Response to Reviewer 2

We would like to thank the reviewer for the interesting discussion on our manuscript. In the following response, we have carefully studied the comments and have made corrections which we hope to meet with approval.

The reviewer starts by a detailed summary of the method we have used to describe the lability distribution of particulate organic carbon. One of the concerns of the reviewer is whether he understood correctly the methodology of the manuscript. Based on the detailed summary he provided in his review, we confirm that he correctly understood our methodology.

Method:

1) **What exactly is the difference between this model and a model that uses several (15) discrete lability classes** We are not really sure to fully understand what the reviewer suggests in this comment. Our parameterization is, as mentioned by the reviewer, based on a discretization of the lability space using 15 classes. The number of classes is not a strict constraint and can be freely specified when the experiment is set up. The remineralization rate of each class is constant over the water column by definition. It is also constant over the entire ocean. Sinking speeds are also constant. Thus, in theory, the vertical distribution of each class could be computed easily from the surface distribution. Concentrations would decrease as an exponential function of depth with a length scale defined as the ratio of the sinking speed over the remineralization rate. However, particles are continuously consumed and produced over the entire water column. In addition, particles are exchanged between the small and large pools which have different lability distributions. Thus, predicting the vertical profiles of each lability pool is not feasible. In the revised version of the manuscript, a new figure will be added which compares remineralization rates predicted when a closed system is assumed (basically when the distribution of each lability class can be analytically determined from surface values) to remineralisation rates predicted by the standard `RC` experiment. This figure is displayed as Figure 3 in our response to John Dunne's review. Following the reviewer's suggestion, we have extended the description of the parameterization in section 2.2 of the manuscript. We also better describe the different terms in Equation 7. Indices are also now explicitly written in that equation. The gamma function is computed using the algorithm of MacLeod (1989).

2) **Why not start explaining the method before the background of nu = 1, and refer to ...** We have removed from the model description the assumption that ν is equal to 1. In fact, this assumption was confusing since the description and the equations of the submitted version of the manuscript were not relying on that assumption. The parameterization we developed for this study can use any value of ν. As suggested by the reviewer, rewriting the equations with the assumption $\nu = 1$ would make the maths much easier but it would also make the parameterization less general. Furthermore, in the discussion section, we mention an experiment in which ν has been set to 0.16 which would be difficult to explain if our parameterization were based on the assumption $\nu = 1$. As suggested by the reviewer, we have carefully defined all the notations in section 2.2 of the manuscript.

3) **How do you arrive at Eqn. (4)? It seems to result from a Laplace transform; ...** No, it does not result from a Laplace transform. It stems from an integration by

parts of the degradation equation:

$$\frac{dg(k,t)}{dt} = -kg(k,t) \tag{1}$$

The explanation of the integration can be found in Boudreau and Ruddick (1991) and Boudreau et al. (2008). This is now mentionned in the revised version of the manuscript.

4) It seems than one of the assumptions if the discretized model is that particles within a lability class do not change their properties over time (or space) ... We are not sure to fully understand what the reviewer means here. In our model, particles are a mixture of compounds with different labilities. This mixture evolves with time in response to different processes such as degradation, grazing, coagulation, ... Thus, the composition of the particles evolves with time and is not constant in time and space. In our model, the lability spectrum corresponds to the compounds which make up particles. In other words, particles are not assigned a constant lability but are made up of compounds which have each a constant lability and whose proportions in particles are changing with time and space.

5) The analytic integration over depth requires that w=const., correct ? ... In fact, a constant sinking speed is not required by our parameterization. Vertical profiles of the lability distribution is obtained by analytically integrating Equation 7 over each individual grid cell. This requires that properties such as sinking speed, production, consumption of particles are spatially constant inside a grid cell. But, they do not need to be constant over the whole water column. In preliminary experiments, we were in fact using a sinking speed that was increasing with depth for large POC. However, we feel that such an increase is not clearly demonstrated by observations and we thus decided to keep sinking speeds constant with depth. We discuss that in details in our response to John Dunne's review (see point 4 of our response to John Dunne). In the revised version of the manuscript, we clearly state that sinking speeds do not need to be constant with depth. However, they should be assumed constant within a grid cell.

Results:

