



Interactive
Comment

Interactive comment on “On the treatment of particulate organic matter sinking in large-scale models of marine biogeochemical cycles” by I. Kriest and A. Oschlies

Anonymous Referee #2

Received and published: 2 October 2007

The authors investigate selected simplified parameterisations of sinking particulate matter fluxes used in global biogeochemical models. The study focuses on the underlying assumptions of functions and their effect on model predicted sedimentation fluxes of particulate organic matter. This is a timely contribution and well suited for publication in Biogeosciences. I recommend publication after major revisions. In particular I would like to see the following points addressed:

1. What was the rationale for choosing the descriptions of POM fluxes included in the paper? Beyond the comparison of different parameterisations of sinking fluxes used in global models, what are the recommendations from this study? What is the principal

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

take home message?

2. The choice of parameters: the reader has to guess why particular parameter values were chosen. For example, p3009, line 27 refers to Table 1 and below for the choice of parameters; Table 1 in turn refers to the text. A throughout discussion of the rationale behind parameter selection has to be included.
3. The authors choose to use a single and constant value for the remineralisation rate ' r '. The effects of sinking speed and remineralisation rate can however not be untangled by their approach. How sensitive are model results to the range of ' r ' reported in literature?
4. The paper would benefit from a comparison with Kriest and Evans (2000) and Kriest (2002). A rapid comparison is included in the appendix. It should be moved to the main part and thoroughly edited to improve its readability. This change will make the discussion more substantial. In the Appendix B section, p.3028, line 18, one reads that the ;results depend on the vertical resolution;. Please elaborate.
5. There is evidence that particle size distributions do evolve with depth, so do particle properties. The assumption of particle properties that are constant with depth is justified as part of the analysis, but appears unrealistic in the light of the role of aggregation and ballasting. How would changing particle properties feedback on the conclusions of this study?
6. I am puzzled by section 3.3. My interpretation is that the approach used by Maier-Reimer (1993) is the most appropriate for use in global models. The usefulness of this approach needs not to be discussed. However, global biogeochemical models have evolved from predicting climatological distributions to representing seasonal and interannual variability. Reproducing the temporal and spatial variability of export fluxes of particulate organic carbon is still an open challenge.

Interactive comment on Biogeosciences Discuss., 4, 3005, 2007.

BGD

4, S1568–S1569, 2007

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU