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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 essentially deals with the depth attenuation of the fluxes of organic and inorganic carbon, and opal, and how to represent that in biogeochemical global ocean circulation models. As the depth attenuation of the C fluxes relates to the time scales at which carbon is removed from the atmosphere, this has important bearing for the efficiency of the biological carbon pump and the carbonate carbon pump. The authors have implemented 5 different model formulations that affect the sinking rate of particulate matter and compared model outcome to a variety of data sets.

The main finding of this paper is that, to date, it is quite difficult to reconcile surface, mid-, deep-water and sediment flux observations. To which extent this is due to data uncertainty or to model deficiency is hard to say: some of the independent estimates are much more dissimilar to one another as to some model results (e.g. Fig. 2). In addition, it does not seem to matter how one prescribes the sinking rate of particles in the ocean, as long as it is allowed to vary roughly between 1-3 m/day and 200 m/day, and as long as the sinking speed increases with depth. A continuous spectrum of sizes and sinking speeds can be efficiently represented with two size classes that have different sinking speeds.

I have two main points of criticisms on this paper.

Point 1. How to judge which model is better or worse. In the paper, the appreciation of what constitutes a better or worse model is highly subjective. The goodness/badness of fit has to be assessed either by mean of scatter plots (Fig. 3), none of which seem to correctly represent the data, or by comparing 2-D images (Figs. 1, 4, 5). Compressing all this information into one or two comprehensible measures (like mean squared residual, r2,Ě) would have benefited interpretation of the model results. The only objective appreciation of model - data (mis)fit is in table 3, but here some of the independent estimates are left blank, although they do exist. For instance, there DO exist estimates of global sediment organic carbon deposition rates (or mineralization rates) below 1000 m. These numbers have, a.o. been compiled in the frame of the same European project (ORFOIS) as the work presented in this article (Andersson et al., 2004 - GRL 31, L03304, based on oxygen consumption rates). Similarly, these estimates could have been estimated from the Jahnke data set. When these data would have been incorporated in table 3, it would allowed seeing in one glance which description best reproduces the sediment-water fluxes.

Without having read the conclusions, my mental judgment was that the aggregate formulation was best, not necessarily because it faithfully reproduces deep fluxes (that was difficult to assess from the scatter), but because it keeps the organic matter longer in the euphotic zone, and total primary production is much better estimated (table 1). In contrast, the author's preference seems to be biased towards ballasting, and they conclude that combined effect of aggregate formation and ballasting drive the biological pump... However, even disregarding the fact that the model hosting the ballasting formulation severely underestimates primary production (table 1), it is found to also overestimate export in those areas where the relative contribution of BSi and CaCO3 in particles is higher (“areas of high diatom and calcareous nanophytoplankton production”). As the differential sinking of particles ballasted with these constituents is what distinguishes this formulation from other equations, I find it difficult to agree with the authors that this formulation might be the ‘best choice’, or even might improve the other descriptions. It seems that if the ballasting formulation provides better fits for deep-water fluxes, it does so because of the wrong reasons.

Point two: can dissolution rates be ignored. Secondly, in this paper only part of the story is given. Depth attenuation of fluxes is the combined result of sinking rates (w) and degradation rates (k): the flux attenuation with depth can be described with an exponential coefficient equal to -k/w. So to reconcile the particle flux estimates, either w must increase with depth, or k decrease with depth, or both. All these possibilities are likely. Not only should this fact be mentioned, it is also necessary to give more information on how the dissolution rates of the various particles is implemented in this model (equations and parameter values). Without this knowledge, it is quite difficult to understand some of the model results (e.g. the increase in sinking rate as resulting from the ballasting model). Therefore, I suggest that the authors also discuss how degradation/dissolution rates are implemented in the PISCES model.

Notwithstanding these comments, I suggest that the paper be published, given due consideration or rebuttal of the points mentioned above.

There are also minor suggestions: Table 1. What is the ‘number equivalent’. The statement in the text “the coefficients phi were obtained by integrating the standard curvilinearĚ” is rather vague. If these phi’s are single numbers, they could be given in the table 1.

Table 2. Parameter B is not dimensionless (m/d /(gˆ-1.17) To better describe the differences in the sinking models, it would be instructive to give minimal and maximal sinking speeds of each of the different formulations in this table.

Table 3. What are the ‘global burial fluxes below 1000 m’? Add global estimate of POC fluxes to sediments deeper than 1000m !

Did not understand in section 2.1: “the nutrient concentration is linked through a constant Redfield ratio”Ĕ and Ĕ “The cycles of carbon and nitrogen are decoupled by denitrification and N fixation.“ Surely denitrification and N fixation also decouples the concentrations of DIN and DIP - how is this deal with? In addition, N and C are also decoupled by air-sea exchange? How does the model include denitrification, if the sediment is coupled offline?

Equation 1: Left hand side of third part is wrong: (should be ..pocb->pocs)

What about the algae: do they sink or is it just the detritus?

Fig. 5. The data (a) and model results (b-f) should use the same picture of the world (i.e. the Atlantic view from fig 5 b-f rather than the Pacific view in fig 5a). It is already hard enough to distinguish differences, without the need to transpose one of the figures to make it compatible with the other figures.

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