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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. (Referee's comments in blue.)

This is a nicely focussed study looking at the parameterization of particle aggregation processes affect ocean biogeochemistry and the characteristics of oxygen minimum zones. There has been an understanding for quite a while that the way in which particle processes are represented in biogeochemical models can affect OMZs (see e.g. Moore et al., J. Climate 26:9291–9312, 2013), so it is nice to see this being addressed directly. Whilst I enjoyed the manuscript a great deal, I did find the presentation confusing in a few places (mentioned below) and the authors should consider small re-writes to explain more explicitly what is going on. I have some questions concerning the methodologies used but the authors.

Firstly, the mineralization of detritus is set in the model to be dependent on oxygen availability, but not temperature. Iversen and Ploug (Biogeosciences 10:4073–4085, 2013) show that temperature can have a strong influence on carbon specific respiration rates and consequently on deep-ocean particle fluxes. I can understand not including this in the model, but I think some discussion of how this might affect the results of the simulations should be included.

Thank you very much for this comment. We included only oxygen-dependent remineralisation and neglected the temperature-dependent remineralisation in an attempt to isolate the potential effect of particle aggregation on remineralisation and particle distribution. If temperature-dependent remineralisation was included, this would additionally impact the depth profile of particle fluxes, and a separation of both effects would be made difficult. Both, temperature-dependent remineralisation and particle aggregation suggest deep particle flux in high latitudes as inferred from observations (e.g. Marsay et al., 2015). By excluding direct temperature effects on remineralisation, we can test the hypothesis to what extent observed regional variations in b could be generated by aggregation rather than by temperature as assumed previously. In our revised manuscript, we integrated this aspect into the discussion.

“However, if temperature-dependent remineralisation, as suggested by Marsay et al. (2015) or Iversen and Ploug (2013), was also included in our model, this would likely enhance horizontal variations in the particle flux profile, with even deeper flux penetration in the cold waters of the high latitudes and upwelling areas. [...] Therefore, two different processes – particle aggregation and/or temperature-dependent remineralisation – suggest low b values and deep flux penetration in the very productive areas of high latitudes. A third process, which consists in oxygen-dependent remineralisation, is superimposed on these in OMZs, causing the steepest particle profiles in these areas.”

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Discussion paper

I find it curious that including aggregation improves representation of the OMZs using a criterion of 50 mmol m-3 for both resolutions of model in the northern hemisphere, but not the southern hemisphere. I almost get the impression that the authors are arguing that there is a fundamental difference in aggregation between the two hemispheres. If this is so, then they need to explain more explicitly what they mean. I could imagine differences in particle production, or community composition, leading to different particle processes or rates, but I wonder if this is what was meant.

Thank you. As already described in Cabre at al. (2015), there are several physical causes, which might explain OMZ biases. One possible factor for this bias consists in a too weak equatorial current system affecting nutrient trapping (Dietze & Loeptin, 2013), a positive feedback between high productivity in upwelling areas and thus strong sinking and remineralisation (Schmittner et al., 2005). Together with the meridional overturning circulation, which is asymmetric with respect to the equator, this could affect the size and location of OMZs, which we suggest to be the case in our study. As we used the Transport Matrix Method (TMM), an efficient offline method that simulates total tracer transport but does not allow its separation into the different components of advection and diffusion, we are unfortunately not able to prove our suggestion. We therefore think that the difference in the representation of OMZs between NH and SH is more caused by physics than by biology. However, we decided to plot the overlap between model and observations, following Cabre at al. (2015), to be able to compare our model results with the results from CMIP models.

The authors find that the relationship between sinking velocity and particle size is crucial, and that better fits of their model are obtained using more formulations with more porous particles. Their best fit is with eta=0.62 and using the formulae in Stemmann et al., 2004 for settling velocity as a function of mass and fractal dimension, this implies a fractal dimension of 1.62. This is in line with recent observations by Jackson who shows that his aggregation models give a best fit data for fractal dimensions of about 1.8.

Thank you for your advice. In the revised manuscript, we now also refer to G. A. Jackson's work.

"The exponent for the relationship between size and mass is set to ζ = 1.62, as proposed for marine aggregates in Kriest (2002), which is in line with more recent findings (Burd and Jackson, 2009; Jouandet et al., 2014)."

The best fit to JRMSE has a maximum particle size of 4 cm, which is quite large. Is there observational evidence that such particles are common? Given the slope of the size spectrum in their models, can the authors calculate how common these particles are and where they would be found, and is there observational evidence for this?

Thank you for your important comment. DL is defined as the maximum diameter for size dependent processes and aggregation. In practice, DL defines the upper limit for these processes i.e. for particles with diameter > 4 cm the sinking speed will not increase anymore. We defined this parameter more clearly in our methods and discussion. Moreover, we find lowest values for the spectral slope in the Southern Ocean and, in line with these results, the highest abundance of large particles with a diameter > 4 cm in the Southern Ocean. Globally, we find a maximum number of these large particles of 0.0016 particles per litre. This value seems negligible small, which, in turn, is in line with observations, where the probability of collecting such large particles is quite low.

"However, model calibration against observed particle dynamics has to account for characteristics and limitations of observations. For example, the size spectrum assumed in our model is of infinite upper size and also contains particles with a diameter larger than, e.g. 4 cm (the upper limit for size-dependency of aggregation and sinking). While these particles exist (e.g. Bochdansky and Herndl, 1992), they are very rare (in the model, and likely also in the observations), and might not be observed with standard methods, which usually rely on a sample size of few litres. The rare occurrence of large particles, and the limited sample size has, for example, consequences for estimated of size spectra parameters (Blanco et al., 1994). Thus, any model calibration against observations of particle abundance and size has to account for a proper match between simulated and observed quantities."

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