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Interactive comment on "Swept under the carpet: the effect of organic matter burial in global biogeochemical ocean models" *by* I. Kriest and A. Oschlies

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General comments

In this paper, it is shown how inclusion of sedimentary burial of organic matter (OM) improves global biogeochemical ocean models. More specifically, models that include this particular benthic process, can have much higher deposition fluxes while still maintaining good correspondence with observed oxygen and nutrient data. This is a very important finding and will lead to improved global biogeochemical models. Also, the manuscript is very well written and documented, therefore I would recommend its publication.

However I also feel that this paper is too much written from a pelagic view, and ignores some important benthic processes.

The authors make a very good case for including permanent burial of (refractory) OM in the sediment in global models. However, they now 'sweep under the carpet' the mineralisation of reactive OM in the sediment. They do this by assuming that the sediment only stores refractory OM (burial), while any mineralisable OM that would enter the sediment now stays in the water column. This choice is remarkable, as (1) their models are equipped with a much better representation of benthic-pelagic (BP) coupling and (2) the authors themselves give in their MS at least two reasons why this choice of benthic pelagic coupling is not a good one. I realize this is a lot of work, but I would argue to rerun the burial models with the instantaneous reflux boundary.

Finally, an important question that remains to be resolved is how to reconcile the discrepancy between pelagic and benthic observations. As is shown by the authors, improved fits of benthic flux measurements lead to worse fits of pelagic flux data. Perhaps benthic and pelagic researchers should try to find a compromise as to what is a good estimate of OM deposition rates.

Specific comments

The pelagically oriented view is most clear when the authors choose a model for benthic-pelagic coupling. In their models, the OM that is not permanently buried simply stays in the water column. The various modes of BP coupling were reviewed a long time ago by Soetaert et al (2000 - Earth-Science reviews, 51, 173-201.), showing the pros and cons of various BP coupling formulations, including the reflective and instantaneous flux boundaries. There it is argued that the reflective boundary model has some drawbacks lacking in the instantaneous flux model.

The authors have two arguments against the instantaneous remineralisation BPC model.

- First argument (p 10881, line 10-15) is 'that this would imply an extremely active benthic fauna'. It is true that an instantaneous remineralisation model overestimates temporal variability in benthic fluxes, but the temporally integrated fluxes will be correct. It is just the time lag introduced by the sediment response and the amplitude that is not correctly represented. Clearly the lack of reproducing short-term temporal variation in benthic fluxes will not have an effect on a model that is only concerned with long-term steady-state trends, that has very thick pelagic boxes and that is not concerned with faithfully reproducing daily variations.

- They also argue that the reflection of OM with benthic burial leads to more reasonable nutrient and oxygen concentrations, compared to the model which has instantaneous flux without benthic burial. This is straightforward, and caused by the ignoring of the burial part in the instantaneous flux model, not due to the instantaneous representation of benthic mineralisation.

The authors give several arguments in their MS that in fact show that their choice for a reflective Benthic Pelagic coupling is not a good one

- A major drawback of a zero-flux, reflective benthic boundary is that it keeps OM in the water column, so that it can be transported with the currents and mineralized elsewhere. As in reality this OM would be removed from the water and mineralised on the spot, these reflective models will lead to too high export production and too high OM concentrations in the water column. Page 10861: 'sinking detritus may accumulate below suboxic zones and, according to our own sensitivity experiments, may reach unrealistically high concentrations.' Page 10881, line 18. . . . ' the models are accompanied by elevated deep detritus concentrations'. Clearly, keeping degradable OM in the water where it should in fact be 'locked away' in the sediment could/will create these high concentrations.

- Secondly, the authors state that 'defining the model's counterpart to benthic mineralization is not straightforward' (p10872, line 15);. This is true for a model where all

mineralization takes place in the water, but not for an instantaneous (rebound) flux type of model, where this comparison is much more straightforward. It would simply be equal to the long-term average oxygen consumption of the sediment.

p. 10877 about benthic flux measurements 'unfortunately these observations are very sparse'. .. This is true, but fortunately, temporal variation is rather small for benthic fluxes, so any measurement gives a rather good representation of benthic mineralization. Also, to overcome the 'methodological constraints' of restricted benthic data, and have good comparison between model and observations, the authors might consider comparing their benthic oxygen fluxes with Jahnkes paper (1996), and subsequent papers from other authors which gives local or global estimates of sediment oxygen consumption rates.

On page 10877, the authors downgrade the usability of benthic data over pelagic fluxes. There has been much discussion about the usability of sediment traps but the authors argue that it is "beyond the scope of this paper to discuss the methodological problems". However, the MS is clearly a good place to discuss methodological problems with respect to the benthic measurements, which, according to the authors measure just 'artifacts', either due to measurements (p 10877, line 16 and line21) or due to the presence of sedimentary animals (line 16). Artifacts induced by retrieving cores on deck are restricted to high water depths. In situ measurements do not have this artifact. Also, I would not call activity of benthic animals an 'artifact', as they are part of the sedimentary ecosystem, and they DO respire. The argument is not that measurements that include animals are overestimates, but that respiration rates based on using diffusive modeling should be considered minimal estimates, as they do not consider the activity of these animals. Also, short-term incubations are good estimates of steady-state mineralization, as sediment animals and bacteria mainly feed from OM in the sediment; the pool of which is very high compared to the instantaneous fluxes, and usually only the oxygen decline at the start of the incubation period is used. As a more benthic oriented researcher, I would attach more belief on benthic fluxes rather

than on pelagic fluxes.

Small comments.

It is surprising to read in the abstract and introduction how benthic fluxes may affect golobal C, O and N fluxes (p 10860, line 2), which is then tackled by a P-based model. There are good reasons to use a N-model, not in the least the oxygen consumed in nitrification, which cannot be well represented in a P model.

p. 10863, line 5, '. .. below 4 mmol O2/m3 remineralisation of OM continues but does not use any oxygen'. This is a strong simplification, of which the implications are difficult to assess. Of course, oxygen consumption may cease in the absence of oxygen, but instead reduced substances will be produced and accumulate in the water. Upon mixing with oxygenated waters, these will be oxidized, consuming oxygen, and this will occur at places at distance from where they originated.

p. 10866. ' we did not use direct P-discharge rates, as they may exhibit a too strong anthropogenic influence". True, but I would like to see then how the modeled external inputs correspond to these discharge rates. At least the model should be in the ballpark of the data, or lower than current anthropogenic inputs.

p. 10866, Unclear how long the model ran before it was deemed to be at 'steady-state' (it says only in the abstract – over millennia). Was there some steady-state stopping criteria? P 10875 line 19: overestimate? This is not what I see from fig 9.

Small typos 10863 line 18 'the the' 10864, l 25 'from our of our' 10868, term WOA05 not explained (add acronym to line 26, p10867).

Interactive comment on Biogeosciences Discuss., 10, 10859, 2013.