

Interactive comment on “A new look at the multi-G model for organic carbon degradation in surface marine sediments for coupled benthic-pelagic simulations of the global ocean” by Konstantin Stolpovsky et al.

Anonymous Referee #2

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This manuscript focuses on modeling particulate organic matter degradation in marine sediments. The authors correctly stress that this topic needs more work in order to improve our quantitative description of benthic biogeochemical cycling on regional and global scales. An existing sub-model for organic matter degradation (the multi-G model with 3 fixed decompositions rates: 70, 0.5, 0.001 yr-1) is adopted, and most of the manuscript focuses on parameterizing the rest of the model without detailed knowledge from site-specific biogeochemical sediment measurements. The model is tested against a large global database of sediment-water fluxes and sediment profiles

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for oxygen and nitrate.

BGD

I do find the work to be important, but the manuscript has a few troubling parts:

As said, the manuscript goes through a lot of considerations/effort to parameterize the proposed model. I understand the rational 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).

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.

The manuscript does a good job reviewing earlier modeling work that is similar. How-

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ever, 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 manuscript is fairly well organized, but due to a dense writing style, the numerous details given, and many abbreviations used, 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.

Finally, it is stated that one key assumption is that the “rate of POC degradation at the sediment-water interface, $R_{POC}(0,t)$, 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.

If the authors can address the above concern the remaining revisions to the paper should be minor. Below are listed some specific comments:

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

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.

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

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

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.

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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).

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".

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.

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