

## Response to Reviewer 1: John Dunne

We would like to thank Dr. John Dunne for his very positive and interesting comments on our manuscript. In the following response, we first address the four general concerns and comments of his review. In a second part, we answer to his more specific comments.

### What generates the regional variability in the transfer efficiency in Figure 11?

In Figure 11, we display the transfer efficiency of the export through the mesopelagic domain. In fact, there are several manners to diagnose the transfer efficiency. For instance, in Henson et al. (2012), two different definitions have been used: 1) the ratio of the export at 2000m over the export at 100m, i.e.  $T_{eff} =^{2000} F / ^{100} F$ , and 2) the value of the Martin's  $b$  coefficient. In our study, we have chosen the second definition. On Figure 11, we compare the annual mean anomalies of this Martin's  $b$  coefficient from the global median value. As discussed in the manuscript, part of the regional variations, i.e. the differences between the two experiments used in our study, is explained by the lability parameterization. However, in the standard experiment, which does not include the new lability parameterization, the  $b$  coefficient still exhibits very significant spatial variations.

These variations stem from three different dominant processes. First, zooplankton grazes upon particulate organic matter. In PISCES-v2 (the version of PISCES that is being used here), two types of grazing by zooplankton are represented. In addition to the conventional concentration-dependent grazing, POC is also consumed by flux-feeding (see Equation 29 in Aumont et al. (2015)). According to our model, flux-feeding dominates by far in the mesopelagic domain. To infer the impact of flux-feeding on the regional distribution of the transfer efficiency, we have performed an additional sensitivity experiment in which this process has been removed. Figure 1 shows the transfer efficiency computed from this experiment together with the transfer efficiency in the standard experiment. First, the global median value differs quite substantially from the standard experiment, i.e. 0.6 instead of 0.87. Second, the regional distribution is strongly altered. When flux-feeding is omitted, the  $b$  coefficient tends to be high in the subtropical gyres, close to the global median value in the productive areas of the low latitudes and very low in the high latitudes, especially in the Southern Ocean. Thus, flux-feeding plays a very important role in shaping the regional patterns of the transfer efficiency. In particular, it tends to strongly reduce the transfer efficiency in the very productive zones of the low latitudes.

The second process which explains the regional variations of the Martin's  $b$  coefficient is the relative contribution of the big particles to total POC in the upper ocean. A large contribution of these big particles tends to lead to a high transfer efficiency because their sinking speed is large and thus, their remineralization length scale is long. Conversely, a pool of POC dominated by small particles will tend to generate low transfer efficiency. This process explains the large values of the Martin's  $b$  coefficient in the sensitivity experiment presented in this response (see panel a) in Figure 1 of this answer). Finally, the third dominant process is temperature. The remineralization rate of POC is made a function of temperature in PISCES (see section 4.1.1 in Aumont et al. (2015)). Marsay et al. (2015) have proposed in their study a detailed analysis of the impact of temperature on the export of POC.

In the submitted version of the manuscript, these three processes are listed and discussed briefly in section 4.2 on page 14. We could provide a much more detailed analysis and add some figures such as Figure 1 of this review. However, since the

