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Comment

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

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Thank you for the helpful and insightful comments. I sincerely appreciate how thoroughly you read my manuscript and I hope the changes that I've made work to make it better and that all your concerns are addressed.

I made two changes to the model. I decided to add a mixed layer and remove the photodissolution. The mixed layer depth is a complex feature of the watercolumn that may affect flux in a number of different ways. Additionally, in SLAMS it conveniently takes care of the problem of the buoyant, TEP-rich microlayer at the surface that would occasionally form and grow continuously once formed.

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The other change was to simplify the zooplankton formulation. The metabolism factor that was a complex function of depth is gone. I found that it is not needed to change with depth and having the zooplankton encounter rate be a simple function of food available gives equally good results compared with sediment trap data.

With these changes to the model I had to rerun all the runs which took a lot of time. Another thing that is modified is that instead of using the Equatorial Pacific as a default case and the data that we use to tune ad hoc parameters, we use an “average ocean” with SST=17C, PP=700mgC/m²day. I have added to and changed the manuscript quite a bit.

A major change to the manuscript is the section: Sensitivity Studies (section 3). I made sensitivity runs for numerous parameters: alpha, bacterial respiration, TEP, zooplankton fragmentation and encounter and a more thorough look at how the slope of the particle size spectrum varies with changes in stickiness and zooplankton encounter. Changes to manuscript: Added two new figures: figure 4 and 6, and section 3.

Below are reviewer comments in CAPS and my response in non-caps.

MIXED LAYER AND PHOTODISSOLUTION Starting with the idea that there should be a mixed layer in the model I made runs with varying thickness of mixed layer, from 10m to 500 m. Since the default SST is 17C, the temperature in the mixed layer is set constant at 17C in these runs. Below the mixed layer, temperature increases linearly to 4C at 1km. At each timestep, a particle that is in the mixed layer gets randomly assigned a new depth within the mixed layer. There are not yet nutrients in the model, primary production is fixed (default=1000mgC/m²d) and so a mixed layer does not affect production or the depth at which it takes place. We’ve added a figure to show the effect of a mixed layer on the flux.

Changes to the manuscript: Section 2.11 (lines 717-722) and 3 (lines 810-820). Including new figure: figure 8.

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ZOOPLANKTON ENCOUNTER MODEL and $g(z)$ NOT WELL JUSTIFIED The zooplankton model we present is very simple primarily because we had to formulate it to fit in the monte carlo and lagrangian frameworks. We've taken out the function $g(z)$ which was a "metabolism parameter" and now the zooplankton encounter model is a simple function of food availability (equation 33).

We include a zooplankton breaking parameter because zooplankton interaction with particles is necessary to get small particles in the deep ocean and get a sensible flux. No or few zooplankton in the model results in no or very few small (<1mm) particles in the deep ocean. Although studies about zooplankton fragmenting aggregates are not numerous, swimming zooplankton clearly do have the ability to fragment marine snow and they are ubiquitous in the ocean. A recent study (NATURE | VOL 507 | 27 MARCH 2014) found it to be an important mechanism in closing the carbon budget of the twilight zone, giving us further confidence that it is not an outlandish idea. That is why we want to include it. The problem is I coded it in such a way to make it hard to tease apart zooplankton ingestion and just fragmenting. Changes to manuscript: section 2.9 (lines 560-580), equation 32 and 33. Removed figure 3. DOCUMENTATION FOR CARBONATE ION PROFILE We use the profile given in Anderson and Archer (2002). This citation is now in there. Changes to manuscript: Added citation, section 2.12 (line 714)

INCORRECT DISCUSSION OF SECTIONAL METHOD AND UNDERAPPRECIATION FOR PAST MODELING STUDIES I see that I was unclear on what the definition of "sectional method" was exactly. I have changed its presentation in section 1 and added some more appreciation for past modeling studies. Changes to manuscript: Section 1 (lines 95-126)

"ab^N – NOT TRUE" I wasn't aware of the study cited by George (Jackson, 1998) and have now added it and changed the wording to reflect that it's only the runtime of the coagulation function that is unaffected, not the whole model. Changes to manuscript: section 4.1 (lines 805-810)

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THE PROGRAM TOOK 74 MINUTES TO EXECUTE Yes, it does take a while. Sounds like you have a pretty nice rig though. I now make clear that the model in its current form isn't suitable to couple to GCMs. Changes to manuscript: Section 4.1 (lines 812-814)

POOR EVALUATION AGAINST OBSERVATIONS We evaluate the particle size spectrum and compare its slope to data. We compare the rain ratio at the sea floor to data, export as a function of SST and PP, and organic carbon flux through the water column. We have expanded figure 10 to include panels excluded from the previous manuscript. Before we thought that it didn't add anything to the story but now we feel that maybe readers are interested none the less. We have also added a study on the effect of a mixed layer on the orgC flux to address the issue of no mixing when comparing to real world data. Changes to manuscript: Mixed layer discussion in section 3 (lines 785-795) and figure 7. 3 new panels in figure 10.

