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Interactive 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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It is useful to separate this paper into two fractions: first, the assumptions, equations, and parameter values used to describe processes that constitute the model; second, the numerical technique used to solve the problem that constrain what is practical to solve. For example, physical oceanographers have approximations to the fundamental Navier-Stokes equations; they also have multiple techniques for solving them, including finite difference, finite element, and spectral methods. In the case of this paper, the model description includes coagulation dynamics for describing particle size distributions, multiple particle types, including TEP and various carbonate minerals, settling

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Interactive Discussion

Discussion Paper

properties, and biological-mediated particle destruction. The model used here is an extension of the coagulation approach extensively used in the past to describe particle dynamics in the ocean; the numerical technique is a relatively new application to solve the model for the oceanic water column. In the following review, I address the two aspects of the paper separately.

MODEL FORMULATION:

The coagulation equations are fairly standard. The descriptions of TEP and stickiness are not, but there have not been many experiments to test the best way to parameterize their effects. As a result, any model has a certain arbitrariness to it.

The model appears to include no standard physical processes, such as mixing or advection. There is an imposed linear temperature relationship with depth. With no mixed layer mixing, it is not clear why, then, there is a photodissolution term (section 2.9).

The phytoplankton model is poorly described. It seems to include a fixed rate of primary production over an imposed euphotic zone of unstated thickness. There does not seem to be a surface mixed layer, as all movement appears to be downward using particle size-dependent sinking rates. Previous studies have shown that the thickness of the euphotic zone and surface mixed layer are important properties in determining the maximum particle concentrations and resulting coagulation rates. The authors need to document their assumptions clearly.

The formulation of the zooplankton encounter model is unclear and not justified by any appeals to the literature. The amount of zooplankton appears to be equal to or proportional to organic carbon concentration, presumably at each depth bin, but the notation used (two indices for C rather than 1 used elsewhere, Eq. 30) make this uncertain. The use of an assumed metabolic efficiency scaling factor g(z) is stated as required, but not explained or its form justified (p17, L4). Given the importance of zooplankton consumption and its role in fragmentation, this arbitrariness is surprising. As noted in 2.7.2, the flux is very sensitive to the parameter choice for zooplankton

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feeding.

The authors argue strongly for the role of zooplankton in disaggregating particles, citing observations that the concentrations decrease at night. In fact, observations show considerable variation. Graham et al (2000) did observe a decrease at night, Stemmann et al (2000) actually observed a decrease during the day, Goldthwait and Alldredge (2006) observed increases, decreases and no change during the night, and Checkley and Jackson (2011, DSRI 58) observed a decrease during the night. Thus, the evidence for their interpretation of zooplankton control of aggregate size during the night is hardly overwhelming.

The chemistry of the inorganic fractions is relatively well described, although it needs documentation to support the assumed description of carbonate ion concentration (2.11).

MODEL SOLUTION:

The authors provide FORTRAN code to run their model, a laudable inclusion. However, they need to go through the code and clean it up. It took me most of a day for me to get it up and running using the Intel Fortran compiler because of relatively minor errors, particularly in variable typing, and small but annoying inconsistencies in function calls. It would also help the user if the authors documented the program. When I was able to run the program using the authors' enclosed data file, the program took 74 minutes to execute. This is a long time compared to sectional code written in slower Matlab. I suspect that the long run time will make this approach unsuitable for use in large scale GCM models, but time will tell.

The references to sectional models in p27, L13-17, are wrong. The model in Jackson (1990) did not use the sectional approach but tracked particles by the number of algal components they contained (P23, L13). The first usage of the sectional model to describe phytoplankton dynamics was Jackson and Lochmann (1992, Limnol. Oceanogr. 37). It used 22 sections, not 10 or 100.

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Kriest and Evans (2000) did not use the sectional approach (p24, L16). They assumed a constant spectral slope and described the resulting distribution with only 2 parameters.

The number of bins required for a sectional approach is stated to be “(ab)ˆN” (p23, L20-22). Not true. First, mass balance constraints can be used to drastically cut the number of compartments needed for the sectional approach. Jackson (1998) J Colloid Interface Sci (202) developed a technique that, in that case, decreased the number of sections from a*b to less than 4*a. Second, just as the authors chose to ignore low probability interactions “to prevent an unmanageable proliferation of particle types”, so too can developers of other techniques.

It would be nice if the authors compared their solution method to those of other models using the coagulation equations pertinent to the ocean. The results used to test the model, Wetherill (1990), uses vastly simplified interaction relationships for planetary accretion that are not relevant to the ocean system.

OVERALL ASSESSMENT:

The authors explore an alternative way to solve the coagulation equations that may well be useful for exploring different aspects of particle dynamics in the ocean, particularly investigating the chemical aspects that could be difficult for a sectional model. I suspect that the model solution method is too complex and time consuming to be useful for inclusion in global biogeochemical models but that it could prove useful for specialized one-dimensional calculations emphasizing chemical properties of particles.

The large number of poorly explained and poorly understood processes that the authors use in their model makes many of their choices seem very arbitrary. The complexity of the model makes it difficult to know what the relative importance of different choices is in determining their results.

The authors have not done a good job of surveying past modeling studies in the litera-

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ture and learning from them.

This manuscript is a useful addition to the literature, but needs to improve its documentation before being ready for publication. It would be far stronger if it had a greater appreciation of past literature and the importance of documenting crucial choices made in this model.

TECHNICAL DETAILS:

Figure 3, caption, L3: “Three plots on the left” should be “Three plots on the right”.

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