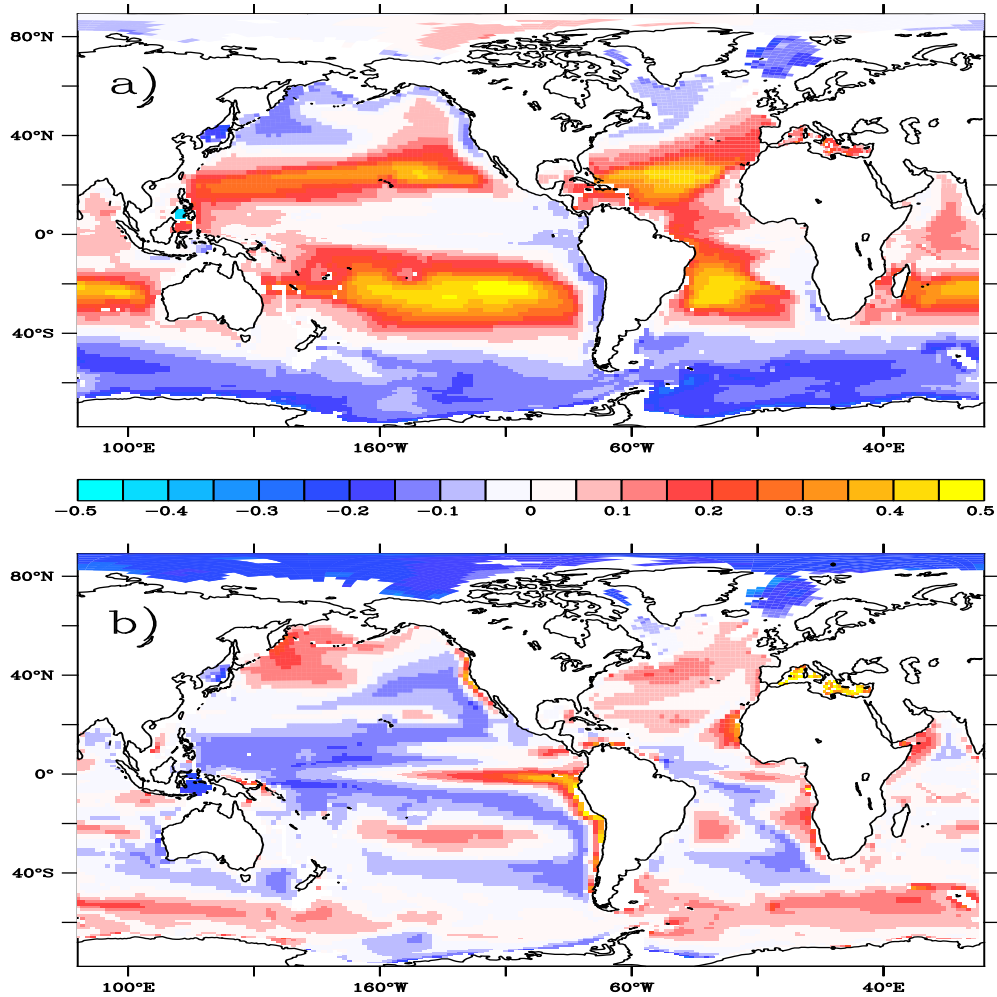


Figure 1: Spatial variations of the annual mean anomalies of the remineralisation coefficient  $b$  from the global median value: (a) from the new sensitivity experiment with no flux feeding, and (b) from the NoRC experiment. The global median values of  $b$  are 0.87 and 0.6, in the NoRC experiment and in the sensitivity experiment respectively.

primary focus of this paper is the impact of a variable lability on the distribution of POC and on the export of carbon, we believe it would distract the readers from this main focus.

**How good is the representation of transfer efficiency as a power in Figure 11? The authors should show some representative profiles of particles ...** Our main purpose on Figure 11 was to discuss the spatial patterns of the transfer efficiency in the mesopelagic domain and the impacts of lability on this transfer efficiency. As mentioned above, there are several manners to diagnose the transfer efficiency. We have chosen a diagnostic based on the Martin's  $b$  coefficient because of the widespread use of Martin's parameterization in ocean biogeochemical models. However, this does not mean that our predicted fluxes follow a power law function. Following the reviewer's suggestion, we have investigated in the RC experiment how close the predicted fluxes are to a power law distribution. To do so,

we have compared the predicted fluxes to reconstructed fluxes using the  $b$  coefficient diagnosed on Figure 11.

Figure 2 displays several statistical indicators of this comparison. The correlation coefficient is close to 1 over large regions of the ocean, especially in the low latitudes. In the high latitudes, it is lower. This would suggest that a power law function could be a good approximation of the simulated fluxes over large areas of the global ocean. However, the correlation coefficient in that case is not necessarily a good indicator of the fit as suggested by the other two indicators. First, the slope often diverges significantly from 1. Second, the normalized RMSE can be very large (above 2), especially in productive areas. In fact, the high value of the correlation coefficient stems from the general vertical shape of the vertical fluxes which decrease sharply with depth in a very convex manner.

