Biogeosciences

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 #3

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The manuscript describes an early diagenetic model in which the mineralization of POC is formulated using 3 POC fractions which differ in their reactivity. This is a commonly used approach embracing minimum complexity while still being able to reproduce flux or porewater observations. The main emphasis of the paper is to establish a parameterization of such a model that is applicable across a wide range of conditions, yet is simple enough to be possibly lend itself for use in Earth System Models. The model achieves this by only requiring knowledge of the burial efficiency and the apparent reactivity of the organic matter at the sediment water interface. These two parameters

are determined from a published relationship of the burial efficiency with the rain rate, and by establishing a connection between kapp and to the settling organic matter via the Martin curve.

The presentation is quite clear and concise, and the authors nicely reiterate the main points in the discussion and conclusion. I also appreciate the significant effort put into the comparison of model simulations to observational data, and the discussion of uncertainties. The latter addresses some of the concerns that I had reading through their methodology (see below). However, there are a number of assumptions I found surprising and that may warrant more discussion.

- use of the Martin curve: this is a good starting point, and the concerns regarding the use of this relationship are discussed. However, it is unclear how sensitive the results are to the value of b (the paper makes the case that b=0.86 matches the data but it doesn't address the sensitivity to this parameter and papers such as the one cited by Buesseler et al. illustrate the wide range of values for b) - the burial efficiency relationship reported in Dunne et al. is used to estimate the proportion of the refractory POC pool and to constrain the rate constants. This estimate of the CBE is applied at 10 cm, presumably well above the depth at which mineralization becomes negligible (which clashes with the choice of no gradient lower boundary conditions at 10cm for POC and mineralization products). Furthermore, the CBE estimate of Dunne et al. is off by about 80% (the average of (estimated-true burial eff.)/true burial efficiency for the data shown in Figure 2 of Dunne et al.). A central benefit of the work presented here relates to its applicability across a wide range of oceanic conditions. I suggest that the authors quantify and show the impact of the considerable uncertainties in the fundamental input variables b and CBE.

- particle classes (page 4): As the authors state, the choice of two particle size classes with distinct sinking velocities is a tremendous simplification (see e.g. Jackson et al. 1997, DSR I 44: 1739-67). Furthermore, the assumption is then made that the ratio of the rate constants for small and large particles of the same POC pool i match the ratio

of the settling velocities (equation 4). I simply don't understand the rationale for this particular choice (e.g., if the POC was the same in the small and large fraction of pool i, then one may expect the rate constants to be the same, resulting in a more pronounced removal of slow settling small particles, which is not what is implemented here). If there is compelling evidence for such a scaling of rate constants, then this needs to be presented, or the relevant literature needs to be cited. Else, the consequences of such an assumption need to be quantified, or the assumption revisited.

- The rate of POC degradation is said to be continuous between sediment and water (page 5, line 14). This presumably is not true for volumetric rates, as POC concentrations change drastically at this interface. On line 20, it then says that kapp is continuous across the sediment-water interface. However, first order rate constants intrinsically reflect the abundance and activity of microbes involved in the breakdown of organic matter, which surely varies across this interface. What data is there to support this approach? (relevant because kapp is used as an anchor for the sediment reaction rate constants).

- model comparison: the 3G model is parameterized to essentially match the OC mineralization integrated over the top 10 cm (input from Martin, burial from Dunne), while parameters were optimized in the power function approach of Stolpovsky et al. 2015 to match the fluxes. Hence, it is not a big surprise that the O2 fluxes match closely, though it is not immediately evident what motivates the use of a 3G approach over the continuum approach the authors presented previously. And what about the 2G model? Can the reason it performs worse be linked to the parameterization (eq. 21; what is mk? summation over k?), in which the less reactive fraction of the 2G model is set to be equal to the refractory pool in the 3G model?

- benthic exchange fluxes reported in Figure 4 seem to be similar for profiles with similar gradients at the sediment-water interface but differ vastly below that (some have subsurface maxima). Are irrigation fluxes not important for the overall exchange, even at the shallower stations?

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- Equation (2): what does w_j represent?

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