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Discussions

Interactive comment on "Dissolved inorganic carbon and alkalinity fluxes from coastal marine sediments: model estimates for different shelf environments and sensitivity to global change" *by* V. Krumins et al.

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General Comments

This paper was very enjoyable to read, especially in light of recent interest in the role of ocean margin sediments on the global ocean carbon cycle, coastal ocean pH and coastal ocean acidification. The modeling approach is well presented, and the discussion of results is very insightful, giving attention to both mechanistic explanations of small-scale sediment processes as well as big picture implications on a global scale.

Some important parameters may deserve more attention in the sensitivity analysis, and necessary simplifying assumptions allow only a first order extrapolation to the global scale. But these limitations are to be expected, given the complexity of coastal sediments, and may be refined here or in later studies.

Specific comments

1. My biggest concern with the results is the large sulfide effluxes, sometimes even larger than O2 influxes, as shown in the supplementary data. This seems unrealistic, and suggests that the model is missing a lot of acidity production from sulfide reoxidation, which, when occurring in surface sediments, certainly has a large effect on carbonate dissolution (section 3.2.1). It seems unlikely that this sulfide flux gets reoxidized in the water column, and not in surface sediments, as the authors suggest (p. 8504, l. 1). It is commendable that the authors address this issue in sections 2 and 3. However, while correcting AT for escaped sulfide in AT* may be more realistic than neglecting it entirely (p. 8494 l. 19), we're still missing the large effect on carbonate dissolution.

One possibility is that sulfide reoxidation is underestimated by the model because the authors may have significantly underestimated O2 inputs into surface sediments by advection and dispersion, and only account for diffusion and biological mixing. The authors mention that advection in permeable sediments is beyond the scope of this study (p. 8510, l. 28). However, the large sulfide effluxes bring to mind questions about modeled oxygen penetration depths (OPD) – how do the model-derived profiles and OPD compare with observations? For this reason, it may be useful to include porewater solute profiles (omitted, see p. 8498 l. 7) and information about OPD in the various simulations.

Finally, I disagree with the statement on p. 8510, l. 11-12. I would argue that the model representation of sulfide reoxidation (and its sensitivity to the kinetic rate constant and OPD) is an important weakness which has a large influence on the uncertainty in alkalinity fluxes, especially due to carbonate dissolution.

2. In general, the model description (section 2.2) is very complete. However, a few important details are missing:

p. 8485, l. 2 – The spacing of depth intervals is irregular, presumably with smaller intervals closer to the sediment-water interface. Can you specify the discretization function or at least the range of interval depths (ie. 1 mm at the sediment-water interface, 5 cm at 50 cm depth)?

p. 8485, l. 12 – Is bioturbation held constant until 10 cm depth, or does Db decrease with depth in the sediment?

3. On p. 8487 l. 19, the authors refer to “relative yields”. It is unclear, until I referred to the reference cited (Thullner et al. 2005), that this refers to cell yield. It could also refer to the thermodynamic energy yield. It may be best to be more specific in the wording.

4. It is a bit disheartening to see that benthic calcification, e.g. by coralline algae, in reef sediments is not considered. The authors mention that non-biogenic calcification is not considered (p. 8488, l. 24), but it seems that neither is biogenic calcification in surface sediments. On the same thread, calcification in surface sediments is linked to benthic primary production, which would also have a significant negative impact on net CaCO3 dissolution in sediments because of DIC consumption in surface sediments. Of course, this is all beyond the scope of this manuscript, but it is worth mentioning in section 4.

5. On p. 8505, l. 13, the authors discuss the implications of reducing the bioirrigation flux of sulfides, which would greatly increase calcite dissolution. Since sulfide most likely gets oxidized on burrow walls, rather than being transported out into the water column, I would argue that this should be the “baseline scenario”, rather than an alternative.

6. Many figures include bar graphs with two bars for each region (e.g., reefs), one for

redox reactions and a second for PIC dissolution or burial. This is a rather confusing way to present results. An alternative kind of plot, or a better explanation in the Figure captions would help.

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