

Interactive comment on “The role of alkalinity generation in controlling the fluxes of CO₂ during exposure and inundation on tidal flats” by P. A. Faber et al.

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This is a very interesting study examining the processes controlling carbon fluxes from tidal flats that undergo periodic exposure and inundation. The experimental work and the modeling effort nicely compliment each other and tell a good story. There are, however, a number of places where clarification, some corrections and/or more detail is needed. None of the issues raised below should be “fatal” flaws to the manuscript, and once it is revised, the manuscript should be acceptable for publication. The comments below are keyed to (page, line number[s]).

1. (5448, 10-11) – The word “sediment” is on both sides of the parenthetical statement

C1955

with the grain size information. I don't think the second one is needed. 2. (5450, 10-11) – If the sediments were sieved to remove macrofauna (5448, 14), why is bioturbation included in the model? 3. (5451, 9-10) – What was the basis for a 75%/25% partitioning of the organic matter into a fast-decaying and a slow decaying component? 4. (5451, 22) – I was surprised there was no discussion in this section about how carbonate chemistry is handled in the model equations. If this is presented in an earlier paper then only a brief discussion is necessary (something akin to the length of the discussion in section 2.2). If it's not presented elsewhere, it needs to be clearly presented here. 5. (5455, 16-17) – “dissolved organic carbon” is by itself not a process. This needs to be clarified. 6. (5455, 23-25) – Net alkalinity production is the balance between gross alkalinity production by, e.g., the reaction listed here, and alkalinity consumption/acid production, by e.g., sulfide oxidation. Thus I'm not sure that strictly speaking this sentence is correct. 7. Related to this, I wonder if for completeness there needs to be a brief section on alkalinity consuming reactions. These are discussed throughout the rest of the text and are an equally important part of the story being told here. 8. In section 4.2 I think it is important to note that here you are really talking about net processes, since any alkalinity production during inundation is offset by alkalinity consumption/acid production that may occur when the sediments are exposed and O₂ input is enhanced. In this experimental study, early on alkalinity production is greater than consumption, but later on in the experiments (as is noted on lines 23-25) the balance shifts and consumption exceeds production. Explicitly talking about net processes associated with the dynamics of the system would, in my opinion, improve the overall discussion and more strongly point out the links between physical processes (periodic exposure and inundation) and biogeochemical reactions in the sediments. 9. (5456, 23) – Is this H₂S stored in the pore waters? If so, you may want to be more explicit. 10. (5456, 23-24) – I assume “This flux” is an alkalinity flux. Again, you may want to be more explicit. 11. (5457, 8) – The linear relationship mentioned here is not evident from Fig. 4 (although we can see the positive relationship between the two). I'm not sure if an additional figure is needed to show this, or if the authors can

C1956

simply re-write this sentence. 12. (5457,14) – I’m not sure what “In the first instance” means here. 13. (5457,15) – Where are the model results showing alkalinity production (flux ?) increased as piston velocity decreased? This seems like an important result that should be shown. 14. (5457, 20) - It seems a little odd to talk here about the “remainder” of the simulations, since they have already been discussed. Might it better to move section 4.3 up in the text? 15. (5458, 3) – “exported to the atmosphere versus alkalinity” is an incomplete phrase. 16. (5458, 11) – Which profile are you referring to? 17. (5458, 15) – I think I would be explicit that this sink for sulfide is the sediment pore waters. 18. Figure 2 a. Are there any significant difference in the observed behavior here between the +OM and -OM incubated cores, or in the model simulation before and after OM addition? Is it worth discussing this (regardless of the observations)? b. Rather than “Lab experiments” I would say “incubated cores (this study)”. 19. Figure 4 – The model treatments are referred to differently in the text (5457, 8) and in the figure caption. While its pretty obvious which is which, it might be better to be consistent within the manuscript.

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