As a consequence of that analysis, we think that a power law function is not a satisfactory approximation of the predicted fluxes in our study. Thus, the Martin's  $b$  coefficient displayed on Figure 11 should be interpreted as a diagnostic of the transfer efficiency in the mesopelagic domain, not as an attempt to describe the fluxes with a power law function. In the revised version of the manuscript, we added on page 13 a discussion on the use of the Martin's  $b$  coefficient: *“Figure 11 displays the anomalies of  $b$  relative to the median value of that coefficient, both in the NoRC and in the RC experiments. The  $b$  coefficient is used here as a diagnostic of the transfer efficiency of POC in the mesopelagic domain. In fact, a close inspection of the vertical profiles of the simulated vertical fluxes of POC shows that they can diverge significantly from a power law distribution, especially in the high latitudes and in very productive areas (see Figure S1, in the supplementary materials).”*

**Is the relationship between initial composition and final remineralization profile amenable to the creation of a numerically efficient metamodel ...** Computing cost is always an issue in global ocean biogeochemical models. New parameterizations often imply a substantial extra cost and any means to overcome this extra cost is beneficial. Furthermore, metamodels can be powerful and efficient tools to reconstruct fluxes from incomplete data, such as satellite data for instance. Thus, we agree with John Dunne that deriving a metamodel from our model experiments would be of a great value.

In this study, we have not attempted to derive such a metamodel as our primary objectives were 1) to investigate the impacts of a variable lability of POC on the distribution and vertical fluxes of POC, and 2) to attempt to reconcile realistic fluxes and POC concentrations. Nevertheless, is the construction of a metamodel feasible in that specific case? Unfortunately, we cannot give a certain answer to that question. As discussed in the manuscript and in the first item of this response, the vertical structure of the fluxes depends on the size structure of POC in the upper ocean (the relative contribution of big particles), the abundance and the vertical distribution of zooplankton and the vertical structure of temperature. The two latter points rely on 3-D fields which do not necessarily correlate well with upper ocean variables. For instance, the vertical structure of zooplankton is impacted in a non linear (and thus non simple) way by oxygen but also by the vertical structure of the concentrations and the fluxes of POC. This should make the construction of a metamodel quite challenging. Furthermore, as discussed in the second item and as displayed on Figure 2 of this response, the vertical fluxes of POC can significantly deviate from a simple power law function over large regions of the ocean. As a

