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

Anonymous Referee #1

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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

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Discussion paper

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.

I find it curious that including aggregation improves representation of the OMZs using a criterion of 50 mol m-3 for both resolutions of model in the northern hemisphere, but not the southern hemisphere. I almost get the 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.

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.

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?

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