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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 #2

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Review of the paper "Interannual sedimentary effluxes of alkalinity in the southern North Sea: Model results compared with summer observations" by J. Pätsch et al., submitted for possible publication to Biogeosciences.

The paper by Pätsch and coworkers deals with an important topic, namely on how coastal systems respond to increased nutrients input. Hitherto the focus of this discussion has largely been on the interaction between primary production, aerobic respiration and oxygen conditions with respect to changes of the overall CO2 pool. In recent times this focus has been broadened to further consider anaerobic metabolic pathways for the respiration of organic matter, which in turn affect both the overall CO2

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and and also alkalinity pools. The paper by Pätsch et al. analyses this issue for the shallow waters of the North Sea and provides compelling evidence for the dependency of metabolic alkalinity generation on nutrient runoff/input. Overall I think the paper is publishable, as it constitutes a major step forward in this field. I do have some remarks for consideration to improve the paper.

Specific remarks:

Figures: All figures should be designed that each panel does clearly reveal the property, its unit and magnitude, such the the reader can read the panel without referring back to the caption. In the present form, only Fig. 9 has been designed accordingly. The other figures are not usable without the caption, which makes it very difficult to follow the text, and might be in part even misleading, such as in Fig. 7, where the property (N) is given, but points to different species of the N cycle. I think this point is also important if any colleagues might use such a figure for a talk or teaching. The only way to prevent misunderstandings is to carry the information in each panel as well (even if redundant).

Page 2, line 4: loss of reduced sulfate products: this (mainly?) refers to burial of sulfides (FeS, FeS2). If so, please be specific.

Page 8, section 3.2, and related figures. While technically correct, it appears during the first and second read confusing to attribute aerobic degradation an alkalinity gain, and then separately name nitrification to a reduction of alkalinity. As this route is o pursued throughout the paper, please explicitly explain this here. Also a consideration might be to lump them together, if these processes always occur together. In other words, are there situations in this modelling study, where these two processes are not strictly coupled, which in turn would justify the need to treat them separately?

Page 9 lines 212-218. Calcium carbonate. To me the calcium carbonate discussion reads a bit like a closed loop argument, since apparently all hinges on the prescribed POC:PIC ratio. I have problems to see the need and justification for this discussion

as it appears to be arbitrarily(!) controlled by the choice to the POC:PIC constant. I' d recommended to tone down this discussion, and focus it on the point where it might be necessary, namely when attempting to explain the difference outcomes of observational (summertime) studies, and year-round modeling studies, the latter ones possibly closing the CaCO3 budget. Alternatively, an attempt could be made to adjust/establish that ratio to improve the pCO2 fields, as for example provided by Thomas et al. 2004. Another weakness of the fixed POC:PIC ratio is that CaCO3 production is reported to occur sporadically, and not necessarily in tandem with primary production, which in turn would diminish or vanish the assumed advantage over the observational records.

Page 11, lines 282-284: I think this statement could be strengthened by looking into coherence or lag-times between changes in NO3-runoff and extent of denitrification. The data are there, so an analysis in that direction should be easy to be carried out.

Page 11. lines 285-300.

I think this discussion needs to be rewritten to some degree, as I see major arguments missing, or not fully considered.

Why attempting to relate a 9-month accumulation to an in-situ observation of a seasonally varying property? While I can can see the reason for this, yet it has to be considered:

A: the residence time of the water at any given location. If the residence time is on average much shorter than the 9-month integration, the latter one does not make sense and should be shortened.

B: the entire concept only applies to vertically mixed regions. In stratified regions the zone of AT production is separate from the surface, thus if there is any impact it can be only visible, once, stratification breaks down in autumn (if at all). Also there are regions in the North Sea, which are permanently stratified. See for example Burt et al. 2014 (GBC) and observations of shortlived Ra isotopes in North Sea surface waters.

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C: for the more Northern regions: what about transport times scales and amounts? Is any of that alkalinity produced in the southern surface sediments transported northward and might have an effect on the pCO2 there? I am not sure whether this plays a role, but it might be more likely to occur than the vertical impact mentioned under B. (See Burt et al., 2016 L&O)

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