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Interactive comment on "The effect of marine aggregate parameterisations on global biogeochemical model performance" *by* Daniela Niemeyer et al.

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First of all, we would like to thank the reviewer very much for his/her thoughtful and constructive comments. Below is our detailed reply.

The authors present a sensitivity study of the impact of particle aggregation on the global performance of a biogeochemical model, with a large focus on the improvement of the representation of the OMZs. While I enjoyed the reading and I think that the model description and sensitivity analysis is a significant step forward in the field, I have several general comments that should be addressed before publications.

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Discussion paper

Title: The title is too broad and I think that the mentioned to OMZs should appears somewhere because most of the sensitivity analysis is directed toward the improvement of their representation (even though O2 is not the only tracer considered).

Thank you for this advice. We added 'and with a focus on oxygen minimum zones' to the title. Our revised manuscript is now entitled 'The effect of marine aggregate parameterisations on global biogeochemical model performance and with a focus on oxygen minimum zones'.

Particle sinking speed: The introduction refers to a large range of particles sinking speed as a function of size (which is true) spanning from 10 to 386m.d-1 (or more). I would have liked to see in the paper (at least in the results or discussion) how the model sinking speed scales with actual data (or sinking speed from other models). For example, page 5, line 18, the minimum sinking speed is mentioned to be between 7 to 2.8m.d-1 for particles of 0.002cm but what are the maximum values? Figure S5 shows a latitudinal section of mean sinking speed of detritus at both 100 and 500m. Values range between 0 to 600m.d-1 and 350 to 850m.d-1 at 100 and 500m respectively which are relatively high compared to whatever has been measured and published in the literature (ex. Jouandet et al., 2011, figure 8). Having this discussed in the paper would be a plus for the validation of the model.

Thank you for this important comment. Please find in Table 1 column 6 the maximum possible sinking speeds for each simulation, which is defined by wL=w1*(dL/d1)ˆeta. Although the maximum prescribed sinking speed depends on parameterisation and has a broad range between 33 m d-1 (porous particles) and 4027 m d-1 (dense particles), our best simulations with regard to JRMSE and JOMZ range between 101 m d-1 (#17) and 51 m d-1 (#26), which is in line with the findings by Alldredge and Gotschalk (1988), Nowald et al. (2009) and Jouandet et al. (2011). Unfortunately, we made a mistake in converting our fluxes in Figure S5, which is now Figure 7 in the revised manuscript, resulting in diagnosed sinking speeds, which were too high by a factor of approximately 16. Figure 7 now shows the correct diagnosed sinking speeds (note that the scale of the y-axis has changed). This leads to a maximum sinking speed of 50.7 m d-1 at 500 m depth, which now agrees with column 6 in Table 1 and previous observations. Because of this mistake in converting diagnosed fluxes and properties, we also had to adapt Fig. 1c, f and i showing the export rates. These are now also lower by a factor of 16. However, in both cases the global pattern remains. At this point, we would also like to note that the sinking speeds shown in Fig 7 are the average speed across the entire size spectrum, i.e. not directly comparable to sinking speeds of individual particles or aggregates.

Particle length scale b: The author acknowledges that b values in their models (the one including aggregation) is much larger than in most empirical studies (page 13 L13-15). This is an important limitation of the model and its ability to represent the extension of the OMZs. I would have like to see a better discussion on this limitation and in particular how far their bs are from empirical observations. For example, values from Marsay et al., 2015 and Guidi et al., 2015 both range between 0 and 2 even though showing different patterns. The current study present bs ranging from 1 up to 4 (Figure 1g) with different amplitude and absolute values and therefore important implication for both the horizontal and vertical representation of the OMZs. One explanation from the author is that the model generates too many small particles because processes such as repackaging, egestion and others are not represented. This could be true but the model also generate large aggregates (up to 4 cm) and sinking average sinking speed (see above) are fairly high. So, these 2 results are inconsistent and I would have like to see a mode developed discussion about this in the article.

