Review comments on "Particulate organic matter controls benthic microbial N retention and N removal in contrasting estuaries of Baltic Sea" by Bartl et al.

Dear Reviewer,
Thank you very much for your valuable and detailed comments and suggestions. In the following you find the responses to your comments and the changes we will apply to the manuscript (italic).

1. Line 34: A reference is needed for the last part of the sentence.

Changes (underlined) from line 34 ff:
Human nitrogen (N) utilization, especially in agriculture (Galloway and Cowling, 2002; Rabalais, 2002) has strongly increased riverine N inputs into coastal zones (Howarth et al. 1996), resulting in the eutrophication of coastal waters (Nixon 1995, Howarth & Marino 2006).

2. Line 85: Patsuszak et al. (2012) is used twice. Does this same reference give two different conclusions? If not, then two references can be given i.e. Patsuszak et al. (2012a) and Patsuszak et al. (2012b).

We apologize for this mistake. The first reference in line 85 will be deleted.

3. Section 2.1: This study emphasizes N transformation processes such as nitrification and denitrification in the benthic boundary layer which are sensitive to O_2. But O_2 regime of the estuarine water columns are not described. The authors should describe the variability of oxygen condition of two estuaries throughout year or at least from spring to summer based on previous study or this study.

The oxygen concentrations are indeed important and have been reported in this study in lines 241-242. We emphasize them more in the study area description and report the changes to the text below:
line 100 ff:
The water column of the Öre estuary is oxic throughout the year, ranging from ~250 µmol L^{-1} in summer to ~450 µmol L^{-1} in spring (monitoring data from dBotnia 2016; SMHI 2016).
line 106 ff:
The water column of the Vistula estuary is oxic throughout the year with small seasonal differences, i.e. lower oxygen concentrations in summer than in winter or spring (Bartl et al., 2018). Under specific conditions (floods, high organic matter mineralization under stagnant stratification) coastal hypoxia has been observed in the Vistula estuary (Conley et al., 2011; SMHI 2011). However, these were not met during our study.

3.1 Line 121: .....to fill 30% (silt) to 50% (sand) of each one.....What does this sentence mean? It sounds confusing. Please clarify by rephrasing the sentence.

The reviewer is correct that the percentage plus sediment type leads to confusion.
Changes from line 121 ff:
Subsamples for denitrification rate measurements (n = 12 per site, except VE I: n = 20) and pore-water oxygen profiles (n = 3 per station) were collected in acrylic cores (iØ 2.3 cm, length 20 cm, except VE I: 15 cm). These were pushed gently into the sediment so that they were filled to 30% of volume with sediment in silty sediments, and to 50% of volume in sandy sediments. The remaining volume was overlying water. The cores were then closed without gas headspace.

4. Section 2.2.1: Was O_2 measured in the water column? If yes, then how? By sensor coupled with CTD or measured analytically? Please give a brief description.

Changes from line 127 ff:
In the Vistula estuary, water column measurements were carried out with a Seabird CTD-system (Seabird 911plus, Seabird Scientific) equipped with sensors for the measurement of the dissolved oxygen concentration (SBE43, Seabird-Scientific) (Bartl et al., 2018). In the Öre estuary, water column measurements were carried out by a Seabird CTD (SBE19plus, ÖE I; SBE19plus V2, ÖE II; Seabird Scientific), and oxygen was measured by an optode (4330, Aanderaa) attached to a Seaguard-CTD (Aanderaa). Oxygen concentrations from the sediment overlying core water were determined via Winkler-titration (Grasshoff et al., 1983; Winkler, 1888).

4.1 The authors should mention the thickness of BBL for both the estuaries in both spring and summer.
Line 129-130: Water samples were.....If BBL thickness is just 20-40cm (If I understand correctly from Line 115) and sampler length is 0.5-1m. Then, how can you possibly say that the water sampler was completely inside BBL? Apparently, the sampler could also enclose the water above BBL.

Reviewer#1 also commented on the BBL thickness and we will describe the determination of the BBL thickness in more detail. The vertical extent of the BBL is given in Table S1.
Changes line 128 ff:
The BBL is defined as the water layer directly above the sediment (Richards, 1990) and is characterized by high turbulence and mixing, which are typically fueled by bottom friction (Dade et al. 2001; Grant and Madsen, 1986; Thorpe, 2005). As turbulence and mixing lead to invariant values of potential density (σ_θ) within the BBL (Turnewitsch and Graf, 2003), the vertical extent of the BBL can be determined based on the variation of the potential density ($\Delta\sigma_\theta$), i.e. the change of potential density over the change of depth. Thus the vertical extent of the BBL is defined by the lowermost point in the water column where the variation of the potential density exceeds a threshold of $\Delta\sigma_\theta < 0.01$ kg m^{-3} (according Holtermann et al. 2012). Sediment overlying water taken from sediment cores (20 – 40 cm) were always within the BBL. Bottom water samples taken with Niskin bottles (0.5 – 1 m length) could only be considered as BBL samples in 57% of all sampled stations, because the vertical extent of the BBL in Vistula and Öre estuary ranged between 1 m and 7 m (Table S1).

5. Line 163: The authors mentioned that porewater was extracted at 2 cm interval from 5 cm to 11 cm depth by Rhizon tubings. But Seeburg-Elverfeldt et al. (2005) says that Rhizon tubings can extract porewater with a vertical resolution of 1 cm only. Please explain.

Seeberg-Everfeldt et al. (2005) recommend a vertical resolution of 1 cm as highest possible resolution when sampling pore-water with rhizons. This means an interval of < 1 cm should not be applied because then the pore-water catchment area of the single sampling depths would overlap and thus bias pore-water nutrient concentrations. However, an interval of > 1 cm is not problematic. At sediment depths > 5 cm, ammonium concentrations generally show a clear increasing trend in coastal Baltic sands and muds (Bonaglia et al., 2014; Lipka et al., 2018; Lenstra et al., 2018; Thoms et al., 2018) which can be well captured at a resolution of 2 cm intervals.

