

Interactive comment on “The impact of intertidal areas on the carbonate system of the southern North Sea” by Fabian Schwichtenberg et al.

Anonymous Referee #2

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The authors examined the impacts of alkalinity export from the Wadden Sea tidal flats on the carbonate system in the southern North Sea, mainly using a digital modeling method. The topic is interesting, and the result explanation looks fair. However, I find one of their references (Pätsch et al., 2018) had demonstrated the same issue using similar or even the same digital model. So the novelty should be further refined. Also it is difficult for me to follow the manuscript, due to the poor organization of the text and the insufficient annotation of charts.

Major concerns

1. The study area is unclearly defined. As an Asian reader, Figure 1 is quite unfriendly for me. For example, where are "the German Bight as well as parts of the Danish and the Dutch coast" (lines 94-95)? Also the Wadden Sea is strange for me. After a

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internet searching, I know that the Wadden Sea is the largest tidal flats system in the world (<https://www.waddensea-worldheritage.org/>). Since the Wadden Sea is a key area in this study, its geography should be clearly introduced to readers. I suggest that a striking section or subsection of "Study area" should be set up, after the Introduction. In this section or subsection, more geographical details and biogeochemical knowledge should be presented. Some contents of the current subsections 2.6.1 and 2.9.1 could be integrated in the subsection of Study area.

2. The model structure and settings are unclear. A structure diagram is needed. As for the the submodule HAMSOM and the original ECOHAM model, some details are needed here, although their "details were described by Backhaus & Hainbucher (1987) and Pohlmann (1996)" (lines 107-109). At least their background and assumptions and fundamental structure and application strengths and limitations should be introduced. I wonder whether it is specially designed for the area under study. This information is also critical for general readers. In the current subsection 2.8, the authors said that "The main extension in the present study was the introduction of a prognostic treatment of TA (Pätsch et al., 2018)" (Lines 191-192). I wonder whether they give any modification on Pätsch et al. (2018) treatment.

3. How did the authors plot Figure 2? There is no relevant information (such as data source) in both the figure caption and main text. Since Figure 2 is the key to distinguish the two scenarios defined in this study, this information is a must to be clarified.

Some minor comments and suggestions

1. To avoid confusion, please unify abbreviations for North Frisian coast (N or NF), East Frisian coast (E or EF) and Jade bay (J or JB). "The respective areas 1-3" in line 350 also refers to the three regions?

2. What is "yr" in Equation (1)? Please clarify.

3. Lines 440-441: Why does this partly compensate the missing TA generation by

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benthic denitrification? Please explain.

4. Line 453: Carbonate dissolution cannot be counteracted by DIC additions. DIC additions (mostly refer to free CO₂) are usually in favor of carbonate dissolution.

5. Lines 534-535: Why was Riv_eff not taken into account for the budget calculations? I notice that the authors mentioned it to the later discussion, i.e. lines 557-559 "Summing up the source and sinks, Wadden Sea exchange rates, internal processes and effective river loads resulted in highest sums in 2002 and 2003 and lowest in 2009".

6. Line 552-556: How to get those percentages (47%, 10% and 59%) based on results in Table 2. Please clarify.

7. Lines 615-616: The reduction of TA here is associated with the oxidation of ammonium to nitrate, instead of the change in DIC. Refers to Zhai et al. (2017, <https://doi.org/10.1016/j.ecss.2017.08.027>).

8. Lines 617-619: The TA/DIC ratio of denitrification is not 1 but 0.8. The TA/DIC ratio of sulphate reduction is not 2 but 1. Refers to Sippo et al. (2016), Are mangroves drivers or buffers of coastal acidification? Insights from alkalinity and dissolved inorganic carbon export estimates across a latitudinal transect, *Global Biogeochemical Cycles*, 30, 753–766, doi:10.1002/2015GB005324.

9. Lines 619-620: The authors mentioned that aerobic and anaerobic oxidation of upward diffusing methane were not considered in present study. How to relate this statement to line 633 "When sulphate reduction associated with organic matter and/or methane oxidation and pyrite burial became the dominant processes...".

10. Lines 624-638 and Figure 9: TA/DIC ratios of <-0.16 in regions under study may indicate other processes than aerobic decomposition (-0.16) and anaerobic reaction (>0). What are them? I would like to suggest the authors mention more possible processes at the beginning of Section 4.4.

11. Table 2 is discussed after all other tables. Please change the order of the tables.

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Additionally, Table 6 can be shifted to the Appendix.

12. Are those TA/DIC ratios presented in Figure 9 mean values in the given regions? Please clarify. Also I would like to suggest the authors compare all data in grid cells with the typical stoichiometric ratios of biogeochemical processes. Refers to Figure 4 in Sippo et al. (2016), *Global Biogeochemical Cycles*.

13. The "Wadden Sea tidal flats" should appear in the title.

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