Biogeosciences
Discussions

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

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We thank the reviewers for their thoughtful and constructive comments which will help us to prepare an improved final manuscript. Here we provide our replies (in **bold text**) to their comments (in plain text).

Reviewer #1

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.

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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.

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.

The reviewer is correct. The correct term for $n(D)$ is the particle size spectrum or particle size distribution, not the particle number density. This will be corrected in the revised manuscript.

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; 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 – 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) – the first in the series is already cited by the authors – uses DYFAMED data and a coagulation model to show that coagulation is generally unimportant below the mixed layer, but fragmentation can be important.

Our apologies for the incorrect citation, which will be replaced by the Stemmann et al. (2004) citation in the revised manuscript (regarding coagulation below the mixed layer). That the assumption of no fragmentation below the mixed layer

may be inaccurate under some circumstances will be discussed in the revised manuscript. In the present formulation of the model, fragmentation could also be assumed to be included in the linear loss rate of particle mass (equation 4), i.e. included in the admittedly simplified biological dynamics.

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).

Thank you for pointing this out.

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.

We use a power-law relationship as it is a widely used relationship for relating sinking speed and particle size. In the current formulation of the model, there is only one generic type of sinking particle. The various different types of sinking particles (e.g. phytoplantkton aggregates, fecal pellets etc.) are not resolved by the model, so it is not possible to consider the different settling speeds of different particles. The limitations of these assumptions will be made more clear in the revised manuscript. Separately resolving different types of sinking particles is beyond the scope of this paper, but will be important to address in the future.

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.

The assumption that the rate of particle mass loss is proportional to the particle mass itself is made, as the reviewer notes, for simplicity and to allow relatively easy solutions. Resolving all of the biological processes that decrease particle mass (e.g. grazing by filter feeders, colonization by bacteria etc.) is beyond the scope of the present study. If one is to parameterize these processes (as commonly done in ocean biogeochemistry models), we think that the assumption that mass loss is proportional to mass is a reasonable one. Indeed, this assumption is shown to hold quite well for sinking diatom aggregates in laboratory experiments (see Iversen and Ploug, 2013, Figure 3).

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.

Equation (7) is the boundary condition for the particle size distribution, i.e. it holds only at the base of the mixed layer. Below the mixed layer the particle size distribution is not described by a power-law (see Fig. 1a). We assume a power-law primarily because we use the satellite particle size distribution from Kostadinov et al. (2009) as the boundary condition, and that work uses a power-law to describe the surface particle size distribution. Equation (7) could in principle be replaced with a sum of several log-normal distribution, as the reviewer suggests. This may indeed provide a better approximation of the surface particle size distribution, and should be investigated in future studies, but is beyond the scope of the present study.

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 above.

Reviewer #2 (E. Ingall)

Overall, this is a very well written and interesting paper. The description of the model and the justifications of the assumptions are clear. The modeling experiments are done in a logical progression. This leads to the key finding that observed phosphate, particle flux profiles and sediment trap data are best fit when organic matter burial and ballast effects are added to the model. The ballast mechanism essentially offers a mechanism

to slow the remineralization of P from organic matter during transit to the sea floor. The authors nicely point out that this could also be achieved by allowing for less degradable P forms to be included in organic particles. No matter the mechanism, one wonders if this less degradable fraction could explain the Redfield-like organic C:P ratios seen in low sedimentation rate pelagic sediments described by Ingall and Van Cappellen 1990?

Thank you for bringing this study to our attention. The study of Ingall and Van Cappellen (1990) found that C:P ratios of organic matter burial were lowest in low-sedimentation rate pelagic environments, which indeed is consistent with a substantial non-degradable organic P fraction as implied by our study. This will be further discussed in the revised manuscript.

A smaller point of this modeling effort is the suggestion that the oceanic residence time for phosphorus that is on the same order as that for nitrogen. This is in line with a progression of papers over the last 30 years revealing data resulting in a calculated reduction of the oceanic P residence time by almost an order of magnitude. Such a shorter residence time is certainly consistent with many observations in sediment data of a rapid and dynamic response of the marine P cycle to glacial-interglacial cycles.

For the non-modeler, the conclusions of this paper can certainly form the basis of future research hypotheses regarding the forms and reactivity of P in marine particulates.

The manuscript is in great shape. However, the text size on many of the figures is so small that it is nearly illegible. This should be an easy fix upon revision.

Thank you for pointing this out. This will be corrected in the revised manuscript.

REFERENCES

Iversen, M.H. and H. Ploug (2013). Temperature effects on carbon-specific respiration rate and sinking velocity of diatom aggregates – potential implications for deep ocean export processes. *Biogeosciences*, 10, 4073–4085, doi:10.5194/bg-10-4073-2013.

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