NO EVALUATION AGAINST PARTICLE SIZE SPECTRUM DATA I appreciate this comment, it prompted us to look for mechanisms that affect the slope of the particle spectrum. The difficulty that arises when we compare the slope of the particle size spectrum of the model to data is that the slope in the real world is all over the place, it ranges from -2 to -5 (Guidi et al. 2009) with a vague pattern that is not well understood, or at all. In our model, the particle spectrum slope is more constant than appears to be the case in the real ocean, we point out that this might be attributable to a simplistic zooplankton formulation. However, as we have recently discovered, stickiness and TEP seem to have an effect on the slope although we are unable to come close to the steepness of -5. Changes to manuscript: We have included a sensitivity analysis of the slope of the particle spectrum (lines 778-787) and added two panels to figure 6. Also, added some discussion in section 4.2 (lines 865-877)

ORGANIC CARBON FLUX DATA PRONE TO PROBLEMS Since our study is about understanding the organic carbon flux in the ocean we think it makes sense to compare our model results to available data on organic carbon flux from papers that have made it through the peer review process. I think that even though it may be a difficult thing to

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measure, enormous resources have been put into those measurements and so I am inclined to feel they are worthy of us using them. No changes to manuscript.

INCLUDE MARTIN CURVE IN FIGURE 8 We have now included a (two actually) Martin Curve in figure 9 (used to be fig. 8). And also taken out the squares and the triangles that were not being referenced. Changes to manuscript: figure 9.

DISCUSS APPLICATIONS OF MODEL We discuss potential applications of the model and future aspirations for our model in section 1 and 6. Changes to manuscript: section 1 (lines 150-160), section 6 (lines 1000-1020)

DEFINITION OF BI I believe the definition is now consistent throughout the manuscript and should thus not be confusing. We have moved the discussion of BI up, to section 2.3 and continued with the example in sec. 2.2 to clarify. In table 3 where before we had $BI=0$ it now says $BI=1$, meaning production is constant throughout the year. Changes to manuscript: table 3. Section 2.3, lines 220-245.

PRODUCTION SCHEME UNCLEAR I have clarified the production scheme by adding equation (2) describing the depth at which production takes place. Changes to manuscript: equation 2. (line 215).

INCLUDE DISCUSSION ON MORE RECENT FINDINGS, EG. LAM, HENSON, MARSAY, GUIDI, ROULLIER Yes, I agree. I read Guidis papers back and forth and I am not sure how I could have omitted citing any of his papers. It would be nice to have a more thorough discussion on transfer efficiency, but I think I will have to put it in the next paper. I have tried to tie ideas in these papers in. Changes to manuscript: Lines 100-102; figure 9; Section 2.3 (lines 222-228), Section 4.2 (865-869), Section 6 (1007-1010)

WETHERILL 1990 IRRELEVANT TO OCEAN SYSTEM This analysis is just to test the theory: how well the derivation in section 2.5.2 holds up and the resulting equation 17. I guess we figured it would suffice to compare our model results to one other model

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(Burd and Jackson, 1997 – fig 8b). It would be a really interesting study, to compare particle models, and I've been talking to Adrian Burd about doing this, but it is beyond of the scope of this paper. No changes to manuscript

Some detailed comments - here my response is at the end of the reviewer comment, starting with –.

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? –Yes, included now. See comment above

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

(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? –We have changed this. As explained above, we now use all the data to tune ad hoc and unknown parameters. We start with average ocean conditions: SST=17C, PP=700, BI=1 and MLD=50m. And find the zooplankton encounter rate that goes through all the data. 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 ...” –Yes, you are right. See comment above. I have changed this section.

(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 was developed by Gelbard (1980) to simulate aerosol dynamics, and later applied by

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Jackson and Lochman (1992) to marine particles. –Thank you, I was confused by the origins of the sectional method. I have now changed the text to include this.

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

(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? – Yes

Table 2 is never referred to. –Ok, now it is (line 204)

(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 seasonality) 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. Thanks, gave me an opportunity to cite my thesis. Further, the two sentences “The role of TEP in controlling the flux of organic matter is complicated however by its low density (0.8gcm³), 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. Fixed.

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

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

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

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timestep is 8 hours. Occasionally this results in $P > 1$ when evaluating aggregation and in that case the timestep is reduced, while the given encounter is being determined. I have changed the text to make this clear (line 360).

(p5941, line 11) “aggregates stick to each i-aggregate” - provided that TEP is in the aggregate? –Well, that is in the P. If there is no TEP, P will be 0.

(p5944, line 15) “there are no lateral currents” - Does this make sense for the equatorial Pacific? –Particles sink to the sea floor in anywhere from a couple of weeks to a couple of years.

(p5945, line 11) “producing the observed decrease in reactivity” - Who observed this? –Thanks, the citation was misplaced. It’s figure 1 in this paper.

(p5945, line 17) “observed respiration rates of fresh phytoplankton” - Respiration rates of the phytoplankton itself? Ahh, changed to: freshly produced diatom aggregates. Fig 4 is never referred to. – Thanks, all figures and tables are now 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. – See comment above. I took out the subpanels because they don’t add to the story. They are/were the total amount of orgC in each depth bin.

(p5947, line 20) “Again, to satisfy particle spectrum data, ...” What is meant with this? –Good question. I’ve changed this to “To simulate a smooth function w.r.t. aggregate size, we come up with the equation:”

Subsection 2.9: The parameterization of photodissolution seems a bit ad-hoc. Is there

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empirical evidence for this (beside the photolysis of DOC)? –We’ve scratched photodis-solution. After the mixed layer was added it is no longer needed.

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. –Ok.

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. –Yes, good point.

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. –Yes, I have now expanded this to include a figure and more discussion

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. –I don’t understand. We made a model that does make simplifications but also simulates many physical, chemical and biological mechanisms. There are some results. I am not saying “The biological pump is most efficient . . .” I am saying “Our results suggest that the biological pump . . .”

Please also note the supplement to this comment:

<http://www.geosci-model-dev-discuss.net/8/C3564/2016/gmdd-8-C3564-2016-supplement.pdf>

Interactive comment on Geosci. Model Dev. Discuss., 8, 5931, 2015.

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