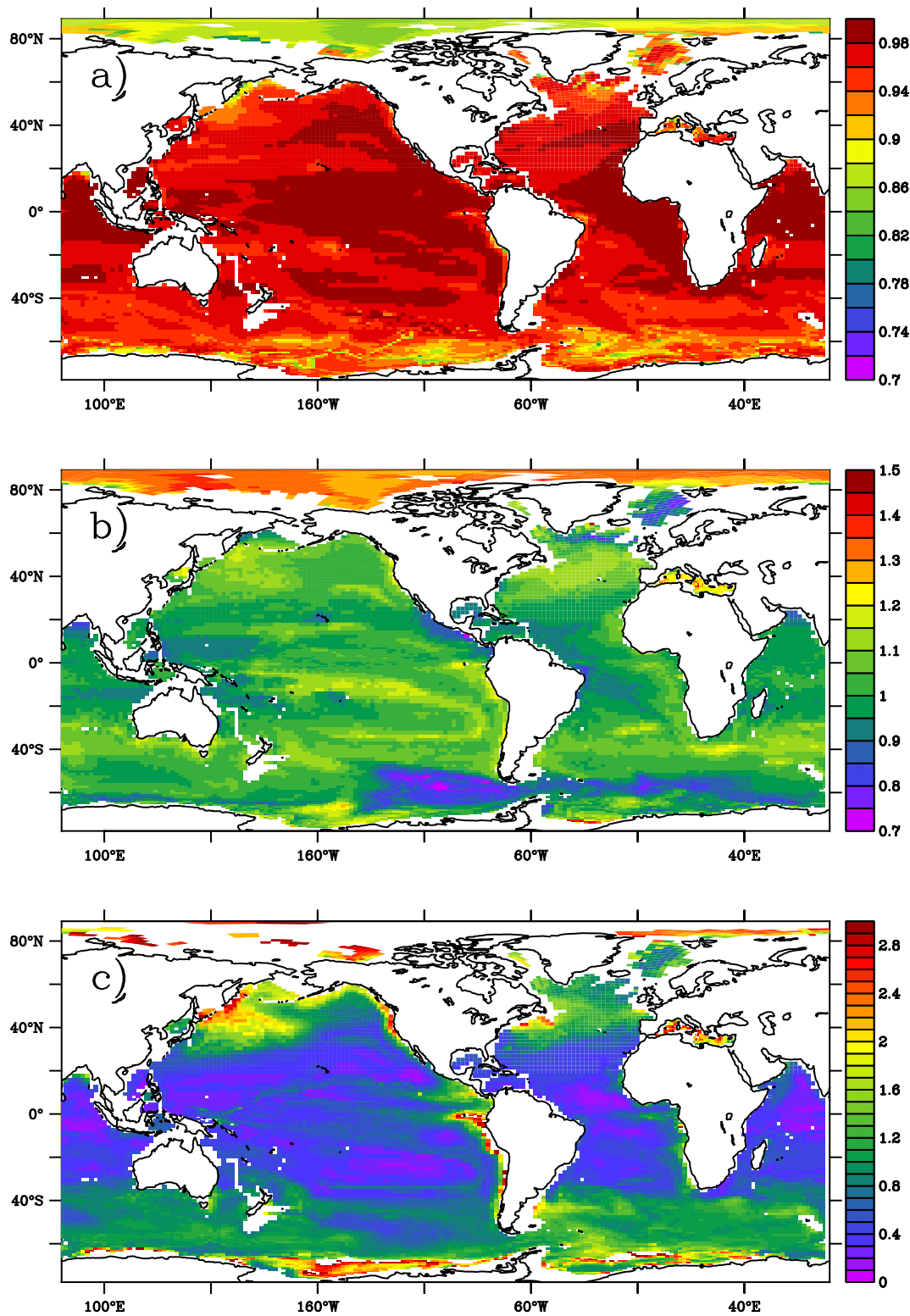


Figure 2: Statistical comparison between the simulated vertical fluxes of POC in the RC experiment and reconstructed fluxes using a power law distribution with the  $b$  coefficient displayed in Figure 11. Panel (a) shows the spatial patterns of the correlation coefficient  $r^2$ . Panel (b) shows the slope of the linear regression analysis. Panel (c) displays the normalized RMSE between both fluxes.

consequence, our feeling is that the construction of a robust metamodel should be difficult. A substantial additional analysis would be necessary which exceeds the primary objectives of our study.

Setting aside the construction of a metamodel, a related point is whether a more numerically efficient model can be constructed from our model. In our study, numerical efficiency has been an important issue which explains the quite strong assumption we made by neglecting the impact of advection and diffusion. This assumption avoids the explicit representation of the computationally intensive lability spectrum. As a consequence, the extra-cost of the lability parameterization is limited to about 20%. Despite being reasonable, this extra-cost is still significant. To considerably reduce this cost, one can be tempted to make the assumption of a closed system for POC. In that case, the model simplifies to Equation 4 of the manuscript. Figure 3 compares the vertical lability distribution of small POC using that strong assumption with the prediction based on the RC model. Differences are very large and can reach almost one order of magnitude in the interior of the ocean. Thus, the assumption of a closed system is not valid. The sources and sinks of POC in the interior of the ocean play a major role on the vertical and horizontal structure of the lability distribution.

In the revised version of the manuscript, we extend the discussion on the computation cost of our parameterization at the end of section 2.2 and add Figure 3 of this comment as Figure 2 in the revised version of the manuscript: *“The lability parameterisation introduces an extra cost of about 20%, but it depends of course on the number of lability pools. To further considerably reduce this extra-cost, one could be tempted to adopt the assumption of a closed system. In that case, the model simplifies to Equation 4 for both small and large POC. Figure 2 compares the vertical lability distribution of small POC using that strong assumption to the prediction using the complete lability parameterization. Differences are large and can reach almost an order of magnitude in the interior of the ocean. Thus, the assumption of a closed system introduces large errors. The sources and sinks of POC in the interior of the ocean play a major role on the vertical and horizontal patterns of the lability distribution.”*

