



EGUsphere, referee comment RC1  
<https://doi.org/10.5194/egusphere-2022-161-RC1>, 2022  
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## **Comment on egusphere-2022-161**

Xinping Hu (Referee)

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Referee comment on "Benthic alkalinity fluxes from coastal sediments of the Baltic and North seas: comparing approaches and identifying knowledge gaps" by Bryce Van Dam et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-161-RC1>, 2022

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Van Dam et al used three independent approaches, including Ra-224 decay balance, core incubation, and porewater profile fitting, to calculate/estimate benthic alkalinity fluxes in the sediment of both the southern North Sea and the western Baltic Sea. Part of the data used in this study have been published elsewhere. The authors also explored porewater stable carbon isotopes as well as the relationships between various parameters (alkalinity, DIC, excess  $\text{SO}_4^{2-}$ ) for possible reaction mechanisms, for example, likely carbon source to porewater DIC, processes responsible for DIC/alkalinity changes. The overall conclusion is that benthic alkalinity fluxes in the studied regions are substantially smaller compared to the results obtained from prior studies in these areas, even though the estimates do vary because of the different approaches taken in this work.

The manuscript is largely well written, and the authors have done a good job tying together both historical and more recent collected data and applying the three techniques to examine fluxes. The detailed geochemical analyses, for example the interpretation of porewater stable isotopes and apparent reaction stoichiometry, are very informative. That being said, this work on one hand lacks some details on the methodology in the geochemical analysis and modeling, for example not all reported porewater constituents have corresponding analytical methods ( $\text{NO}_3^-$ ,  $\text{K}^+$  etc) and none of the methods has precision information, and the parameterization of the PROFILE model seems to offer no context regarding where these values are from; it also seems to bog down in details of flux values of many constituents coming from different methods while there is little quantitative understanding of indeed how much benthic alkalinity is exported to the water column on a regional scale, other than the fact that the values are much smaller than thought. It may be of interest to readers to show the flux ratios of constituents that could be illuminating for understanding overall reaction stoichiometry (e.g., carbonate dissolution/precipitation) based on the PROFILE model calculations, and perhaps complement the discussion with both the stable isotopes and porewater ratio information, so the latter two do not necessarily stand alone. In the end, the authors stated that seasonality of this flux needs to be researched, among other things. However, given the fact that data from the four cruises already spanned different seasons, it is unclear why seasonality cannot be addressed here, or at least some effort can be taken in this work.

The PCA analysis is interesting, although it also provides little quantitative knowledge on understanding benthic fluxes other than showing that both study region and sediment particle size matter for benthic fluxes, which is not surprising but hardly unexpected. The choice of the input parameters also seems arbitrary and more contextual information is needed if the authors decided to keep this section.

In figure presentations, the authors almost exclusively used bar charts, and some of the figures (Fig. 4) uses fairly complex notation schemes. It will serve readers better if the authors could consider using correlation plots as an additional visual aid to compare and contrast values of the same nature but obtained from different means.

Below are some detailed comments:

Fig. 1 add coordinates axes to the map.

L112, spell out IOW even though it appears in the affiliations already?

L113, is it HCl too?

L133, "at IOW" appears twice.

Section 2.2, please list the precision for all constituents analyzed, even if they may have appeared elsewhere for example prior publications. Later in the text, for example Fig. 4, it seems that not all solutes are mentioned in this section.

Section 2.4–2.6 seem to be more appropriate as subsections of 2.3 (2.3.1, 2.3.2, and 2.3.3) because 2.3 lays out all three techniques but sections 2.4-2.6 elaborate them.

L179, only DIC and Ra were measured? In fact, sections 2.4-2.6 lack general information on what were collected and modelled. Even though the lab analysis section (2.2) mentioned analytical methods for porewater parameters, it is unclear whether all or parts of the parameters were used for all incubations/modeling studies. For Table 1, are these values part of the input? If so, how were the values obtained?

Fig. 2, there is no discussions on Ra-223 throughout the text, where does this information come from? In figure caption please note these sites are from the North Sea.

L236, if TA values are reported to the second decimal place, it would imply that the precision only reached 0.1 mM or 100  $\mu\text{M}$  at best as by analytical chemistry convention the last digit is used as an estimate, then the bottom-pore water TA difference of 2-4  $\mu\text{M}$  appears unrealistic, please clarify.

L237, briefly state the method that Voynova et al. (2019) used to inform readers.

L240-241, in Table 2, TA flux at the maximum 33.6  $\mu\text{mol}/\text{m}^2/\text{hr}$ , but the statement that two prior studies reported results "more in line" values is confusing. Please clarify as these values are nowhere close to what's reported in this section.

"237.5-275" should be either 237.5-275.0 or 238-275. Significant figures matter.

L248-249 and L254, are they the same thing? If so, merge to reduce redundancy.

L291, the larger  $\text{NH}_4^+$  flux may have organic matter breakdown component as well, see L300. If DNRA is an important process, some references to back it up would be helpful.

Fig. 5, it seems that the site label and data points are misaligned so it's difficult to see where some data points are from.

L343-344 is repeated in. L350.

L350 paragraph, Site 1 is said to have methane as the possible organic carbon source, here the authors suggested that shallow  $\text{O}_2$  penetration and high MSR together with sulfur recycling does not lead to net sulfate reduction. As this is a "mud" site, the interpretation seems counterintuitive as marine sediments of this nature in general would see a reduction in sulfate concentration (high MSR rate and low permeability).