6. Section 2.3.1: It is not clear whether 100-170ml from BBL and 625 ml from water column were mixed together prior to $^{15}NH_4^+$ enrichment or they were separately enriched with the substrate and incubated. If they were mixed, then what was the reason for that? BBL and the overlying water column can have different biogeochemical properties. So, if they were mixed and incubated with $^{15}NH_4^+$, it cannot represent nitrification rates of BBL only and the aim of the study is to determine nitrification rate in benthic system not in water column. Please explain.

We agree with the reviewer that the method description in section 2.3.1 is misleading and must be clarified. No water samples were mixed, they were separately enriched with the tracer.
Changes line 173ff:
Water samples for incubations with $^{15}N\text{-}NH_4^+$ tracer (Damashek et al., 2016; Ward, 2005) were collected from the bottom water (water sampler/Niskin bottle) and from the sediment overlying core water and processed as described in detail by Bartl et al. (2018). Briefly, six polycarbonate bottles (sediment overlying water: 100mL bottle volume, VE II, 170mL bottle volume, VE I, ÖE I, II; Niskin bottle: 625 mL bottle volume, all field campaigns) were filled with water and sealed gas-tight.

6.1 O_2 content of BBL and water column is not mentioned. Was O_2 measured in sealed gas tight bags just prior to the experiment?

Oxygen concentrations are given in Table S1 and described in lines 241-242. Oxygen was not measured in sealed gas tight bags prior to the experiment. We did not use gas tight bags for incubations, but polycarbonate bottles. Please see answer to comment no. 4 regarding oxygen measurements.

6.2 Moreover, why were nitrification rates in the top oxic sediments not measured in both the estuaries and both seasons? The authors have emphasized the role of coupled nitrification-denitrification in these sediments. Then it makes sense to discuss benthic

nitrification here which can have much higher rates compared to BBL nitrification due to higher availability of NH4 + diffusing from deeper sediments and its oxidation in top layer.

Indeed, nitrification rates were not determined in the surface sediments which is unfortunate. However, the IPT gives denitrification rates based on nitrate from the sediment overlying BBL water and based on nitrate from nitrification in the sediments (see lines 226-228; 287-288; 425-431).
For your information, we estimated nitrification rates in the sediment from the Vistula estuary in spring, based on the sum of coupled nitrification-denitrification (Dn) and total nitrate fluxes out of the sediment (according to Bonaglia et al., 2014). Since no comparison with the Öre estuary was possible we decided to leave these data out. We explain below how our estimates of nitrification in sediments compare to other sites, where rates have been measured. However, we do not want to add the text to the manuscript because it is a bit beyond the scope of our study.
Nitrification rates in the sediment of the Vistula estuary were estimated from the sum of Dn (this study) and total nitrate fluxes from the sediment to the overlying BBL water (in situ incubations with chamber lander, Thoms et al., 2018). In the permeable sediment, mean Dn is 58 µmol m^{-2} d^{-1} and the mean nitrate flux is 507 µmol m^{-2} d^{-1} (n=3); resulting in a nitrification rate of 565 µmol m^{-2} d^{-1}. In the non-permeable sediment, mean Dn is 110 µmol m^{-2} d^{-1} and the nitrate flux is 140 µmol m^{-2} d^{-1} (n=1); resulting in a nitrification rate of 250 µmol m^{-2} d^{-1}. These estimates fall in line with other estimates from muddy sediments of the Baltic estuary Himmerfjärden (~389 µmol m^{-2} d^{-1}; Bonaglia et al., 2014). Compared to rate measurements, these nitrification rates in the permeable sediment are higher than wintertime nitrification rates in subtidal North Sea sediments (very fine sand: 198 µmol m^{-2} d^{-1}, fine sand: 216 µmol m^{-2} d^{-1}; Lohse et al., 1993), and higher than springtime nitrification rates in intertidal sands of the North Sea (342 µmol m^{-2} d^{-1}; Jensen et al., 1996). The estimated nitrification rate in the non-permeable sediment of the Vistula Estuary is similar to measured springtime nitrification rates in muddy sediments of the Baltic Gulf of Finland (286 µmol m^{-2} d^{-1}; Jäntii et al., 2011).
Comparing the estimates of nitrification rates in the sediment to areal nitrification rates of 131 µmol m^{-2} d^{-1} measured in the BBL of the Vistula estuary in spring (integrated over the vertical BBL extent of 3.2 m), nitrification rates in the sediment are 2 – 4 times higher than in the BBL, most likely due to the higher availability of NH_4^+ in the sediment (sediment at 1 cm/2 cm depth: 6.1/14.8 µmol L^{-1}; BBL: 0.6 µmol L^{-1}).

7. Section 2.3.2: The authors have not given a diagram for diffusive experimental set-up.

Diffusive core incubations are an established and widely used incubation method for cohesive sediments e.g. Jørgensen & Sørensen 1985, Nielsen 1992, Nielsen & Glud 1996, Sundbäck et al. 2006, Hietanen & Kuparinen 2008, Jäntti et al. 2011, Bonaglia et al. 2014, Bonaglia et al. 2017. To reduce the number of figures in this paper we decided to explain the diffusive design in the text (line 192-196 of the manuscript) and only show an illustration of the new advective incubation set-up, which has been designed for this study and needs detailed explanation. Nevertheless, if the reviewer feels that an illustration of the diffusive set-up is necessary, we will add one in the supplements.

8. Line 194-200: For ÖE I and ÖE II, 4 replicates were made for each concentration but 12 replicates were made for 120µM VE I and 3 replicates for VE II. Why? Moreover, for ÖE I, ÖE II and VE I, three concentrations i.e. 40, 80, 120 µM $^{15}NO_3^-$ were used but for VE II, four concentrations i.e. 30, 60, 90, 120 µM $^{15}NO_3^-$ were used. Again, for permeable sediments of VE, three concentration treatments were given with 5-7 replicates. Why were $^{15}NO3^-$ concentrations different and why were no. of concentration treatments different? What was the rationale behind such varied no. of treatments, replicates and concentrations between ÖE I, ÖE II, VE I and VE II? Why didn't the authors use same concentrations treatments and no. of replicates? For example, let's say, why couldn't they use 40, 80, 120µM $^{15}NO_3^-$ treatments for all types of sediments with 4 replicates?