Thank you for your advice. DL is defined as the maximum diameter for size dependent processes and aggregation and thus it doesn't describe the maximum diameter of all the particles in the size spectrum. In our revised manuscript, we defined this parameter more clearly in our methods and discussion. However, the simulated abundance of large particles with a diameter > 4 cm is low and the ability of recording or collecting these large particles is limited, as e.g. the upper size limit of particles that can be

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measured by the UVP 5 is 26 mm. According to our model assumption, the particle distribution always covers the entire size range from an individual small particle to infinity. (Please note that in our model concept very large particles are extremely sparse, and, because of density decreasing with increasing size, consist almost entirely of water.) Large values of b are associated with a pronounced dominance of small particles and low sinking speeds. As described above, there was a mistake in computing effective sinking speeds in the original manuscript, leading to diagnosed sinking speeds about a factor 16 too high. Although the diagnosed sinking speed is now corrected and generally agrees with previous observations, the particle length scale b is, in regions such as the subtropical gyres, still higher than in observations. Especially in the subtropical gyres, we find a too steep particle size spectrum i.e. too many small particles. Thus, more processes affecting the particle size spectrum, e.g. vertical migration of zooplankton or particle breakup in the deeper ocean, might be necessary. Moreover, it should be noted that our particle length scale, b, is calculated by a simple regression using the log-transformed flux and depth. As the aggregation model shows an increase of average sinking speed to a depth of 1,000 m, the calculated particle flux length covers a vertical range of 100 to 1,000 m and thus does not necessarily correspond to the observed depth ranges. However, Marsay et al. (2015) showed that the considered depth range seems to be important for the comparison of the particle flux length. A mismatch of considered depth ranges can thus constitute a potential factor for deviations of b values in our model compared to observations. We now have extended the discussion on the divergence of simulated and observed particle flux length scales, and on potential processes that might explain this divergence.

Particle size distribution (slope): There is no comparison of the model size spectrum (slope at least) to actual in-situ measurements of particle size distribution which are increasingly available in the literature (ex. Kiko et al., 2017). I would have like to see this comparison in the paper to present evidence that the dynamic of particle aggregation is well capture by the model before to perform any sensitivity analysis.

Thank you for your comment. Please find in our new Figure S5 the number of particles with a size range of 0.14 to 16.88 mm, equal to the range of Kiko et al. (2017). The comparison to the observed transect in the Atlantic equatorial region of Kiko et al. (2017) (in their Figure 1) shows an underestimation of particle concentrations in our model in the surface layer as well as over the full water column. As the model calibration on observed particle data is currently underway, we hope to further improve the fit of particles between model and observations. In our revised manuscript, we compared our model data with the observations by Kiko et al. (2017).

Specific comments Page 3 L 15 and L 20: Are you referring to Marsay et al., 2015 and Guidi et al., 2015 or Henson et al., 2015 and Marsay et al., 2015 as stated?

Thank you for this comment. Marsay et al. (2015), Henson et al. (2015) and Guidi et al. (2015) showed different patterns regarding the b. While Henson et al. (2015) and Guidi et al. (2015) showed similar patterns – although Guidi et al. (2015) presented a more regionalised b – Marsay et al. (2015) found a completely different pattern. We have extended the discussion on regional variations of b by including references to Guidi et al. (2015).

“4. Can the assumptions inherent in the model confirm either of the spatial particle flux length scale maps proposed by Marsay et al. (2015) or Henson et al. (2015) and Guidi et al. (2015)? [. . .] We finally examine and discuss derived maps of particle flux length scales against the background of maps derived from observed quantities (Henson et al., 2015; Marsay et al., 2015; Guidi et al., 2015).”

Table 1 is very hard to go through even though very informative. Representing the 4-last column with a clustergram (heatmap) could help to cluster simulations that present similar outcomes.

Thank you for your suggestion, which is very helpful. We now clustered the last four columns of Table 1 as a heatmap ranging from yellow (high fit to observation) to red (low fit to observations).

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