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Interactive comment on "Biogeochemical processes and buffering capacity concurrently affect acidification in a seasonally hypoxic coastal marine basin" *by* M. Hagens et al.

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First of all, we would like to thank the reviewer for his/her positive and constructive feedback. In this Author Comment we will briefly discuss the minor suggestions for improvement given by the reviewer.

Reviewer comment 1

"The experiments undertaken were thorough and well designed, I agree they are likely to cover the key proton generating and consuming reactions, with the possible exception of nitrification which was modelled. Ideally this would have been measured, but I

accept that not everything is possible. . . . Perhaps the authors could make a statement as to the sensitivity of the proton budget to changes in the nitrification rate? Given the stoichiometry of 2 $[H^+]$ *produced for each mol of* $[NH_3]$ *oxidised, the uncertainty in this reaction could have a significant effect on the proton budget in November, which might be worth commenting on."*

In fact we tried to measure nitrification rates at each depth for the months of March, May, August and November. This was done in 4 h incubations using $^{15}NH_4^+$ isotope labelling, followed by extraction of ammonium via the diffusion method of Sigman et al. (1997). Using Devarda's alloy, all NO_x was then converted to NH_4^+, which was subsequently extracted and measured for its isotopic composition (Middelburg and Nieuwenhuize, 2001). From this, nitrification rates were calculated according to Veuger et al. (2013). Unfortunately, due to incomplete recovery of the ammonium in the first step of the extraction procedure we believe we cannot trust these measured rates. This was confirmed by simple budget calculations based on the total oxygen consumption rates from the oxygen light-dark incubations. Additionally, there are two methodological reasons why we believe that some of these (non-reliable) rates would represent potential rather than actual nitrification. First, in some cases $^{15}NH_4^+$ was added at levels well beyond 20% of the ambient $[NH_4^+]$, due to the lack of a priori knowledge on the ambient concentration. Additionally, we think that in August, during the period of hypoxia when availability of oxygen may have limited nitrification, $[O_2]$ in the incubation bottles was not maintained at its ambient level, i.e., $[O_2]$ was higher.

All of the above mentioned factors led use to decide to model nitrification rather than use the measured rates. A comparison between measured and modelled rate confirmed our suspicion. When neither O_2 nor NH_4^+ was limiting nitrification, the ratio between measured and modelled rates was relatively constant, between 20-30, which we believe is due to the incomplete recovery that affected all incubations in the same manner. At low ambient $[O_2]$ or $[NH_4^+]$, measured rates were up to 150 times higher than the modelled rates, confirming that in these cases the incubation conditions did not mimic in situ conditions. Disregarding this latter category, the measured rates show a similar Michaelis-Menten type dependency on [NH_4^+] as the modelled rates. This consistency in pattern between measured and modelled rates led us to believe that the modelled rates well capture the seasonal trend and are more reliable in terms of magnitude as the measured rates.

In the revised version of the manuscript we will discuss the uncertainties of the modelled nitrification rates and their potential effects on the proton budgets.

Reviewer comment 2

"Could the fluxes in figure 6 be shown a little more clearly, with the sites marked on the x axis for example? I don't really like colour coding to differentiate between sites, but I accept this might be a very personal taste."

In the revised manuscript we will carefully reconsider the lay-out of figure 6.

References

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Interactive comment on Biogeosciences Discuss., 11, 15827, 2014.