

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

Anonymous Referee #2

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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. 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)

Particle sinking speed: The introduction refers to a large range of particles sinking

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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 ~ 0.002 cm 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.

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 b s 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 b s 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.

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

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is well capture by the model before to perform any sensitivity analysis.

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?

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.

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