The sampling campaigns in the Vistula and the Öre estuary have been carried out over a period of two years, during which improvements in the incubation design were undertaken, such as increasing the number of replicates (from three to four) in favor of a decrease in the number of used $^{15}N\text{-}NO_3^-$ concentrations (from four to three). These changes do not affect the resulting data: the concentration series is used to check whether the requirements of IPT (homogeneous distribution of the tracer and nitrate limitation of the sediment, Nielsen 1992) are fulfilled (plotting D15 against increasing $^{15}N\text{-}NO_3^-$ concentrations), as well as to check for a contribution of anammox to total N_2 production (plotting D14 against $^{15}N\text{-}NO_3^-$ concentrations, Risgaard-Peterson et al. 2003, see also response to comment no. 11). These tests are done by regression analysis. In order to ensure adequate number of replicates for the regression analysis we decided to use more replicates per concentration, with fewer concentrations, as samples were sometimes lost, unrepresentative or disturbed. In case of VE I (spring sampling), 12 replicates were run at the concentration 120 µM, to measure labelled N_2 production over time, which had earlier been shown to be insignificant due to seasonal limitation of denitrification activity in spring (explained in section 4.2.2); it was thus used as an internal test.

9. Line 204-205: Was the overlying water drawn only from the ports that were 5mm above oxic-anoxic interface or from all the ports lie above at 5mm resolution?

Water was only drawn from the one port that was located ~5mm above the approximated oxic-anoxic interface and recirculated during the incubation time.

10. Line 212-213: What are the sampling time points? Was O_2, NO_3^-, and NO_2^- measured in the overlying water at different time points?

Time points were start (0 h) and end (3-5 h) for N_2 and start (0 h) only for overlying water O_2 NO_3^- and NO_2^- concentrations. We incubated samples in a concentration series, not a time-series. All cores had a total incubation time of 3-5h (line 196, 211) without sampling in between (neither for labelled N_2 production, nor O_2, NO_3^- or NO_2^-).

11. Line 220-228: This paragraph needs to be rephrased. Risgaard-Petersen (2003) talks about the contribution of anammox to total N2 production from slurry incubation. But this study was based on intact core incubation. So first of all, please justify well that it can be

applied to this study, given that the availability of $^{14}NH_4^+$ can be less in case of intact core incubation compared to slurry incubation which can affect p14 and p15 values described by Risgaard-Petersen et al (2003).

According to Risgaard-Peterson et al. (2003), core-samples incubated in a concentration series can be used as an alternative to slurry incubations to indicate a contribution of anammox (page 72, first paragraph in Risgaard-Peterson et al., 2003). Following the method, a contribution of anammox to total N_2 production is indicated, when the production of ^{14}N-N_2 correlates positively with the concentrations of added $^{15}NO_3^-$ tracer. In such case, calculations have to be performed to distinguish N_2 production from anammox and denitrification. In this study, ^{14}N-N_2 never correlated with the added tracer concentrations, indicating no contribution of anammox to total N_2 production, which leaves denitrification as the sole N_2 production process.

11.1 Also I see that the first sentence of this paragraph i.e. from According to.....till...1992) is a word to word copy from a sentence from Helleman et al (2017). This is not acceptable. Please rephrase the sentence.

We rephrased the sentence so that it now reads:
A contribution of anammox to the measured N_2 production is indicated, when the production rate of ^{14}N-N_2 (D14, calculated with the IPT, Nielsen, 1992) correlates positively with the increasing ^{15}N-NO_3^- concentration in the incubation. In this case, calculation of N_2 production needs to distinguish between denitrification and anammox rates, following Risgaard-Petersen et al. (2003).

12. Line 230: Replace it with significance of difference or variability.

Changes from line 230:
Significance of differences between the factors 'site' (Öre estuary, Vistula estuary), 'season' (spring, summer) and 'sediment type' (permeable, non-permeable) was tested using…

13. Line 250 and Line 253: Both sentences contradict each other. Please rephrase the sentences. Sentence in line 250 means in both spring and summer POM in Öre River is dominated by terrestrial fraction but sentence in line 253 says in both spring and summer, POM is largely phytoplankton derived.

Changes from line 250 ff:
In the Öre River and river plume, POM contained a large share of terrestrial POM in both seasons, while the Vistula River and river plume were dominated by phytoplankton-derived POM (Table 2). The terrestrial origin of POM from the Öre River and river plume was reflected by the high C:N ratios and low δ^{13}C-POC values, neither of which occurred in the BBL of the Öre estuary or in the Vistula River and estuary (Table 2). In the estuarine water column (river and river plume excluded), the POM contained a large share of phytoplankton-derived POM in both estuaries and in both seasons (Table 2). This was further reflected in the high Chl.a

concentrations measured throughout the water column in spring and in the surface water in summer (Fig. 2, 3).

14. Line 273: It doesn't look so from the rates presented.

Also reviewer#1 commented on the missing results from the statistical analyses. We defined the significance level in section 2.4 (line 235) rather than adding it after every comparison/sentence in the results section, as we thought this would disturb the reading flow. However, we see that there is a need to add the statistical results in the text, which we will do in the revised manuscript. In this case (line 273) the significance level of the Kruskal-Wallis-Test is p=0.478 and clearly shows that there are no significant differences between nitrification rates. We added the Box-Whisker-plot here, to visualize this result (Figure R1).

Changes at line 273 ff:
Nitrification rates in the BBL did not significantly differ either between seasons or between estuaries (KW-Test, p=0.478; Table 4).

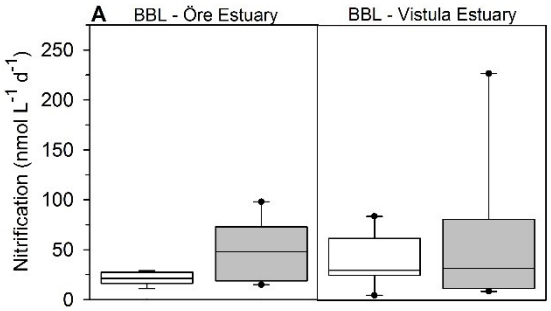


Figure R1: Nitrification rates in the BBL of the Öre and Vistula Estuary in spring (white) and summer (grey).

