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Discussions

Interactive comment on "A reactivity continuum of particulate organic matter in a global ocean biogeochemical model" *by* Olivier Aumont et al.

Anonymous Referee #2

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The paper introduces a new scheme to parameterize a spectrum of particle lability classes in in global biogeochemical model of the marine pelagial. The scheme follows approaches that have been developed for benthic degradation (Boudreau and Ruddick, 1991), and applies these in modified form to the pelagic realm.

I think the approach is a very interesting and new one. However, I have several concerns about the way it is presented, which hampers understanding and evaluation of the approach. My concerns are related to the method description and comparison to (and distinction from) other models, that include different detritus classes, or otherwise attempt to represent the particle continuum with depth and time. These are explained in detail below:

Method:

I had some difficulties to understand how exactly lability is parameterized in this model; what I understood from the method description is the following:

After an overview about the different types of lability models (multi-G, RC), the authors present and discuss the RC model applying a Gamma function, namely g(k,0)=gamma(nu,ak)/Gamma(nu) (Eqn. (1)). On page 6, line 7-8 they state that "for the sake of simplicity, we assumed that the shape factor nu is equal to 1."

In case nu=1, Eqn. (2) becomes a simple exponential function exp(-ak). Further derivation yields G(0)=g_0/a (integrating over k from 0 to infinity), and dG/dt = G/(a+t), as in their Eq. (4). This derivation was also carried out by Boudreau and Ruddick (1991); they even went a step further, derived and described the decay of organic matter as a nu'th order of organic matter concentration: dG/dt=-nu/(a*G(0))*Gˆ(1+1/nu) (their eqns 41 and 42). Thus, nu=1 results in a second order process, with dG/dt=1/(a*G(0))*Gˆ2, which would be easy to implement in any model, the restriction being the assumption of a closed system, and 0 =< k =< infinity.

However, on page 5, lines 17ff the authors rightly note that this continuous form is difficult to apply in a global model, because of the many processes that can distort the surface distribution on its way through space and time. Therefore, they seem to discretize the lability spectrum in 15 classes for each POC component, resulting in a multi-G model of particle lability, where the boundaries of the lability spectrum are defined by -ak_(i+1) and -ak_i, and the fraction each class occupies in the reactivity continuum is given by Eqn. (6), namely G(bar(k)_i,0)= exp(-ak_(i+1))-exp(-ak_i). (All assuming nu=1, i.e. Gamma(1) = 1).

Then, on p. 6, line 12 the authors "that the lability distribution of POC is insensitive to ocean transport and is only modified by sinking, by the biological sources and sinks, and by vertical mixing in the mixed layer.", thereby reducing the model to a 1D system. Based on this assumption, they then exchange the time axis by z, via t=z/w, and solve the system analytically. For nu=1, the outcome will then be:

G(k,t) = g_0 * a/(a+f(T)*z/w)*(exp(-(a+f(T)z/w)k_(i+1))-exp(-(a+f(T)z/w)k_(i))

Provided I understood everything correctly, my questions are now:

(1) What exactly is the difference between this model, and a model that uses several (15) discrete lability classes at the surface, whose remineralization is distributed instantaneously over depth according to their constant remineralization rate and sinking speed? Of course, by choosing the Gamma function for the distribution over the lability space, and hence their average lability, it avoids the necessity to select for many different parameters (this choice to went to assigning "a" and nu). It is difficult to determine differences this from Eqn. (7) alone, as neither sources nor sinks are specified; it also seems that the indices for the different lability classes are missing. I suggest to extend on this part, and shorten the one about the gamma function (see 2), or (this relates to the nu=0.16 experiments) comment on the way the gamma function was computed (if necessary).

(2) Why not start explaining the method before the background of nu=1, and refer to Boudreau and Ruddick (1991) for a complete (and in my opinion very thorough) review of the merits and pitfalls of the incomplete gamma function to represent the lability spectrum of organic matter? As you rightly note, math becomes much easier with nu=1, and the paper will be more susceptible for a greater audience. On the other hand, I would suggest to be much more specific about the coupling of the lability spectrum to biogeochemistry: many parameters and variables are not defined and explained (a, g_0, S, P). Decay is denoted both in terms of k_i, bar(k)_i, f(T), bar(lambda) ... This is very confusing, particularly as the model is all about decay.

