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Interactive comment on "Efficient removal of phosphorus and nitrogen in sediments of the eutrophic Stockholm Archipelago, Baltic Sea" *by* Niels A. G. M. van Helmond et al.

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General comments/requests (in random order):

1) The authors present bottom water nitrate as one controlling factor of benthic denitrification. What about nitrate formed during benthic nitrification as one controlling factor? Very little is mentioned about it. Please do, and discuss nitrification as a control of benthic denitrification in these sediments.

Reply: We thank the reviewer for taking the time to critically assess this work. We reply to all points raised below. In the original manuscript we presented the percent

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denitrification supported by nitrification in Table 4 (see 'nitrification-denitrification %') and in the text of the results section (3.3). We then discussed the potential effects of nutrient reductions on nitrification-dentification in the discussion (final paragraph of section 4.3). Our results show that nitrification is indeed an important in controlling denitrification. We agree with the reviewer that this can be discussed in more detail. We have now completely rearranged section 4.2.2 and added additional references on the importance of nitrification-denitrification.

2) Lines 421-436 and elsewhere: Denitrification rates decreased going seaward, and the authors explained this by lower bottom water nitrate concentrations and lower organic C content of sediment along the transect going seaward. However, it is generally assumed that in coastal and shelf sediments availability of nitrate controls denitrification rates since there often is no shortage of organic C in such sediments – at least not to limit benthic dentrification. Also, in coastal and shelf sediments, the nitrate consumed in denitrification is mostly produced during nitrification in the sediments rather than being nitrate from the bottom water (cf. e.g. papers by Seitzinger (et al.)). The 18 mm oxygen penetration depth in sediment of Ingaröfjärden should allow active nitrification so that the lower bottom water nitrate concentration there should not lead to a decreased denitrification rate. Could the authors please discuss in their paper this apparent inconsistency between what has been generally found and what was found in the present study?

Reply: See our reply to the comment above. We have completely rewritten this section and provide references illustrating that our findings are supported by the published literature on the role of organic C availability.

3) The measurements were made in March, which is late winter. This period should be among the most oxygenated of the year; the vertical stratification is weak and mixing/ventilation of bottom waters should be facilitated. In addition, bottom water temperature is at its minimum, the spring bloom has not started yet, and there should be very little fresh organic matter in sediments. Can the authors please include a discussion in

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their paper on this and especially on to what extent the results presented are representative on an annual scale? In my opinion, the results represent a late winter situation, and fixed N removal and retention of P in sediments most likely are very different than in e.g. summer-fall when bottom water oxygen levels and quantity of fresh organic matter in sediments can be completely different. The authors should make this very clear in their paper.

Reply: In order to highlight and more clearly discuss this aspect we now added a new sub section discussing the seasonality and the representativeness of our data on an annual basis (4.2.3 - Seasonal cycles of N processes).

4) Section 4.3 Implications: Although this section includes some interesting discussions, I cannot see that it is relevant in this paper. This section consists of discussions and speculations far beyond what can be found warranted based on the original results of this paper. This paper is not a review paper. Please focus the discussion, and the presentation of implications, on the results obtained in this study (carried out in March 2017).

Reply: In this section, we primarily wish to summarize what our findings imply for future expected developments in nutrient dynamics in the Stockholm Archipelago. We will modify this section to clarify this (including explicit references to our results).

5) Section 4.3 Implications: "...artificial reoxygenation of bottom waters (e.g. Stigebrandt and Gustafsson, 2007) will not be a long-term effective measure towards improving the water quality of the (coastal) Baltic Sea." Please explain how the results of this paper justify this statement. If the authors cannot convincingly do this, this statement should be removed.

Reply: See our reply to point 4. We will modify this sentence so that the focus lies on the Stockholm Archipelago. Our results show that better oxygenation leads to a larger surface sedimentary P pool. At the well-oxygenated site Ingaröfjärden, this pool is $\sim$5 times larger than at the site with the most reducing conditions (Baggensfjärden)

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– see Figure 9 of our initial submission (now Fig. 10). At depth, however, sedimentary P distributions and concentrations are rather similar at all stations (with exception of the enrichments in Fe-P at Strömmen), presumably also because of the presence of relatively high concentrations of sulfide in the pore waters at all sites. This suggests that there is relatively little control of bottom water oxygen concentrations on permanent P burial and thus removal. Therefore we think this sentence is important and warranted.