15. Line 273-276: These two sentences look contradictory. How can nitrification be positively correlated with POC if it shows negative trend with particulate C:N in case of Öre estuary? How can nitrification be positively correlated with PON, if it shows positive trend with particulate C:N?
16. Line 276-280: Same contradiction as in the case of nitrification. How can NH_4^+ assimilation be positively correlated with POC if it is negatively correlated with C:N? What is the logical explanation?

Comments 15 and 16 are answered together in the following:
In both estuaries, nitrification rates and ammonium assimilation rates are positively correlated with both PON and POC concentrations (see lines 274-275 and Figure 6 A, C). In the Öre estuary, nitrification rates show a negative trend with the C:N ratios (Kendall's $\tau = -0.52$, p=0.10, n=7; Figure 6 B) and ammonium assimilation rates show a significant negative correlation with the C:N ratio (Kendall's $\tau = -0.71$, p=0.02, n=7, Figure 6 D). A positive correlation between a rate and the POC or PON concentration does not necessarily imply that there should also be a positive correlation with the ratio of POC:PON. In the case of the Öre estuary, the C:N ratio is negatively correlated to PON and POC concentrations (Kendall's $\tau = -0.62$, p=0.05, n=7; Figure R3). Consequently, nitrification and ammonium assimilation rates seem to be influenced by a

combination of the concentration and the ratio of POC and PON. Interestingly, lowest C:N ratios were found at greatest depth (Kendall's $\tau = -0.81$, $p=0.01$, $n=7$), which indicates accumulation of phytoplankton-derived POM in the deeper parts of the Öre estuary.

Unfortunately, there was a mistake in the plotted C:N ratios of the Vistula estuary in panels B and D of Figure 6. We apologize for this and added the corrected figure below (Figure R2). We would not interpret the relationship of nitrification or ammonium assimilation rates with the C:N ratio in the Vistula estuary as positive trend, which is underlined by the lacking correlations (nitrification vs C:N: Kendall's $\tau = -0.04$, $p=0.93$, $n=7$; ammonium assimilation vs C:N: Kendall's $\tau = -0.03$, $p=0.94$, $n=9$).

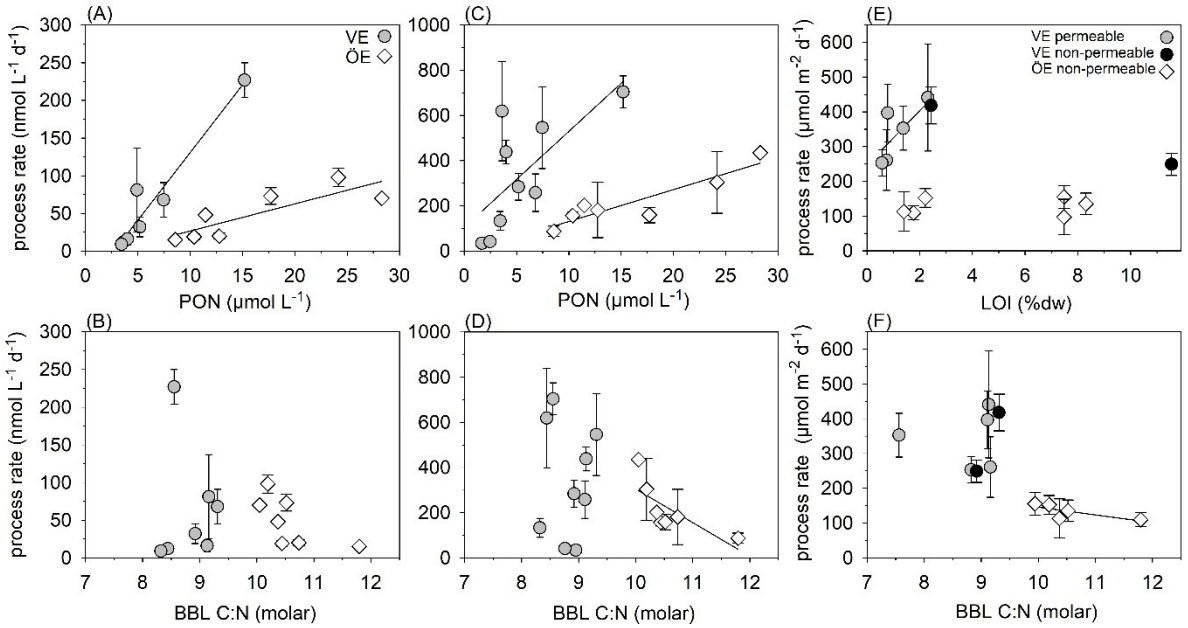


Figure R2: Correlations of nitrification rates in the BBL with PON concentration (A) and particulate C:N ratio (B); ammonium assimilation rates in the BBL with PON concentration (C) and particulate C:N ratio (D); and coupled nitrification-denitrification rates in the sediment with LOI (E) and particulate C:N ratio (F). Solid lines represent significant correlations. Please note the different scaling of C:N ratios compared to figure 6 in the manuscript.

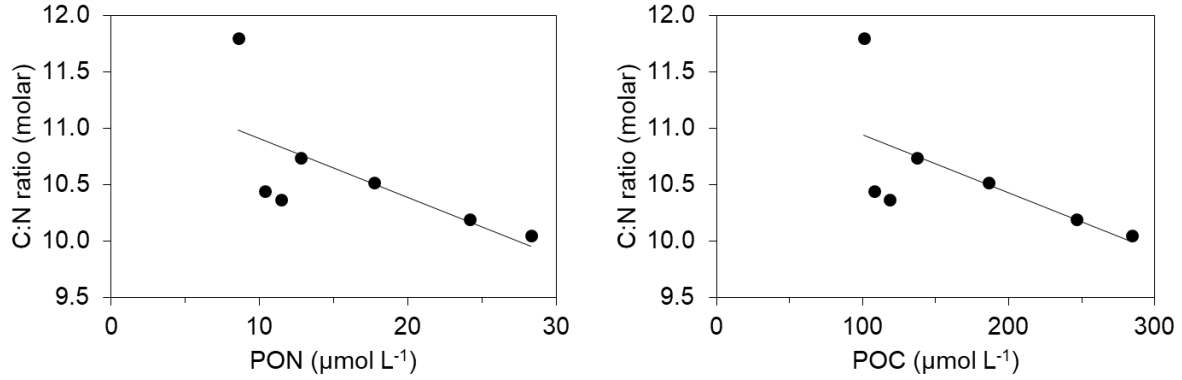


Figure R3: Correlations of PON (left panel) and POC (right panel) concentration with the particulate C:N ratio.