(3) How do you arrive at Eqn. (4)? It seems to result from a Laplace transform; if so, I would mention it or refer to some source (again, Boudreau and Ruddick seem to be a good source). This also applies to eqns. (9) and (10).

(4) It seems that one of the assumptions if the discretized model is that particles within a lability class do not change their properties over time (or space). If so, I would mention

it briefly.

(5) The analytic integration over depth requires that w=const., correct? I suggest to state this briefly; this is in contrast to the common notion that particles become faster with depth, and might help to distinguish this model from other models making that assumption.

Results:

The good fit to observed POC profiles (Fig. 3-6) is really impressive at first sight. To make it even more convincing, you could explain what exactly sPOC and bPOC is (Fig. 5, 6). I also wonder whether such a good fit could be obtained using the standard 2-component PISCES model, with different/tunes decay rates for the small compartment. Did you ever test this?

Figure 7, lower panels: please choose a different colour scale. It it very difficult to see the dots for observations. (At first sight I thought there were no observations at all.) Further, a plot such as Fig. 3 of Gehlen et al. (2006) would be very nice - I know that comparison to trap data always shows a huge scatter, but it might nevertheless help the reader form an opinion about model performance ("Has anything improved?")

Figure 8 looks very impressive. Did you also compare against the data set by Seiter et al.? As far as I know, the data are available, and a comparison against this data set possibly includes less assumptions (stoichiometry; interpolation).

Figure 9 and p. 12, lines 13ff: How did you compute nu from the RC model? As the model's assumption was nu=1, where do these variations come from? Beside variation in nu, Boudreau and Ruddick found a huge range of "a"'s, from months to thousands of years. If applicable, did you try the same approach for "a" (perhaps better: apparent nu and apparent "a", in contrast to the prescribed "a" and nu)?

Discussion:

The discussion repeats some things that were already presented in the method description. Then, several other experiments (nu=0.16, p. 12; 9 lability classes, modeled both implicitly and explicitly) and model - data comparisons ("apparent" nu, Fig 9 and p. 12; iron distribution, p. 15 and Fig.) are are presented or mentioned briefly. I suggest to move these to the results section, and also explain their setup in the methods section. How was the nu=0.16 experiment computed? Did it require numerical evaluation of the incomplete gamma function, and if so, how expensive was these in terms of computation time?

Given the quite large uncertainties associated with the parameterization of the iron cycle (Tabgliabue et al., 2015; 10.1002/2015GB005289) I find a comparison against dissolved iron concentrations quite ambitious; nevertheless, I could not see any observations of iron in Fig. 12 (Does "vertical difference of the difference the simulated iron distribution" mean "difference between model and observations"?) Do simulated nutrients or oxygen differ in the same way as iron does?

Finally, there have been two recent papers that attempt to simulate the degradation of particles, in local or global models, namely Yokulsdottir and Archer (2016, www.geosci-model-dev.net/9/1455/2016/) and deVries et al. (2014, www.biogeosciences.net/11/5381/2014/) - perhaps some discussion about the assumption of this model in contrast to these (and possibly others) might encourage the reader to dive deeper into this very interesting topic.

Specific comments:

- p. 1, line 13: The particle flux profile introduced by Suess is not an exponential one, but is described by 1/(a*z+b)

- p.6, Eqn (7) What are the P-term and S-term exactly?

- p.6., line 20 "Assuming constant sources and sinks over each grid cell" - constant over what? Time?

- Table 1: The number of lability classes refers to each POC compartment, correct? Could you give the value for "a" for experiment RC? Possibly also the range of k_i (i.e. the lowest and highest value)?

- Table 2: It seems to me as if the headers (RC, noRC) are swapped. Please comment on the different acronyms for the metric: instead of "B" one could also write "bias". It is quite inconvenient to search through the paper for RI and MEF.

- Table 3: What exactly is POC production - production of fecal pellets? Primary production (as in the table header)? What is Phi? I assume sedimentation through a depth horizon of at the sea floor - but this should be clearly stated in the caption.

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