The good fit to observed POC profiles is really impressive at first sight. To make it even more convincing, you could explain what exactly sPOC and bPOC are ... The definitions of sPOC and bPOC are now given in the description of PISCES in section 2.1. Furthermore, the nominal cutoff between the two size classes is also specified. We don't think that such a good fit can be obtained by properly tuning the parameters of small POC. Improving the fit requires to increase the sPOC concentrations in the interior of the ocean. The sinking speed of sPOC cannot be changed because it is constrained by its (small) size. Thus, the only remaining way is to decrease remineralization rates. However, in the standard version of PISCES, remineralization rates are by definition constant over the global ocean (except for the effect of temperature). Thus, decreasing the remineralization rates at depth necessarily implies to decrease them also in the upper ocean. Based on the `RC` experiment, remineralization rates have to be reduced by one to two orders of magnitude. In the upper ocean, this would result in an increase in sPOC by about
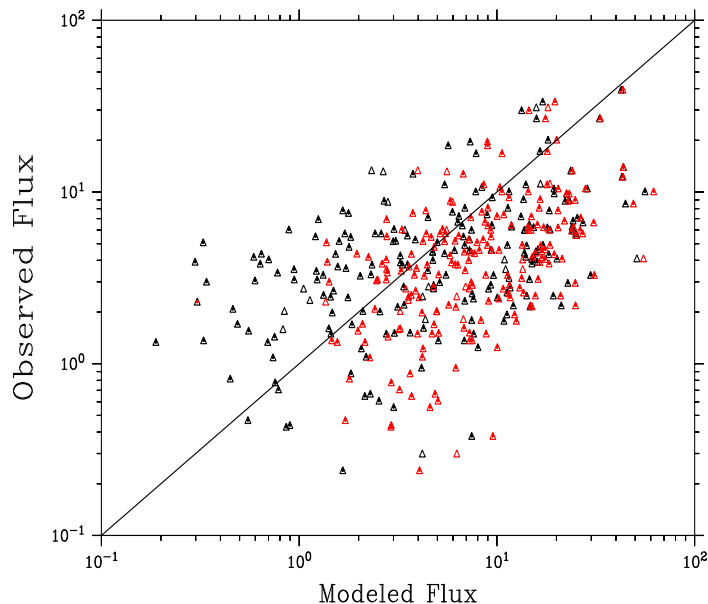
Figure 1: Comparison between POC fluxes $(\mathrm{mg\,C/m^2/d})$ estimated from sediment traps and modeled fluxes. Black and red triangles denote the `NoRC` and the `RC` experiments respectively. Data are from Dunne et al. (2005), Gehlen et al. (2006) and Le Moigne et al. (2013).

the same magnitude (at least if the model is not too non-linear). sPOC would have concentrations comparable to DOC which is not observed.

Figure 7, lower panels: Please choose a different colour scale. It is very difficult to see the dots for observations. We have redrawn that figure in which the observations should be easier to see now. Furthermore, we now provide with the manuscript supplementary materials in which we provide a figure similar to Figure 3 of Gehlen et al. (2006). This figure is quoted now in the result section of the manuscript (Figure S2). The figure is provided in this response (see Figure 1).

Figure 8 looks very impressive. Did you also compare against data set by Seiter et al? In the submitted version of the manuscript, we did not compare our model result to the quoted data set. We have tried to retrieve those data but they don't seem to be publicly available, at least the dataset which has been used to draw Figure 7 in Seiter et al. (2005). It would be of great value if this dataset could be made available to the community.

Figure 9 and p. 12, lines 13: How do you compute nu from the RC model? As the model's assumption was nu=1 ... In our model, we make the assumption that ν is equal to 1 in freshly produced organic matter. However, we do not constrain the lability distribution in POC to follow a gamma distribution with a shape factor of 1. The lability distribution evolves with time and space in response to sources and sinks of POC. In fact, the ν displayed on Figure 9 is the apparent ν of the lability

distribution. It is computed from the actual lability distribution of POC using the mean and the variance. In a gamma distribution, ν is equal to the square of the mean divided by the variance. As stated by the reviewer, Boudreau and Ruddick (1991) found quite huge variations of a (see Figure 6 in Boudreau and Ruddick (1991)). However, these values were obtained from sediment data. They explain these huge variations by the different sedimentation velocities: As sedimentation velocities decrease, the fast decaying part of the organic matter is removed before being incorporated in the historic part of the sediments. Thus, values of a being deduced from sediment observations cannot be directly used in the oceanic domain. In the revised version of the manuscript, we describe the computation of the apparent ν in the legend of Figure 9 as well as in text on page 12.

Discussion

The discussion repeats some things that were already presented in the methods description. Then, several other experiments (nu = 0.16, p12 ; 9 lability classes, modeled both implicitly and explicitly) ... These additional experiments have been performed to test the robustness of our assumptions. The experiment with $\nu = 0.16$ has been designed to test if the value of the shape coefficient in the open ocean can be as low as some of the values inferred from sediment data. The experiment with 9 lability classes has been designed to make a comparison with a simulation in which the 9 lability classes are explicitly modeled. All these experiments support the discussion presented in the discussion section whose objective is to analyse the potential limitations of our study. In the result section, we rather show the potential impacts a variable lability of organic matter could have on the ocean carbon cycle. In that sense, the additional experiments do not bring any support to those objectives of the result section. That is why we think these experiments should be presented and discussed in the discussion section. As mentioned above, the lower incomplete gamma function is computed in the model using the algorithm proposed by MacLeod (1989). This algorithm is very efficient and the additional cost is minimal. Furthermore, the incomplete gamma function is only computed at the beginning of the simulation to define the lability classes (the $\bar{k}_i$ classes).