17. Section 3.2.2: The authors clearly concluded that there was no anammox and denitrification was the sole N loss process. What about DNRA? The authors didn't mention anything about it although it is only an N transformation process. I think the authors are coming to conclusion here rather abruptly without considering findings of Jensen et al. (2011) in the Arabian Sea. Coupling of DNRA-Anammox can happen which can create an impression of denitrification signal and hence the conclusion can be misleading. Thus, the authors should relook at their incubation data and reinterpret if necessary.

We did not measure DNRA rates in this study and no significant anammox rates were found, as analyzed with a concentration series following Risgaard-Petersen et al (2003; see answer to comment no. 11, manuscript line 220-226). In case DNRA was active in the estuarine sediments, it could have produced $^{15}N\text{-}NH_4^+$ based on transformation of $^{15}N\text{-}NO_2^-$ originating from the reduction of the added $^{15}N\text{-}NO_3^-$ tracer (during IPT incubation). However, only a further combination of this $^{15}N\text{-}NH_4^+$ with $^{14}N\text{-}NO_2^-$ or $^{15}N\text{-}NO_2^-$, such as described by Jensen et al. (2011), could result in additional single ($^{29}N_2$) or double labeled N_2 ($^{30}N_2$) that would not have originated from denitrification and thus would violate the binomial distribution required for denitrification calculations based on IPT (Nielsen 1992). Without anammox, the $^{15}N\text{-}NH_4^+$ produced by DNRA would simply stay within the sediment or be nitrified again to $^{15}N\text{-}NO_3^-$, but not interfere with the production of N_2 via denitrification. Consequently, as we did not find any sign of anammox, we are certain that the measured single and double labeled N_2 production came from denitrification only.

17.1 Again, the authors have not given any figure on ^{15}N-labelled intact core incubation which is very important.

Please see response to comment no. 7 regarding the graphic display of core-incubations.

17.2 Please present few figures depicting increase in $^{15}N\text{-}N_2O$ and $^{15}N\text{-}N_2$ with time to support your conclusion on denitrification being a major N loss pathway. Similarly, if you find anammox and DNRA upon re-analysis of the incubation data, then please show the proof in terms of additional figures.

The presense / absence of anammox, thus its significant /non-significant contribution to total N_2 production and the consequential role of denitrification in N_2 production were investigated by concentration series (Risgaard-Petersen et al. 2003), not in time-series.
In the concentration series, D15 (= the denitrification of $^{15}N\text{-}NO_3^-$) has to correlate with increasing tracer concentration to fulfill basic requirements of IPT (homogeneous distribution of the tracer and nitrate limitation of the sediment, i.e. basically homogeneous uptake of the tracer, Nielsen 1992), whereas D14 (= the true denitrification) should be independent of tracer concentration, if no anammox occurs. In contrast, a significant increase of D14 with increasing tracer concentration would indicate anammox, for which then separate calculations need to be applied, following Risgaard-Petersen et al. (2003). These relations were tested with regression analyses (significance level $p < 0.05$).
Below an example plot of N_2 data without contribution of anammox (i.e. D14 not dependent on increasing tracer concentration: A= Öre Estuary, station N34, summer; B= Vistula Estuary, station VE05, summer), as was the case in all incubations.

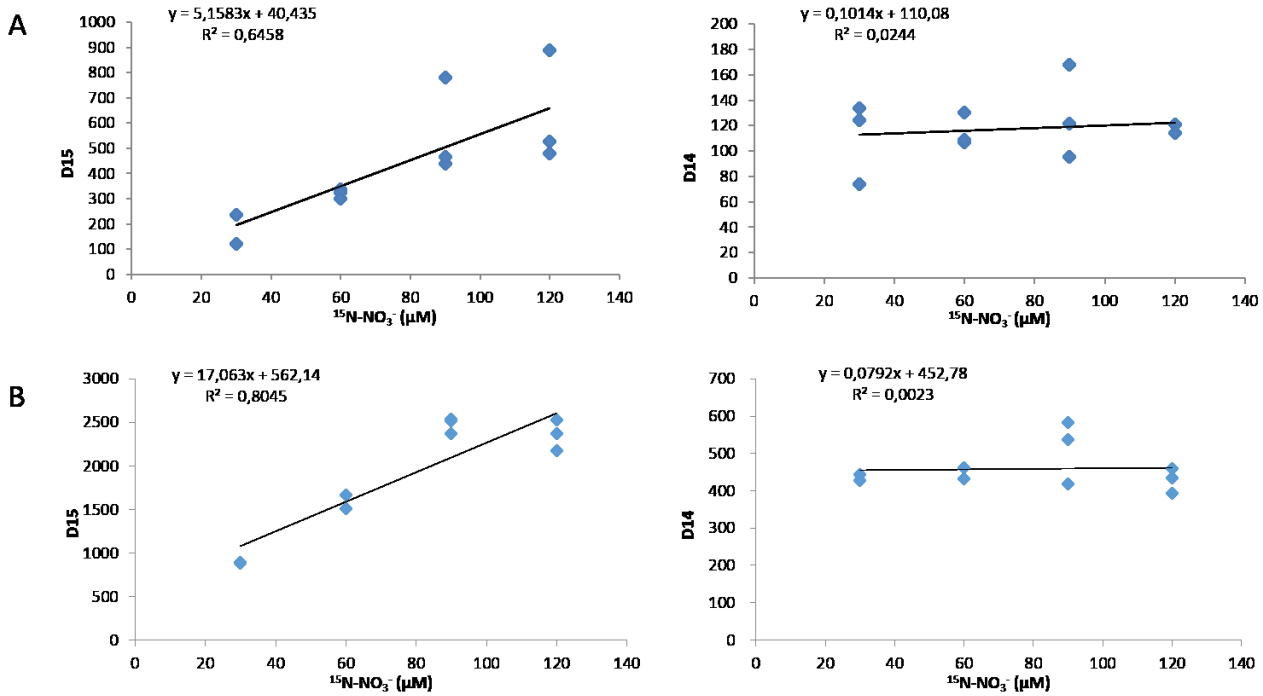


Figure R4: Denitrification of labeled $^{15}N\text{-}NO_3^-$ (D15) and unlabeled $_{14}N\text{-}NO_3^-$ (D14) at increasing $^{15}N\text{-}NO_3^-$ tracer concentration for the Öre estuary (a) and the Vistula estuary (b).

