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Interactive comment on "A mechanistic particle flux model applied to the oceanic phosphorus cycle" *by* T. DeVries et al.

Anonymous Referee #1

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The authors develop a mechanistic model for the decrease in particle mass from consumption and remineralization as particles sink through the water column. They the use this model is a global model examining the phosphorus cycle.

I am extremely sympathetic to the direction that the authors have taken and I very strongly agree with their sentiments concerning how poorly these processes are generally considered within global models. This manuscript is very timely, especially given recent analyses of results from CMIP5 models by Keith Moore and others showing the consequences of poor remineralization models.

Whilst I think the approach taken by the authors needs to be strongly encouraged, I am concerned by some aspects of the model and presentation.

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1) Terminology: I think it is important that the authors get their terminology straight and in agreement with existing terminology. For example, in section 2 what the authors define as the “number density of particles” is actually not a number density at all, but a spectrum or spectral density. A number density would have units of number per volume of fluid.

2) Again in section 2, the authors cite a paper by Burd and Jackson (2002) as justification for neglecting coagulation and fragmentation processes in their model;I the rational appearing to be that the authors are applying their model to waters below the mixed layer depth (but see item 6 below). However, the Burd and Jackson paper does not say this âĂŤ it concentrates on the accuracy of scaling solutions to the Smoluchowski equations in the presence of disaggregation. Indeed, the second paper in the series by Stemmann et al. (2004) âĂŤ the first in the series is already cited by the authors âĂŤ uses DYFAMED data and a coagulation model to show that coagulation is generally unimportant below the mixed layer, but fragmentation can be important.

3) Equation (2) is a form of raindrop equation (an equation showing the change of mass of a falling raindrop) or rocket equation (an equation showing the change in speed of a rocket that is burning fuel).

4) The authors claim that the variation of particle settling velocity with particle size is well fitted by a power-law distribution. This may hold for a single type of particle (copepod fecal pellet, diatom cell etc.) but is not generally true. Although people have fitted power laws to settling velocity data, those fits have generally a huge amount of scatter associated with them and generally do not hold if one looks at different types of particles. Assuming a single, power-law relationship for settling velocity is a commonly used simplification, but the data are definitely not “well fit” with a power law. There is an additional problem about whether or not the fitting procedures used in obtaining these power laws are the correct ones or not, power-laws are deceptively tricky relationships to use in regressions.

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5) In equation (4) the authors assume that the rate of mass loss within each particle size class is simply proportional to the particle mass, leading to an exponential relationship. This is a nice approximation to make (one can solve equations with a pencil and paper) but it effectively removes by fiat the processes that are meant to be changing particle mass, i.e. the "biological dynamics". This seems self-defeating. I am not questioning the assumption, I would probably make it myself, but rather than simplifying the "biological dynamics", it decouples it entirely from the changes in particle mass. So the logical problem here is that biological processes are meant to be changing the particle mass through "a complex set of processes by which particles are grazed by filter feeders ...", but they are effectively decoupled from changes in particle mass. One thing that might be possible to do is to compare the current model results with those using a simple, linear coupling with a depth dependent microbial abundance that can either be dynamic or imposed. That might indicate the viability of the assumption made.

6) In equation (7) the authors assume a power-law again, but this time for the size spectrum. It is unclear to me that there is evidence that a single power law covers the relevant range of particle sizes. I do appreciate that data are commonly fitted to a power law, but the only evidence I'm aware of for a single power law is from the Monterey Bay data analyzed by Jackson et al. in 1997. But these data were all in the top 20 m or so of the water column. However, the authors here are looking at size distributions below the mixed layer where coagulation is no longer important and so one would expect to see scale dependent processes that lead to non-scale invariant size distributions. Another size distribution for which equations can be analytically solved is the log-normal distribution, and the authors might wish to examine what were to happen if they replaced their power-law size distribution with a sum of say 3 or 4 log normal distributions.

In summary, I find the manuscript timely, the approach exciting, but there are details in the assumptions that have been made that have caused the authors to stymie themselves. I would very strongly encourage the authors to consider the suggestions made

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above.

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