

Interactive comment on “A Stochastic, Lagrangian Model of Sinking biogenic aggregates in the ocean (SLAMS 1.0): model formulation, validation and sensitivity” by T. Jokulsdottir and D. Archer

Anonymous Referee #2

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Jokulsdottir and Archer present a new approach to modeling marine aggregates, that not only focuses on the size of aggregates, but also on their composition and age. The aggregation (size) dynamics are based on standard algorithms. The model also includes TEP-dependent stickiness, fragmentation and ingestion by zooplankton feeding, age-dependent decay, photodissolution, and different dissolution mechanisms of carbonate. A 0D version of the model is evaluated against standard algorithms for aggregation. The 1D model (including all associated processes) is evaluated against sediment trap data of organic carbon flux at 11 stations, export production, export ratio and rain ratio.

I really like this new approach to modelling many different particle properties (size,
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composition, age, all of which are deemed important for particle flux and remineralization) while keeping the model numerically tractable. Combining aggregation dynamics with a Monte Carlo method, and the application of super-particles is a new and elegant move in this field of research. I also appreciate the detailed and careful way, in which the authors have tried to parameterize some processes. However, I think these very useful features of the model merit a better presentation. In particular, I have the following two major concerns regarding the current state of the paper:

(1) The model in its current state combines a variety of highly non-linear processes, most of which have never been described (numerically) in this way before. In addition, while some of these processes are based on fairly straightforward principles (e.g., aggregation), others are parameterized in a quite ad-hoc way (e.g., zooplankton grazing/ingestion/fragmentation; photodissolution). Therefore, I think a thorough sensitivity study across the different processes and parameterizations is necessary in order to present this very advanced, complex model properly, and also the impact and relevance of the different assumptions it contains. Alternatively, one may switch off some of the processes which are not well understood, and just present the effect of new parameterizations for aggregation and particles vs. a “simple” (e.g., sectional) model of aggregation.

(2) The model is poorly evaluated against observations. Firstly, although the model is simulated with very simple physical forcing (temperature profiles; no seasonality; obviously neither mixing nor advection), it is evaluated against data from the real world, at sites with pronounced hydrographic features. Secondly, the model is evaluated against organic matter flux - perhaps the biogeochemical diagnostic most prone to methodological problems, and the least “1D-ish” variable (as suggested by the large statistical funnel of some sediment traps). Further, although the model focuses on particle size and composition, there is no evaluation against similar parameters from the real world.

I feel very sorry to be so critical about this paper, because I am convinced that it contains a very interesting and innovative way of parameterizing particles in marine

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biogeochemical models. I encourage the authors to revise the manuscript, perhaps even redesign their model experiments, and present it in a different way, as this model (and the conclusion that may be drawn from a more detailed analysis) may be of large interest to the modeling community. As my criticism is of more fundamental nature, I have only few detailed comments (see below).

Some detailed comments :

Abstract

(p 5932, line 8) “orgC” - here (at the beginning) I wondered if this also refers to dissolved organic matter.

1 Introduction

(p 5933, lines 10ff) What about the papers by Guidi et al. (2008, 2009), or Roullier et al. (2014), who present a detailed view on particle size and flux?

(p 5933, line 29) “Three processes take place in zooplankton guts: respiration of orgC, ..” - sounds strange.

(p5934, lines 13-14) “Observed flux from sediment traps in the Equatorial Pacific is used as a study site to tune parameters that are ad hoc or unknown.” - Observed flux as a study site sounds odd. Further: Why choose this particular site? What about the other sites?

2 Model description

(p5934, lines 24-25) “The most common approach to modeling the flux of material through the water column is using a spectrum of particle size classes ...” - The most common approach is probably the Martin curve; I assume you want to say: “The most common approach to modeling the size-dependent flux ...”

(p5934, line 26) “that aggregate according to coagulation theory, based on the sectional method of (Jackson, 1990)”. As far as I know, originally the sectional method

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was developed by Gelbard (1980) to simulate aerosol dynamics, and later applied by Jackson and Lochman (1992) to marine particles.

(p5935, lines 14-15). “Aggregate classes ... ” - Are these the ACs mentioned/defined above? Then I would stick to that name “AC”.

(p5935, line 24) “20 new ACs are created per 8 h time step within the euphotic zone in the model.” - Added to the already existing ACs, correct?

Table 2 is never referred to.

(p5935-p5936) The section about TEP is very interesting, but I think it could be presented in a more comprehensive and clearer way. E.g., from “In SLAMS, TEP production consists of 6 % of the primary production in the default case, in terms of carbon. This number comes from sensitivity studies where we simulate Equatorial Pacific conditions (SST, PP and sea-sonality) and compare orgC flux to the deep ocean to sediment trap data.” it is not clear to me how this number was derived, and to what sensitivity studies the authors refer to. Further, the two sentences “The role of TEP in controlling the flux of organic matter is complicated however by its low density (0.8gcm⁻³), which acts to decrease the overall density of an aggregate, potentially to the point where it becomes buoyant and ascends rather than sinks (Mari, 2008).” and “The density of TEP is less than the density of sea water which results in a possible upward ascend of particles.” basically state the same thing twice.

(p5937, lines 24-25) “where VTEP is the volume of TEP in the aggregate and Va is the volume of the entire aggregate, represents how likely two colloids or aggregates are to stick” - sounds odd.

(p5939, line 23) “unless one or more aggregates sank out” - aggregates or ACs?

(p5940) Is aggregation the only process that determines time step length?

(p5941, line 11) “aggregates stick to each i-aggregate” - provided that TEP is in the aggregate?

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(p5944, line 15) “there are no lateral currents” - Does this make sense for the equatorial Pacific?

(p5945, line 11) “producing the observed decrease in reactivity” - Who observed this?

(p5945, line 17) “observed respiration rates of fresh phytoplankton” - Respiration rates of the phytoplankton itself?

Fig 4 is never referred to.

(p5947, lines 2-5) “However, the various constraints on the model dynamics arising from the observational data required a metabolic efficiency scaling factor, $g(z)$, that increases from the surface to about 400 m and then decreases again (Fig. 3).” This - particularly the $g(z)$ - seems rather arbitrary, and, in my view, somehow spoils the otherwise carefully designed particle dynamics. If the three subpanels in Fig 3 are sensitivity experiments for this rather weakly constrained assumption, I would suggest to present and discuss this in more detail.

(p5947, line 20) “Again, to satisfy particle spectrum data, ...” What is meant with this?

Subsection 2.9: The parameterization of photodissolution seems a bit ad-hoc. Is there empirical evidence for this (beside the photolysis of DOC)?

Subsection 2.10: The physical setting is very simple. This is fine for a purely theoretical study, but seems awkward, when later model results are compared to real data of sediment traps.

3 Model validation

Subsection 3.1 (p5953, line 10 - p 5954, line 17) The first paragraphs of subsection 3.1 rather seem to belong to a discussion section.

Subsection 3.2 Comparison to observed size spectra should be more detailed. So far, it just states “In the ocean, the slope of the particle spectrum is found to be in the range -2 to -5 and in the model it is mostly around -3 .” which is very vague.

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4 What controls the variability

I don't think the statements made in this section (e.g., "Our results suggest that the biological pump is most efficient ...") can be made from the current model setup, with the very simple physical setup.

References:

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