17.3 Why coupled nitrification-denitrification was not correlated negatively particulate C:N in case of Vistula estuary?

A low C:N would indicate a high amount of N in the organic matter, which would favor nitrification via NH_4^+ from PON degradation. This in turn enhances denitrification coupled to nitrification in the sediment. A negative correlation of Dn with particulate C:N would thus be expected, as mentioned by the reviewer and as also found in the Öre estuary in summer. Yet, we did not find this correlation in the Vistula estuary (see Figure 6), likely due to the overall lower C:N ratio (see lines 258-260) indicating a higher availability of N compared to the Öre estuary. In addition, the lower POC:Chl.a ratios in the BBL of the Vistula estuary (see Figure R7) suggest a high share of phytoplankton-derived POM which results in a high availability of labile organic carbon as well, the second substrate for denitrification (see line 419).

18. Line 302-303: It doesn't look so. I don't see NO_3^-+NO_2^- in BBL of estuaries differing significantly if we strictly consider standard deviation (SD) given in Table S1. On the contrary, POC and PON in BBL of Vistula estuary are much higher than that in BBL of Öre estuary (Table 2). Please rephrase these sentences.

We performed a Mann-Whitney-U-Test for significant differences between NO_3^-+NO_2^- concentrations. In summer, BBL NO_3^-+NO_2^- concentrations are significantly higher in the Öre estuary than in the Vistula estuary (U-Test, $p<0.001$; Figure R5 A). Also PON (U-Test, $p=0.048$; Figure R5 B) and POC (U-Test, $p=0.04$; Figure R5 C) differed significantly, although ranges are overlapping. Results of the U-Tests will be added in lines 302-303.
In Table 2, there was a copy&paste mistake in the row of BBL POC and PON concentrations. We apologize for this mistake and corrected the values (see Table R2).

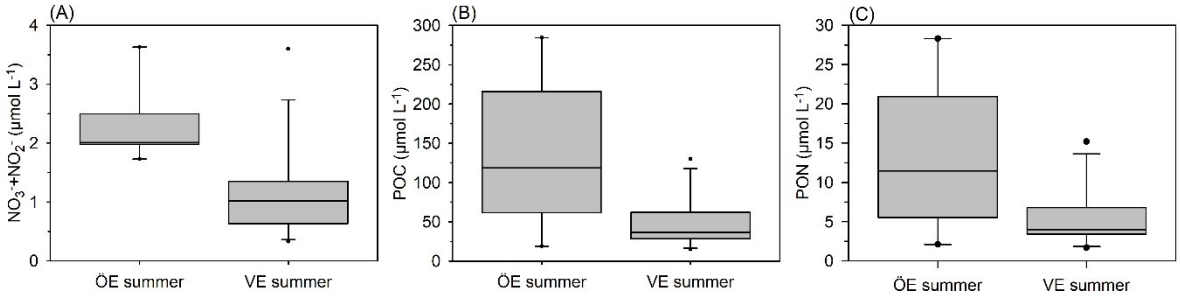


Figure R5: Comparison of $NO_3^-+NO_2^-$ (A), POC (B), and PON (C) concentrations in the BBL of Öre and Vistula estuary in summer.

Table R2: Table 2 from the manuscript corrected (corrected values highlighted in yellow). Concentration of particulate organic carbon (POC) and nitrogen (PON); natural isotopic composition of POC (δ^{13}C-POC); the contribution of terrestrial and phytoplankton-derived particulate organic matter (POM) to the total POM pool measured in the river and river plume water as well as at the surface and in the bottom boundary layer (BBL) of the Öre and Vistula estuaries in spring and summer. The contribution of POM sources was estimated based on a two-component mixing model following Jilbert et al. (2017), using end members from Goñi et al. (2003). Values are average and standard deviation of each water layer. The sample size is shown in parentheses.

Site	Season	Water source	POC (µmol L^{-1})	PON (µmol L^{-1})	δ^{13}C-POC (‰)	Contribution terrestrial POM (%)	Contribution phytoplankton POM (%)
Öre estuary[a]	Spring	River	153.6	11.2	-29.1	71	29 (1)
		River plume	53.7	5.1	-29.5	44	55 (1)
		Surface	40.2 ± 13.5 (8)	4.3 ± 1.4 (8)	-25.7 ± 1.0 (8)	19 ± 16	83 ± 16 (8)
		BBL	36.8 ± 14.1 (10)	4.2 ± 1.5 (10)	-25.0 ± 1.0 (10)	19 ± 16	81 ± 16 (10)
	Summer	River	67.2	5.7	-30.2	56	44 (1)
		River plume	46.9 ± 0.7 (3)	4.1 ± 0.7 (3)	-28.7 ± 0.2 (3)	55 ± 16	45 ± 16 (3)
		Surface	34.1 ± 7.9 (13)	4.0 ± 0.8 (13)	-26.5 ± 0.6 (13)	15 ± 11	85 ± 11 (13)
		BBL	135.9 ± 85.5 (9)	13.1 ± 8.4 (9)	-26.1 ± 0.3 (9)	38 ± 11	62 ± 11 (9)
Vistula estuary[b]	Spring	River	164.2	16.5	-25.7	37	63 (1)
		River plume	61.1 ± 25.9 (8)	6.9 ± 2.5 (8)	-26.5 ± 1.4 (8)	25 ± 14	75 ± 14 (8)
		Surface	45.6 ± 15.8 (6)	5.8 ± 2.4 (6)	-24.8 ± 0.7 (6)	10 ± 16	90 ± 16 (6)
		BBL	25.4 ± 13.6 (18)	2.6 ± 1.3 (18)	-25.6 ± 0.8 (18)	31 ± 24	69 ± 24 (18)
	Summer	River	-	-	-	-	-
		River plume	103	10.2	-25.8	33	67 (1)
		Surface	73.6 ± 34.6 (7)	8.3 ± 3.7 (7)	-25.7 ± 0.6 (7)	20 ± 10	80 ± 10 (7)
		BBL	46.9 ± 30.7 (11)	5.3 ± 5.5 (11)	-25.4 ± 0.8 (10)	15 ± 10	85 ± 10 (9)

