will not allow more than three references.**

We will revise this.

**Page 2, line 18: Define in few words the 'reactive continuum model'.**

Continuum models assume a continuous distribution of organic matter reactivity, thus avoiding the need to partition the bulk material into a defined number of discrete compound classes. A similar definition will be added to the manuscript.

**Page 4, line 12: What is meant by 'a rather strong assumption'?**

We mean that there is no data to support this assumption. Please see also comment to Reviewer #3.

**Page 7, line 7: I believe that the clever definition in terms of diagenetic modeling of an ODU pool was first proposed by Soetaert et al. (1996). This should be recognized.**

Correct. We will cite them appropriately in the revised manuscript.

**Page 7, line 12: Solutes can be transported by advection, molecular diffusion, irrigation, and also by 'diffusion-like' bioturbation. The latter can be as important as molecular diffusion (Aller and Aller, 1992).**

Bioturbation is included in the calculated of the depth-dependent sediment diffusion coefficient:  $D_s = D_{sw}/t_o + D_{Bio}$ , where  $D_{sw}$  = diffusion coefficient in seawater,  $t_o$  = tortuosity and  $D_{Bio}$  = bioturbation coefficient.

**Page 8, line 20: Am I misunderstanding something here? With the total input of degradable organic matter known and with the model boundary conditions used, the good match between the measured and simulated oxygen flux is expected. It does not mean that it "demonstrates that the total carbon degradation rate is correctly simulated at each station".**

It does because the depth-integrated rate of POC degradation is partly estimated from the carbon burial efficiency (Eq. 20). Not all the POC that arrives at the seafloor is mineralized.

**Page 12, line 25: A highly effective numerical solution to this prohibitive modeling problem was proposed by Berg et al. (2007) and should be cited.**

We will include this reference.