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Discussions

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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 #1

Received and published: 2 November 2017

This paper presents a three-component (3-G) diagenetic model for POC remineraliza-
tion in near-seafloor bioturbated sediment, where the three components are degraded
over time scales between few days and 1000 years. The model parameters are con-
strained by a database of oxygen and nitrate fluxes at the seafloor and are also val-
idated against measured profiles of oxygen and nitrate pore water concentrations in
sediment. The goal of the work is to provide a general model of near-seafloor diage-
netic processes useful for global biogeochemical Earth system models. The diagenetic
model and the data that constrain it are clearly explained, the comparison of model

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[Discussion paper]

predictions and measurements is discussed thoroughly, and the manuscript presents a solid procedure to model POC remineralization in near-seafloor marine sediments.

I have some comments that the authors should address, however, concerning the relationship of this paper to earlier work by the same authors (Stolpovsky et al. 2015; hereafter SDW2015). The paper under discussion is based on the the diagenetic modeling procedure of SDW2015, except that it uses a 3-G formulation for POC remineralization where each of the three fractions of POC is degraded at a constant reaction rate. SDW2015 instead applied a continuum formulation where the overall reaction rate decreased with sediment age and depth. The fundamental basis for a continuously decreasing reaction rate is the recognition that POC is a mixture of components that have a broad range of reactivities. As the most reactive fractions are consumed first, the overall reaction rate will diminish during burial; see the lucid presentation of this point by Boudreau and Ruddick (1991, quoted in the paper).

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 fit to measured fluxes and concentrations seems to be good in both models, and the continuum model is also defined by a small number of parameters that describe the decrease of reaction rate during burial. 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? Perhaps I am missing something and the 3-G model has other advantages. In my opinion, this paper needs a clear explanation of why it is a step forward compared to SDW2015 as opposed to a subsidiary modeling exercise.

I also list a number of suggestions to improve the text and figures in the following.

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

P3 L23: The POC fraction that degrades over a 1000-yr time scale is termed here "refractory." There is plenty of evidence for 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."

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).

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.

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

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

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 ODUs is small and this is not an issue.

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.

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.

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