

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.

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» A continuum formulation based on the recognition that POC has a spectrum of reactivities seems to be a better representation of reality compared to a multi-G model with a finite number of components. The question is then: in what way is the 3-G model presented in this paper an improvement over that of SDW2015? The authors stress that the model in the paper under discussion is constrained by the Martin curve for POC degradation in the water column; is this an important difference with SDW2015? If so, why not update the continuum representation?

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We thank the review for his/her constructive comments on our manuscript. A continuum formulation is more appropriate under steady state conditions, i.e. when changes in POC rain rate (RRPOC) are slow compared to transport and reaction rates in sediments. POC degradation in the continuum model (SDW2015) is a function of RRPOC, such that RPOC over the whole sediment profile responds instantaneously to changes in RRPOC. The major advantage of G-type models is that they can be used at the short time-scales, for example temporal variations in rain rate. The temporal storage of POC or solutes is therefore explicit in the multi-G approach. We omitted this information from the final version of the paper (for length consideration) but now recognize its relevance and will include it in the revised manuscript (Reviewer #2 made a similar comment).

Specific comments

» P3 L30: A typo in Equation 3: POC_{j,i} should be POC_{i,j}.

Corrected as suggested.

» P3 L23: The POC fraction that degrades over a 1000-yr time scale is termed here "refractory." There is plenty of evidence for POC remineralization taking place much deeper than the bioturbated zone (e.g., microbial methanogenesis beneath the sulfate reduction zone), and POC that degrades at time scales well above 1000 yr is not well described as refractory. A better term, used on P10 L14, would be "poorly reactive."

We agree that POC remineralization does not end at the base of our modeled sediments (20 cm). However, for our purposes the k_3 value in present study is low enough to call this fraction "refractory". We will clarify this in the revised manuscript.

» P4 L19: The "apparent reactivity" defined here is the weighted average of the three reaction rates (Equation 7), and it corresponds to the mean of a continuous distribution of POC reaction rates. A better term for this parameter would be "average reactivity" or "bulk reactivity:" the "apparent" refers to the 3-G model, which is an approximation of reality that the authors recognize (P11 L30).

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We used “mean reactivity” in the older versions of the manuscript but later changed it to “apparent reactivity” to be consistent with previous studies (e.g. Arndt et al., 2013).

» P6 L11-24: I found the introduction of the parameter $fr=f1/f0$ very confusing. Is it necessary to get bounds on $k0$ and $k1$? If that's the case, it should not be used further in the paper, where it can be substituted by the ratio $f1/f0$, which is more meaningful to the reader.

Agreed. We will replace fr with $f0/f1$.

» P7 L2: There is no listed reference to the quoted Burwicz et al. 2011. This is an important source, as the sedimentation rates control the actual value of $k2$ in the modeling (Equation 15). Corrected.

» P7 L9: What are "Porewater distributions in the porewater?" Do you mean concentrations?

Corrected. 'in the porewaters' will be deleted.

» P8 L9: The zero-gradient boundary condition at the base of the bioturbated layer (10 cm) is fine for oxygen and nitrate (Figure 4), but it may not be adequate for reduced products of anaerobic POC remineralization (ODUs) such as sulfide generated by sulfate reduction beneath the bioturbated layer. On the other hand, maybe the overall contribution of ODU is small and this is not an issue.

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 for ODU and $NH4+$ at 20 cm. However, the overall contribution of 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

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ODU and $NH4+$ is hence justifiable. We will also stress in the revised manuscript that the kinetics of POC mineralization in deeper anaerobic sediments is not the aim of the study since it is hardly constrainable from the empirical data presented in the paper.

» P11 L30: The authors recognize here the simplification of the 3-G model they use; this important issue should be stressed in the abstract and conclusions.

We agree. Corresponding changes will be made.

» Figures 1 and 7: These two figures show the decrease of apparent reactivity with water depth and of POC with time. In both cases, this decrease approximately follows a power law, with values that rapidly decrease with increasing depth/time. It is hard to judge fits and differences in these plots, because they are dominated by few relatively large values near the origin. I suggest adding to these two figures a plot in log-linear or log-log scales, which will show the fit for all values.

Log scale gives a false feeling of importance of variation in $kapp$ value at the deep sea (see the answer to the first question to Rev. 3), hence we prefer to keep it linear.

Please also note the supplement to this comment:

<https://www.biogeosciences-discuss.net/bg-2017-397/bg-2017-397-AC1-supplement.pdf>

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-397>, 2017.

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