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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.

Anonymous Referee #2

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General comments This is a solid model evaluation of global benthic total dissolved inorganic carbon and total alkalinity fluxes constrained by the current best estimates of the major controlling factors. The investigation and manuscript are excellent and deserve publication. However, in general, I think the manuscript could benefit from additional comparison between the current model results and observational studies where appropriate. Furthermore, as a number of generalizations are made in terms of fluxes to the four different environments, which in reality are highly heterogeneous in space and time, I think additional discussion and recognition of this variability might be

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warranted at the onset and throughout the manuscript. If this natural variability were considered, would it be important and how would it affect the model results? Also, I suggest that the authors consider using a quantitative assessment of their sensitivity analysis. Nonetheless, this is a very good manuscript and it was a pleasure to read it. My comments are mainly for the authors to consider to improving an already good manuscript.

Technical comments p8482, line 3. Need clarification. Coral reefs are most productive on an area basis and constitute about half of the total CaCO_3 production in the global coastal ocean (see Milliman, 1993; Milliman and Droxler, 1996).

p8482, line 15. In-situ dead coral reefs? Are there dead reefs that are not in situ?

p8484, line 15. Insert space in units for linear burial rate.

p8484, line 19. Delete space between greek symbol micro and cm.

p8485, line 1. What is the justification for using 50 cm sediment depth and not deeper or shallower? What is controlling the reactive depth in the natural environment? Does the choice of depth play an important role in affecting the model outcome?

p8486, line 12. A substantial proportion of the organic matter deposited in the coastal ocean is of terrestrial origin and probably do not follow Redfield ratios. In the current model, what is this proportion of this? Also, does Redfield ratios hold in coral reef and carbonate environments? How would changes in the Redfield ratios affect model outcome?

p8486, line 18. Provide typical range of pH observed in pore waters.

p8488, line 9. If you are using concentrations you should be using the stoichiometric solubility product (K_{sp}^*) for aragonite and calcite and not apparent solubility products (see e.g. Zeebe and Wolf-Gladrow, 2001). For Mg-calcite you have to use constants determined at stoichiometric saturation combined with total activity coefficients (see Andersson et al., 2008, Marine Ecology Progress Series).

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p8489, line 24-25 and throughout ms. Need to be consistent with units throughout ms (e.g., mol m⁻² yr⁻¹, mol C m⁻² yr⁻¹, mol CaCO₃ m⁻² yr⁻¹, mol yr⁻¹) so it is clear what you are referring to.

p8490, line 17. What is the average Mg-calcite composition?

p8490, line 19-20. Are there evidences for this in the literature? Light availability is probably a bigger constraint, which is as a function of both depth and turbidity. Please discuss/provide justification.

p8491, line 1-3. Can you assume that Fe deposition homogenous among the different environments? What are the main sources of this Fe?

p8491, line 13-14, 22. Should use stoichiometric dissociation constants.

p8494-8495, equation 15 and 16. Pyritization appears in two places in the combined equation that would cancel out. How are they different?

p8495. I suggest considering using a quantitative assessment of the sensitivity analysis (see for example Andersson et al., 2005, American Journal of Science).

p8496, line 9-10. It would be preferable to introduce the different sources of Fe deposition when you introduce the parameterization of this flux.

p8497, line 14-19. Coral reefs experience significantly different seawater CO₂ chemistry relative to the open ocean due to biogeochemical processes modifying the seawater chemistry including large diurnal variability (see for example <http://www.pmel.noaa.gov/co2/story/Coral+Reef+Moorings>). The same is true for near-shore environments in general. Somewhere you need to discuss the fact that the model does not take this into account and what it means for model outcomes. For example, you did not observe seawater undersaturated with respect to any carbonate minerals in the acidification scenarios, but if you accounted for the diurnal variability that is typically observed perhaps this would have been the case?

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p8498, line 7-8. I suggest showing results.

p8498, line 20-23. Reflecting my ignorance of this topic, I find these results somewhat surprising. How do they compare with observational studies of sediment diagenesis and organic matter decomposition?

p8499, line 8-9. Clarify whether you refer to depth in sediments or water column. Although it becomes apparent at the end of the sentence you could easily state “. . .sediment depth-resolved. . .”

p8499, line 13-26. This section is somewhat confusing. Explain why less POC depositional flux is required to initiate dissolution of aragonite on shelves than what is required to initiate dissolution of 15 mol% Mg-calcite on reefs despite the fact that the Mg-calcite is more soluble.

p8503, line 23-p8504, line 3. I suggest emphasizing the distinction between flux and generation earlier in the manuscript as I was somewhat confused from the part when you first presented the generation of total alkalinity from sulfate reduction until this part when it becomes apparent that a significant proportion is reoxidized.

P8506, line 4-8. It has previously been shown that alternating the rate constant (k) has a relatively small effect on the dissolution outcome, but the reaction order (n) has a significant larger influence (e.g., Andersson et al., 2005, American Journal of Science). It may make sense to alternate the reaction order based on estimates in the literature rather than a fixed percentage of the rate constant.

P8506, line 9. What global change? I suggest referring to “global environmental change.”

P8507, line 1. This statement is a classical model statement and has little relevance to reality where dissolution occurs when the seawater or pore water saturation state becomes less than 1. It clearly depends on the boundary conditions. I suggest connecting this result to reality or introduce qualifying statements about what it means.

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P8508, line 15-16. Why does dissolution increase in the ocean acidification scenario? Please explain.

P8509, line 16-23. I very much like that you have a section dedicated to model limitations but perhaps a little bit more effort could be dedicated to this section. As you mention, one of the big shortcomings of the model is that it doesn't capture spatial and temporal variability, but how would the model outcome be affected if you considered this variability? For example, it has been well illustrated that seawater CO₂ chemistry in near-shore and coral reef environments vary significantly on diel timescales (see for example <http://www.pmel.noaa.gov/co2/story/Coral+Reef+Moorings>). How would this affect your dissolution estimates?

P8511, line 1-4. See recent studies by Burdige et al. (2010) and Bradley Eyre and his colleagues (e.g., <http://www.scu.edu.au/coastal-biogeochemistry/index.php/18/>).

P8511, line 12. I suggest referring to global environmental change.

Table 6b. Double check that the numbers are correct. Higher aragonite saturation state associated is with lower pH in reef environments.

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