6) Lines 536-538: "Further reductions in P and N inputs are necessary to ensure a reduction in the frequency of hypoxic events. Eventually this will lead to a larger surface sedimentary P sink and will be key to maintaining the efficient N filter and avoiding additional P and N recycling." I agree that this is one important measure to improve the environmental status of coastal systems and that it should be done, BUT please explain to what extent the results of this study justify this final conclusion.

Reply: Please see above our reasoning concerning P burial and the associated changes in the text (i.e. focus on the Stockholm Archipelago).

In terms of N cycling, it is possible that denitrification may increase with more oxygen in bottom waters (e.g. by increasing the oxygen penetration depth and sediment volume for nitrification) although oxygen levels at the time of sampling were most likely the highest these sites experience year-round. It is challenging to predict how N cycling processes will respond to changing oxygen conditions – particularly when sediments are exposed for longer-term (weeks-months) in nature as opposed to short term (days) in laboratory experiments. It is likely (as discussed in the manuscript – and also now amended in the abstract) that denitrification will initially increase due to fresh organic matter inputs but then decrease in favor of recycling processes (i.e. DNRA) as NO3- is consumed and oxygen decreases as C/N ratios increase. Thus the sediments act as a source rather than a sink of N during summers (as shown in monitoring data).

We will rephrase the sentences brought forward by the reviewer so that it is (more) focused on the results of this study.

Other comments by line number:

Line 24-25: What other form(s) of P make up the remaining 50-70% (i.e. the major fractions) of P burial? Please make this clear already in Abstract.

Reply: We will modify this sentence in the abstract to: “Sedimentary P is dominated by Fe-bound P and organic P in the surface and by organic P, authigenic Ca-P and detrital P at depth.”

Line 31-32: Regardless whether this statement in general is correct, what evidence does this paper provide that this statement is correct? What level of removal or retention of N and P would have occurred in the studied area if bottom waters had been better oxygenated (natural or manmade)? I do not think this statement is warranted based on the results this paper presents, so I suggest it is deleted unless the authors convincingly can argue that it is warranted. See also above.

Reply: Please see our reply to point 5 of the reviewer above. We have revised this sentence and no longer mention “artificial reoxygenation” in the abstract. The line now reads: “We emphasize the importance of nutrient load reductions as a critical management strategy for N and P removal and for the recovery of eutrophic Baltic Sea coastal zones.”

Line 390: Remove “rate” in the beginning of this line.

Reply: We will remove the entire sentence in response to Reviewer 1.

Lines 412-413: “...and the increasing role of sediments as a NO3- sink along the estuarine gradient”. Please clarify this text. In which direction are you meaning the estuarine gradient goes? Landward or seaward?

Reply: The trend that we were describing here is seaward. Based on comments 1, 2 and 3 this part of the discussion will be modified. We will make sure to clarify the direction of the trends in the revised manuscript.

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Lines 414-415: "...reduction in organic matter quality as shown by a concomitant reduction in surface sediment N and organic C contents". Does organic matter quality necessarily go down when contents of N and organic C go down? Please explain.

Reply: This sentence will be removed due to rearrangements/changes to the N cycling sections.

Lines 440-451 (and later in Discussion): What did Bonaglia et al. (2017; BG) report on DNRA and its importance as a nitrate reducing process in Baltic sediments?

Reply: Text will be added describing the co-occurrence of denitrification, anammox and DNRA in sediments of the Bothnian Bay in the discussion (section 4.2.3) and the reference to Bonaglia et al. (2017) will be added to other relevant sections in the discussion.

Lines 534-535 (and elsewhere): "Combining our process measurements with available monitoring data, it is likely that N in the Stockholm Archipelago undergoes seasonal cycles of removal and retention." What do you mean with "retention" of N here? Did you intend to write recycling (e.g. through DNRA)?

Reply: This should indeed have been "recycling" and will be amended in the revised manuscript.

Table 4 head. "DN is nitrate supplied from nitrification". DN does not exist in the Table, so why is it mentioned in the Table head?

Reply: "DN" was part of an earlier version of the manuscript, which we decided to remove in the writing process. We, however, forgot to adapt the heading of Table 4. We will adapt the table heading in the revised manuscript.

Table 4: Annamox is incorrectly spelled. Should be anammox.

Reply: This will be corrected in the revised manuscript.

Reply: References:

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Bonaglia, S., Hylén, A., Rattray, J.E, Kononets, M. Y., Ekeroth, N., Roos, P., Thamdrup, B., Brüchert, V, and Hall, P. O. J.: The fate of fixed nitrogen in marine sediments with low organic loading: and in situ study. Biogeosciences, 14, 285-300, https://doi.org/10.5194/bg-14-285-2017, 2017.

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