**Does this result finally solve the challenge of distinguishing between the two hypotheses of increasing sinking velocity with depth and decreasing lability with depth?** Unfortunately, the answer to that question is no. In our model, vertical sinking velocities are constant and lability is decreasing with depth. Nevertheless, our feeling is that the most probable hypothesis is a decreasing lability with depth. As a clue to support that hypothesis (it is a clue, not a demonstration), the assumption of an increasing sinking velocity with depth implies that the vertical variations of POC could be described as a power law function with an exponent equal to  $-(b+1)$ . So POC concentrations should decrease quite strongly with depth in the deep ocean which is not supported by the observations (see Figure 3 of the manuscript for instance). An alternative would be that only the fraction of POC that contributes the most to the vertical fluxes, i.e. the big particles, decreases with depth. In that case, the relative contribution of the big particles should be decreasing with depth. Again, this is not supported by the observations (see Figures 5-6). As already stated, this does not demonstrate that mean sinking velocities do not increase with depth, but simply suggests that such is not case.

In the rest of this response, we address the more specific comments made by the

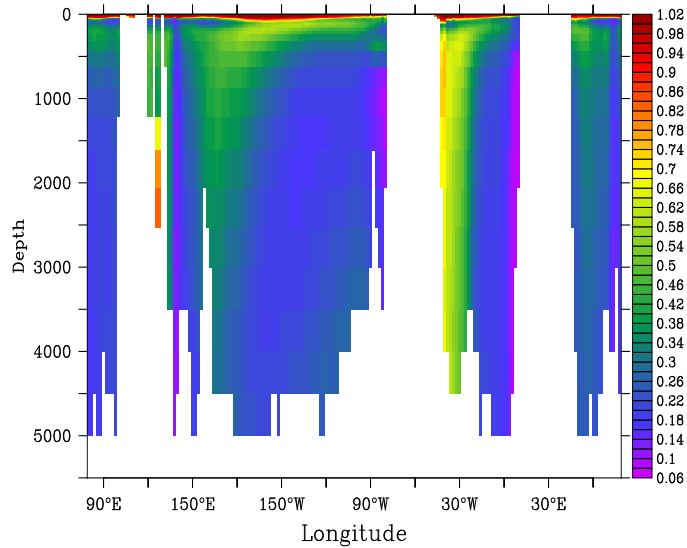


Figure 3: Vertical distribution along the equator of the ratio between the remineralization rate of small POC computed when the assumption of a closed system is made and the remineralization rate computed in the standard RC experiment.

reviewer.

**P3, ln 13** - “explicitly” should be added before ... Done.

**P4, ln 3, 10, and P6, ln 6** - “big” should be “large” This has been changed. The nominal size cutoff is  $100 \mu\text{m}$ . This indication has been added to the model description on page 4.

**P7, ln 14** - **Will Gardner has a database . Jim Bishop may have one as well.** We thank John Dunne for this information. We were not aware of that database. Following John Dunne’s advice, we have downloaded the database from the given website. In fact, this dataset is not based on POC observations but include beam attenuation observations. Then a regression relationship should be applied to derive POC concentrations from beam attenuation values. After inspecting the related publications (i.e., Gardner et al., 2006), the first issue we faced is that the relationship has been built mainly from surface data. For small values, typically measured in the interior of the ocean, the scatter is extremely large (see for instance Figure 3 in Gardner et al. (2006)) which makes the relationship not very robust, at least for the mesopelagic and deep domains. The second related issue comes from the absolute value given by the relationship. There is a minimum value given by the intercept of the regression between beam attenuation and POC and at least, for the Pacific ocean, this intercept is very high (larger than  $2 \mu\text{M}$ ), almost an order of magnitude larger than typical directly observed values. Figure 4 compares the mean vertical profiles of POC over the Pacific and Atlantic Oceans as derived from Gardner’s database to the profiles computed from the database shown on Figure 2 of our manuscript. They differ a lot which makes the use of both challenging.

