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Interactive comment on "Interannual sedimentary effluxes of alkalinity in the southern North Sea: Model results compared with summer observations" *by* Johannes Pätsch et al.

Anonymous Referee #1

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GENERIC COMMENT the paper presents an interesting implementation of a vertically structured benthic model to estimate the alkalinity fluxes from the Southern North Sea sediments. This is a challenging topic that needs to be addressed and authors are commended for this. The methods are generally sound, with some clarification needed and some suggestions for improvements provided. The results are presented clearly, the discussion could benefit of some more in-depth analysis, particularly on the role of pelagic primary productivity and on the relevance of the alkalinity fluxes for the entire ecosystems.

SPECIFIC COMMENTS: section 2.2.5 and table 1: why I appreciate that turnover rate

for deep ocean would not be suitable for shelf seas, authors did not specify how they define the new values, if via calibration (against which observations?) or with literature (which references?).

Lines 144-146: authors assumed that in advective sediments the coefficient for diffusion is increased tenfold. They state that this has been determined by several sensitivity analysis, but they did not state what were the criteria of the sensitivity analysis (stability? calibration? Something else?) more details are needed

lines 175-178: authors claims that a reduction of 10% of riverine input of nitrogen corresponds to a "pristine" scenario without anthropogenic influence. Authors cite a paper to corroborate this assumption. However it seems to me that 10% is a bit of an underestimation for such and industrialised area.

Line 179-183: do the rates in the "plate run" scenario are comparable with those of table 1? I appreciate that the equation will be different and therefore the value of the parameter, but reporting these for a comparison would help in understanding how much of the difference is due to model structure and how much to simple parameter values

line 220 and following. I'm not sure that providing a point-to-point comparison on a single day is the more effective way to assess the model. Small shift in phenology (not rare in coupled biogeochemical models) could result in a significant error that could not be related to the benthic model rather to error in the physics or in the forcing. I would suggest to compare the observations with a longer temporal means (monthly?) and to discuss the uncertainty. Also, while visual comparison can be appealing, they are not much informative: I suggest to provide also measures of the actual fit. For example, in relative error term, I'm not sure figure 6 shows a much better fit.

Section 4.2: authors seem to suggest that the strong undersaturation of pCO_2 in the German bight is driven by the strong alkalinity fluxes from benthos. I'm not entirely sure that the simple co-location of the two is enough to establish a causal link. For instance what's the role of pelagic primary production (PP)? A high PP could explain both the strong undersaturation (DIC is fixed into plankton) and the alkalinity fluxes (due to strong POM settling and associated processes in the benthic environment). Have the author tested to turn off the benthic fluxes of TA and check the consequences in the delta pCO2 signal?

Section 4.3: authors said that it's astonishing that the model simulates higher benthic-pelagic fluxes under higher porosity, when the diffusion coefficient is lower. Do authors have any suggestion on what are the mechanisms driving this?

Section 4.4: this section is important to understand the need for detailed model. However authors simply state the difference between the two models, without trying to tease out the reason behind that, particularly in regard to the difference in the seasonal signal

technical comment: please translate “gedankenexperiment” in English

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