[a] Including data from Hellemann et al. (2017)
[b] Including POC and PON concentrations from Bartl et al. (2018)

19. Line 306-309: This is true only for spring where we see high POC and PON in BBL of Öre compared to Vistula. But again on closer look, if we take SD and no. of replicates into account, POC in BBL of Öre is similar to that in Vistula and interestingly PON in BBL of Öre is higher than that in Vistula. This claim is anyway not true for summer.

Please see answer to comment no. 18.
PON and POC concentrations are higher in summer than in spring in both estuaries, and higher in the Öre estuary than in the Vistula estuary in summer (Table R2).

20. Line 328: Delete Fig.4 from the sentence as it does not show C:N.

Figure 4 shows particular C:N ratios on the y-axis of the graphs.

21. Line 330-332: Not a satisfactory explanation. Öre estuary has a sill and thus restricted exchange of estuarine water with seawater can likely cause more sedimentation within the estuary.

The reviewer is correct that there is likely more sedimentation of particulate matter within the Öre estuary. We were not aiming to contradict to this with our explanation in lines 330-332. We wanted to highlight the finding of Forsgren and Jansson (1992), that a large part of the terrestrial POM from Öre River directly sediments at the river mouth not reaching the estuary at all.
Changes from line 330 ff:
This was likely due to the abundant, widely dispersed estuarine phytoplankton (Fig. 2). Furthermore, Forsgren and Jansson (1992) showed that the terrestrial POM from the Öre River immediately sediments right at the river mouth and is not transported far into the Öre estuary, which may explain the small terrestrial signal in the POM from the estuarine BBL.

22. Line 333-334: C:N in Öre is higher than that in Vistula but POC:Chla in Vistula vary from 5.4 to 33.2 which is <<200. How can it indicate degraded POM? Only because of C:N<12?

The reviewer may have made an error in her/his calculation. The POC:Chl.a ratios are calculated as mass ratio, i.e. µg POC L^{-1} (not µmol) divided by µg Chl.a L^{-1} (following the approach of Cifuentes et al., 1988; Savoye et al., 2003). We converted the molar concentrations of POC (µmol L^{-1}) to mass (µg L^{-1}) by multiplying with the molar mass of C (12.011 g mol^{-1}). The POC:Chl.a ratios in summer are >200 in both, Vistula and Öre estuary (Table R3), indicating a low percentage of fresh chlorophyll containing biomass.
For clarification, we will add this information (underlined) in section 2.2.1:
lines 146-147: *Particulate organic nitrogen and carbon (PON, POC) concentrations (µmol L^{-1}) and…*
lines 145-146: *The degradation state of POM was evaluated by determining the mass ratio of POC:Chl.a (µg $µg^{-1}$) and molar C:N (µmol $µmol^{-1}$) ratios, which increase simultaneously during degradation (Savoye et al., 2003).*

Table R3: POC concentrations in µmol L^{-1} and µg L^{-1}, Chl.a concentration in µg L^{-1}, and POC:Chl.a ratios calculated from mass concentrations. Data from the BBL of Öre and Vistula estuary in spring and summer.

Site	Season	Station	POC (µmol L^{-1})	POC (µg L^{-1})	Chl.a (µg L^{-1})	POC:Chl.a (µg $µg^{-1}$)
Öre estuary	Spring	N6	37.0	444.8	7.5	**60**
		N11	34.8	417.7	6.4	**66**
		N11	28.1	337.0	6.4	**53**
		NB8	33.3	399.7	3.1	**131**
		NB8	21.6	259.2	3.1	**85**
Vistula estuary	Summer	N6	137.1	1646.6	0.61	**2684**
		N11	246.2	2957.5	0.60	**4919**
		NB8	284.5	3417.1	0.55	**6184**

	VE07	14.9	178.5	1.0	**174**
	VE07	43.7	524.5	5.7	**93**
	VE04	38.9	467.1	2.8	**164**
	VE06	11.2	134.8	1.0	**132**
	VE06	28.2	338.2	1.4	**247**
	VE18	21.7	261.1	1.9	**135**
	VE13	11.9	143.2	1.1	**131**
	VE13	22.9	274.8	1.5	**181**
Spring	VE09	12.3	147.1	1.4	**108**
	VE09	38.8	466.3	1.3	**356**
	VE10	10.6	127.4	1.3	**96**
	VE10	41.3	495.7	1.7	**292**
	VE05	11.3	135.7	1.3	**104**
	VE05	25.6	307.2	1.3	**240**
	VE02	56.4	677.9	2.9	**232**
	VE49a	21.9	263.1	3.4	**78**
	VE49a	34.7	416.4	4.9	**86**
	VE15	15.0	180.6	0.3	**661**
	VE02	30.5	365.9	1.4	**256**
Summer	VE13	28.6	343.0	0.8	**460**
	VE23	21.6	259.2	0.2	**1178**
	VE49a	33.3	399.8	0.7	**597**

23. Line 336-338: Summertime POC:Chla in Ore varies from 12.6 to 140 that is <200. How can the POM be in degraded state?

Please see response to comment no. 22.

24. Line 401 and Line 397: Please show the r and p values of the correlation.

The correlation coefficient and the p value are given in line 274 and line 280.

25. Line 398: because the less-degraded POM in.....This is questionable as POC:Chla is not above 200 rather <<200.

Please see response to comment no. 22.