Given the quite large uncertainties associated with the parameterization of the iron cycle, I find a comparison against dissolved iron concentrations quite ambitious ... We would like to thank the reviewer for this comment. In fact, the legend of Figure 11 in the submitted version of the manuscript is incorrect. It displays the average of the difference between the simulated iron distributions and the observed iron distributions for both experiments. In the revised version of the manuscript, we have changed the caption of this figure. For nitrate, as explained in the submitted version of the manuscript, changes are much smaller. For oxygen, differences are more significant but do not exhibit the same vertical distribution as for iron. The anomalies occur deeper in the water column and extend over a much larger vertical extent. This is not surprising because oxygen and iron experience different processes. In particular, in the interior of the ocean, the iron distribution is quite strongly constrained by scavenging and the ligands distribution (at least in our model) which do not affect oxygen. In response to John Dunne's comment (reviewer 1), the discussion on oxygen and nitrate has been slightly more detailed in the revised version of the manuscript.

There have been two recent papers that attempt to simulate the degradation of particles in local or global models ... We would like to thank the reviewer because we were not aware of the first study. In that study by Jokulsdottir and Archer (2016), a detailed model of particles is being designed. It relies on a stochastic lagrangian description of particles. Many processes are explicitly described and modeled including aggregation, fragmentation by zooplankton, ingestion by zooplankton, bacterial degradation. A ballast effect is also represented through its impact on the density of the aggregates. This detailed model also makes the assumption that aggregates are composed of different compounds with distinct labilities, in a manner similar to our approach. As a consequence, when the aggregates age, their degradation rate decreases. However, in their study, they don't explore the impacts of this varying lability on the POC distribution and on the POC fluxes. This prevents a direct comparison to our approach and results. Their model is also very complex and its implementation in a classical biogeochemical model should prove to be quite difficult which is mentioned in the conclusions of their study. They suggest that a required step is to significantly simplify their model. They envision to use their model or a simplified version of it to study the transfer efficiency of POC in the mesopelagic domain and potentially to analyse the variations of this transfer efficiency as a function of environmental and biogeochemical parameters. If this study is done, it should prove to be very useful to better understand and constrain the processes that control the fate of POC in the ocean.

The reviewer mentions a second study performed by DeVries et al. (2014). In their work, they develop a more classical model of particles that solves the evolution of the size distribution of particles with depth. They explore the sensitivity of their model to different parameters: size distribution in the upper ocean, sinking speed, mass-volume relationship, ... Their conclusion is that observed fluxes can only be reproduced when a protective ballast is modeled. This is not very different to some extent to the conclusions of previous studies (Armstrong et al., 2001; Francois et al., 2002; Gehlen et al., 2006). They also mention that an alternative explanation to the ballast hypothesis could be the existence of more refractory compounds in organic matter such as polyphosphates (Diaz et al., 2008). This alternative explanation is compatible with our study which assumes that organic matter is made of various compounds of which some are quite recalcitrant. However, two points are missing in their study which prevents insightful comparison with our work. First, they don't display and analyse the POC distribution in the water column. Second, the contribution of small particles to the total flux in the deep ocean is very small in DeVries et al. (2014), much smaller than what is suggested by observations (Durkin et al., 2015). Unfortunately, the authors don't show this contribution for their ballast experiment. Thus, it is impossible to evaluate if their improved parameterization improves the contribution of small particles to the total flux of POC in the interior of the ocean.

In the revised version of our manuscript, we now quote these two studies and briefly discuss their results and their implications for our work.

Specific comments

p1, line 13: The particles flux profile introduced by Suess is not an exponential one ... The reviewer is correct. We have corrected this in the revised version of the

mansucript and we quote now proper references: (Lutz et al., 2002; Boyd and Trull, 2007).

p6, Eqn(7) What are the P-term and the S-term exactly? This point was also mentioned in the reviewer's main concerns. Some terms were not properly defined in our submitted version of the manuscript. This is now the case in the revised version. P is the production of POC and S is the sink of POC.

p6, line 20 "Assuming constant sources and sinks over each grid cell" constant over what? Time? This point was also mentioned by the reviewer in his main concerns. These lines have been extended to better explain the method (see our response to the reviewer's main concerns). We assumed that sources and sinks do not vary spatially within a grid cell. In other words, we assume that sources and sinks are homogeneous within each grid cell of the model. But they do change with time. The text is now: *"Assuming spatially constant sources, sinks and sinking speed within a grid cell (i.e., sources, sinks and sinking speeds are homogeneous within each grid cell), this system can be solved analytically over each grid cell."*

Table 1: The number of lability classes refers to each POC compartment, correct? We have remade Table 1 which now displays more information. The values of k_i are given at the bottom of page 5 of the submitted version of the manuscript.

Table 2: It seems to me as if headers (RC, noRC) are swapped. Please comment on the different acronyms ... Caption of Table 2 has be extended to define now the acronyms. And the headers have been swapped.

Table 3: What exactly is POC production - production of fecal pellets ? What is Phi? ... Caption of Table 6 has also been significantly extended to better describe the different terms displayed in the table.

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