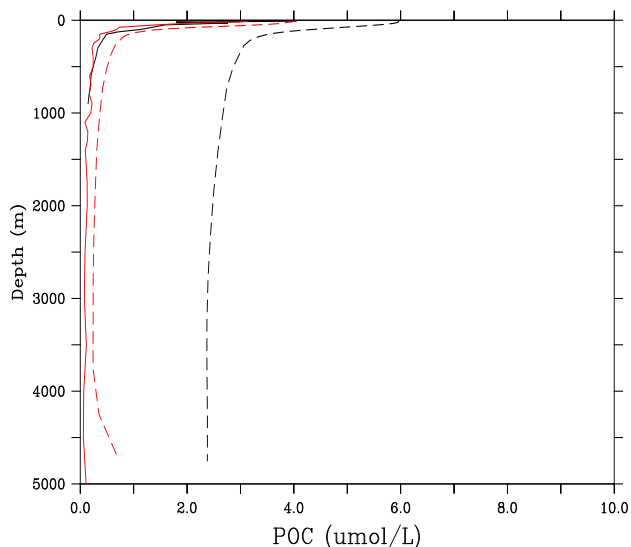


Figure 4: Vertical profiles of POC ( $\mu\text{M}$ ) averaged over the Pacific (black) and the Atlantic (red) oceans. Solid lines correspond to the data presented on Figure 2 of the manuscript. Dashed lines display the POC data reconstructed from Gardner’s dataset.

Furthermore, POC concentrations in the deep ocean reconstructed from the beam attenuation observations exceed in the Pacific Ocean those in the Atlantic ocean by almost an order of magnitude which is hard to explain by biogeochemical arguments. For those reasons, we prefer not to use Gardner’s database in our study. We have not been able to find any publicly available database from Bishop’s group.

**P7, ln 25 - The authors can also consult the Honjo dataset for 2000m values**  
 Again, we would like to thank John Dunne for his suggestion. In fact, most of the data in Honjo dataset were also included in the dataset from Gehlen et al. (2006). However, some were missing. Figure 5 has been redone to include those missing data.

**P8, ln 18 - “does represent” should “represents” ...** This has been changed.

**P8, ln 21 - What are the units of “0.1 to 0.4”?** There are no units since this is a relative contribution.

**P8, ln 24 - Remove “associates to a relatively strong remineralization”** We don’t think we should remove those words because the sinking speed by itself is not enough to explain the sharp decrease in POC with depth.

**P9, ln 29 - What are these modeled and observed C14 ages in conflict?** We do not model C14 in POC here. The observed C14 isotopic ratio in suspended POC suggests that this pool is quite older than sinking POC. This observation is in contradiction with the prediction of the standard version of the model which simulates a slow-sinking POC pool that is very young. We have changed this line to make our point clearer.

**P9, ln 15 - add “a” before “result”** Done.

**P9, ln 16 - add “variable” before “lability”** Done.

**P19, ln 19-20 - to make the case the model is good, one compares with observations ... one should show the  $r^2$  for both mods-obs and mods-mods** We agree with John Dunne that a comparison based on the value of  $r$  is not sufficient to assess the performance of a parameterisation. In particular, this does not tell if two different parameterizations produce significantly different results, especially if we restrict the computation to a model-data comparison. To overcome that limitation, we computed in the submitted version of the manuscript other statistical indicators relative to the observations. Since the performance of the two model configurations is very different, especially the scores based on the MEF and RI indices, the POC distributions should significantly differ between the two experiments. Following John Dunne’s suggestion, we have also computed the correlation coefficient between the RC and NoRC experiments which is equal to 0.98. Thus, it is very high which would suggest that both model configurations produce very similar results in terms of POC distribution. However, in that specific case, the correlation coefficient is not necessarily the best index (see our response to general comment 1). When we compute the RI index, its value over the global ocean is 14.6 which indicates on the contrary that both models strongly differ. We did not change the manuscript because we think that the different statistical indices provided in our study are sufficient to prove that the two different models produce POC distributions that are significantly different, following the recommendations given by Stow et al. (2009) and Doney et al. (2009).

**P9, ln23 - What is the third simulation “in all three simulations”** This is a mistake. There are only two simulations. Three has been changed to two.

**P10, ln 1-10 - The authors need not be concerned ...** We deeply thank John Dunne for his detailed analysis of the estimates provided by Henson et al. (2012) and Guidi et al. (2015). We were not aware of the recent study by Weber et al. (2016). This study clearly challenges the transfer efficiencies found in Henson et al. (2012) and Guidi et al. (2015) and suggests larger export to the deep ocean. The horizontal patterns of this transfer efficiency seem also in qualitative agreement with what Marsay et al. (2015) found. We will change the text in section 3.2 to include a discussion on the potential biases in the estimates of Henson et al. (2012) and Guidi et al. (2015).

