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Discussions

Interactive comment on "Reconciling surface ocean productivity, export fluxes and sediment composition in a global biogeochemical ocean model" *by* M. Gehlen et al.

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This paper examines how various parameterizations of water column particle dynamics in ocean GCM's affects the simulation of surface ocean productivity, export fluxes, and sediment composition. It is a thought provoking paper that tackles a very interesting and important problem, namely, how models that use an aggregation-disaggregation mechanism compare with models that include the role of ballast in determining the water column flux of organic matter. While the results lead the authors to conclude that aggregation alone is insufficient and that ballast has an important role to play, the case made in this paper is very qualitative and hand-waving and in the end not entirely convincing. I would strongly urge the authors to consider expanding their analysis along the following lines:

(1) The model simulations should be compared with water column observations of nutrients, oxygen, silicic acid, DIC, and alkalinity. We have a far better idea of what these observations actually look like than any of the properties that the authors have compared their model to, all of which have major uncertainties. Furthermore, one would expect that the different model simulations should have a considerable impact on the water column distribution of properties.

(2) To compare the model simulations with water column properties, the sensitivity studies will have to be integrated out for far more than the 100 model years of this study. They will need to be run out for at least 1000 years, or better yet for 3000 years, until the water column distribution of nutrients, oxygen, DIC, and alkalinity, come closer to equilibrium. There may be an issue of computer time available to do these simulations. If so (which I hope not) then the authors need to state this and provide a clear justification as to why the reader should take seriously the results of a 100-year simulation, and warn the reader as to how this will affect the interpretation of the results.

(3) I strongly suspect that many of the results that the authors would obtain with millennial time scale simulations would look very different from those obtained with 100 years simulations. In particular, the differences in water column processes will likely lead to a significant reorganization of water column nutrient distributions and thus to export production and the such. In previous studies such as in Anderson & Sarmiento (Anderson, L. A., and J. L. Sarmiento (1995), Global ocean phosphate and oxygen simulations, Global Biogeochemical Cycles, 9, 621-636.), the results of such reorganizations of property distributions were often quite unintuitive.

(4) The comparisons of model simulations with observations should be made more quantitative. Taylor diagrams, which were introduced to our field by the LSCE

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group, if I remember correctly, can be quite useful in evaluating model performance, e.g., Gnanadesikan, et al. (2004, Oceanic ventilation and biogeochemical cycling: Understanding the physical mechanisms that produce realistic distributions of tracers and productivity, Global Biogeochemical Cycles, 18, GB4010, doi:4010.1029/2003GB002097.). The paper by Gnanadesikan et al. also shows three alternative versions of primary production estimates using the algorithms of Carr (2002) and Marra et al. (2003) in addition to Behrenfeld & Falkowsi (1997). The newer studies give a PP distribution that looks a lot more like the Schlitzer model result, with a peak at the equator and less of a peak in the high latitudes. Also, Dunne et al. (2005, Empirical and mechanistic models for the particle export ratio, Global Biogeochemical Cycles, 19, GB4026, doi:4010.1029/2004GB002390.) provide alternative particle export ratio estimates to the Laws et al. (2000) analysis, which Dunne et al. show does not agree very well with observations. Showing these alternative estimates would give a better idea of how poorly we know the PP and export flux.

Some specific suggestions:

(1) The description of STD2 is inadequate. I did not understand what was done in this model. In fact, it would be very helpful if there were a table summarizing the main characteristics and differences between the five models.

(2) Figure 4 is too small to discern the main features of the data. I had to blow each map up to full page size. Something will need to be done about this.

(3) On page 816, the authors, referring to Figure 4, claim that the model predicted sediment TOC, BSi, and CaCO3 all agree with observations (except for continental margin TOC). I do not believe that their figures support this statement. Specific comments:

a. With TOC, there are some high latitude regions, arguably open ocean, where the model has huge organic carbon (order many 10's of %) in regions where the data are in the 0 to 1% range. Something looks to be wrong here.

b. With CaCO3 the model basically has 0 to 1% sediment CaCO3 content throughout almost the entire world, thereby missing most of the tropical Pacific values of >50%, as well as vast swaths of high CaCO3 concentrations in the Atlantic and Indian Oceans. What is wrong here? Is the model CCD too shallow, or is the model topography too deep? A comparison of equilibrium model simulations of the carbonate ion concentration with observations of this would answer part of this question immediately.

c. With BSi, the model does a lot better, but again predicts values of 0 to 1% in low latitude regions of all three ocean basins where observations seem to be in the range of 10% and higher.

(4) On page 817, the description of how "flux feeding" (a term I have never heard before and that should be explained) is parameterized in the model is inadequate. What are the zooplankton actually grazing on?

(5) On pages 819 and 820, I am not convinced by the explanation for the contrasting results between K&E and BAL. In particular, the authors attribute the low export of K&E to the low sinking speed within the upper 100 m, whereas I would have thought that this result is likely attributable to decreased nutrients in the region below 100 m where the sinking speed of K&E is much larger than for any other model. The authors do not show nutrient profiles, so I am guessing here; this is why they need to show the nutrients per the earlier suggestion I made.

(6) On page 820, top of page, the discussion of Figure 3 is a place where a statistical measure of model-data correlation would be very useful.

Interactive comment on Biogeosciences Discuss., 3, 803, 2006.