**P11, ln 30 - “relies on” should be “tests”** Changed.

**P12, ln 28 - what is the small particle ages in these simulations?** We don’t really understand that question as the ages of the particles are not mentioned here. Nevertheless, the ages of the small particles in the NoRC experiment never exceeds a few weeks. In the RC experiment, the ages of these small particles exceed 5 years at the bottom of the ocean.

**P12, ln 5 - Again, don’t just trust the Henson’s numbers** The numbers of the  $b$  coefficients found in the studies by Henson et al. (2012) and Guidi et al. (2015) are only used to illustrate that this coefficient should not be considered constant.

**P13, ln 2 - Some discussion of the modeled ecological factors driving the regional variability in transfer efficiency is warranted** Please see our detailed answer to the first general comment.



**P15, ln 10-14 - Provide the r2 or RMSE comparison for these runs** Following John Dunne’s suggestion, we have computed the RMSE of the nitrate and oxygen distributions between the two model experiments. They are equal to 0.85 and 17.4  $\mu\text{M}$  respectively. The text has been changed accordingly to quote these values.

**P16, ln 10 - It would be extremely helpful for the ocean biogeochemical modeling community ...** Please see our response to the third general comments made by the reviewer.

**Figure 4 - This should be combined with Figure 3 into a single figure** Done.

**Figure 6 - This should be combined with Figure 5 into a single figure** Done.

**Figure 11 - Missing color bar** In the submitted version of the manuscript, the colorbar does not seem to be missing on figure 11.

## References

- Aumont, O., Ethé, C., Tagliabue, A., Bopp, L., and Gehlen, M.: PISCES-v2: an ocean biogeochemical model for carbon and ecosystem studies, *Geosci. Model Dev.*, 8, 2465–2513, doi:10.5194/gmd-8-2465-2015, 2015.
- Doney, S. C., Fabry, V. J., Feely, R. A., and Kleypas, J. A.: Ocean Acidification, *Annu. Rev. Mar. Sci.*, pp. pp. 169–92, doi:10.1146/annurev.marine.010908.163834, 2009.
- Gardner, W. D., Mishonov, A. V., and Richardson, M. J.: Global POC Concentrations from in-Situ and Satellite Data, *Deep Sea Research Part II: Topical Studies in Oceanography*, 53, 718–740, doi:10.1016/j.dsr2.2006.01.029, 2006.
- Gehlen, M., Bopp, L., Emprin, N., Aumont, O., Heinze, C., and Ragueneau, O.: Reconciling surface ocean productivity, export fluxes and sediment composition in a global biogeochemical ocean model, *Biogeosci.*, 3, 521–537, doi:10.5194/bg-3-521-2006, 2006.
- Guidi, L., Legendre, L., Reygondeau, G., Uitz, J., Stemmann, L., and Henson, S. A.: A new look at ocean carbon remineralization for estimating deepwater sequestration, *Global Biogeochem. Cy.*, 29, 1044–1059, doi:10.1002/2014GB005063, 2014GB005063, 2015.
- Henson, S. A., Sanders, R., and Madsen, E.: Global patterns in efficiency of particulate organic carbon export and transfer to the deep ocean, *Global Biogeochemical Cycles*, 26, GB1028, doi:10.1029/2011GB004099, 2012.
- Marsay, C. M., Sanders, R. J., Henson, S. A., Pabortsava, K., Achterberg, E. P., and Lampitt, R. S.: Attenuation of sinking particulate organic carbon flux through the mesopelagic ocean, *Proceedings of the National Academy of Sciences*, 112, 1089–1094, doi:10.1073/pnas.1415311112, 2015.
- Stow, C., Jolliff, J., Jr., D. M., Doney, S., Allen, J., Friedrichs, M., Rose, K., and Wallhead, P.: Skill assessment for coupled biological/physical models of marine systems, *J. Mar. Syst.*, 76, 4–15, doi:10.1016/j.jmarsys.2008.03.011, 2009.
- Weber, T., Cram, J. A., Leung, S. W., DeVries, T., and Deutsch, C.: Deep Ocean Nutrients Imply Large Latitudinal Variation in Particle Transfer Efficiency, *Proceedings of the National Academy of Sciences*, 113, 8606–8611, doi:10.1073/pnas.1604414113, 2016.