26. Line 399: By contrast, the more degraded POM in.....First of all, POC:Chla in both Öre and Vistula estuary are much lower than 200. So can we call it degraded POM? Even though we assume higher POC:Chla (>200) as indicator for highly degraded POM, POM in Vistula estuary looks more degraded compared to that in Öre estuary. Not the other way.

Please see response to comment no. 22, lines 258-260 and section 4.1.2.

27. Line 401: How significant is this correlation? What are the r and p values?

Please see line 280 and response to comment no. 15 and 16.

28. Line 402-404: These two sentences contradict each other. First sentence is questionable. I see a significant seasonal difference in the rates in both the estuaries. Please clarify the role of trophic state on these two processes.

There is no significant difference between the nitrification rates of the two estuaries (see response to comment no. 14).
We agree that this short explanation is confusing and decided to remove this statement (line 401-404) from the manuscript text, as a more precise and understandable version of this is also given in the conclusions (section 5).

29. Line 407-409: Difference in denitrification.....How do you know that? Where is sedimentary Corg data and δ13C-Corg for both the estuaries?

As described in the results, the Vistula Estuary had more labile organic matter than the Öre Estuary based on the C:N and POC:Chl.a ratios in the BBL (see Table 2 and section 4.1.2). It is very likely that the more labile organic matter in the Vistula estuary originates from high riverine N-loads and the resulting high primary production rates. Heterotrophic denitrification uses labile organic carbon as electron donor to reduce NO_3^-, an increase in labile organic matter can thus increase denitrification rates (Seitzinger & Nixon 1985). Our correlation result of Dn with the LOI of the surface sediment provides evidence for this effect (see Figure 6). The organic matter content (LOI) strongly correlates with the Corg in the sediment (Figure R6). So, although we do not have sediment Corg data from the summer sampling in the Vistula and Öre estuary, we can confidently use LOI as measure of organic matter/carbon content. The δ^{13}C-Corg data give information about the contribution of terrestrial material to the total POC. δ^{13}C-Corg (-28.5 – -25 ‰) of the surface sediment in the Vistula estuary (Figure 2 in Thoms et al. 2018) are similar to δ^{13}C-POC values in the BBL (Table R2) and indicate some terrestrial contribution in the benthic POM-mixture of the Vistula estuary (see lines 326-328).

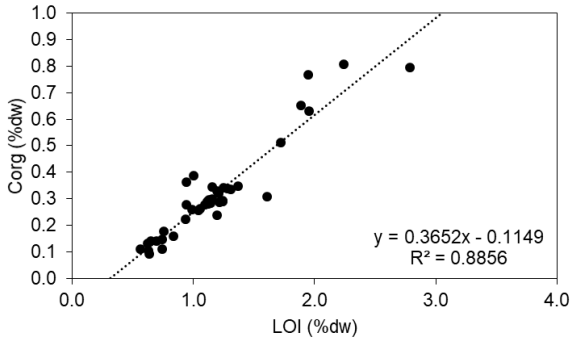


Figure R6: Correlation between LOI and Corg in the surface sediment of the Vistula estuary in spring 2016 (data kindly provided by Franziska Thoms, IOW)

30. Line 415-419: While newly produced.....If higher POM availability increased denitrification rates in sediments, then why not in water? Especially in BBL?

All bottom waters were oxic (Supplement Table S1), thus the oxic-anoxic interface, where denitrification takes place, was located within the sediment. Thus denitrification in both estuaries only happened in the sediment, not in the BBL.

31. Line 421-423: what are the r and p values of the correlation?

Please see line 291.

32. Line: 425-427: Not written properly. OPD itself can get NO_3^- from BBL. Nitrification is significant in BBL and NO_3^- is not that low.

The IPT calculation (Nielsen 1992) can clearly distinguish between the NO_3^- source used in denitrification: the NO_3^- from the bottom water or the NO_3^- from nitrification within the oxic surface sediment (see line 225-228). The result of this calculation was that in both estuaries denitrification mainly used NO_3^- from nitrification within the sediment (≥ 93%, line 288). In lines 425-427 we refer to the calculation result and discuss its reason.

33. Line 427: Hence, only a small.....How did the authors calculate that? Is there any nitrification rate measurement in the top oxic layer of the estuarine sediments?

Please see answer to comment no. 32.

34. Line 429: This sentence contradicts the previous sentence. If the dominant NO_3^- source is controversial, then how can you say that <10% of NO_3^- from BBL was removed by denitrification in permeable sediments of Vistula estuary?

The reviewer is right, in that "controversial" was not the right word used here. We adjusted the section accordingly at line 429:
In permeable sediments, the dominance of the NO_3^- source is highly variable due to the complexity of pore-water flow (Kessler et al., 2013; Gihring et al., 2010; Marchant et al., 2016; Rao et al., 2007). On the one hand, pore-water flow was shown to stimulate nitrification by increasing the oxic sediment volume (Huettel et al. 1998, Giehring et al. 2010, Marchant et al. 2016), and to increase the areal oxic-anoxic interface across which NO_3^- and NH_4^+ can be exchanged (Precht et al. 2004, Cook et al. 2006), thus favoring denitrification coupled to NO_3^- produced in the sediment (Dn; Rao et al. 2008, Marchant et al. 2016). On the other hand, pore-water flow was also shown to separate the oxic inflow from the anoxic outflow zone, limiting the exchange of NO_3^- and NH_4^+ within the sediment (Huettel et al. 1998, Cook et al. 2006, Kessler et al. 2012, 2013) and thus favoring denitrification of NO_3^- from the near-bottom water (Dw; Cook et al. 2006, Kessler et al. 2012, 2013, Marchant et al. 2014).

The 10% of NO_3^- removed from the BBL by denitrification is the result of the IPT calculation, that ~10% of denitrification was fed by NO_3^- from the BBL water (Dw). Please see also answer to comment no. 32.

35. Line 449: Please write it as During summer.....

Changes at line 449:
During the summer cruise, permeable sediments in the Vistula estuary were not subjected to significant advective pore water flow, thus allowing the use of a diffusive incubation design.

36. Line 450-452: How is that possible? What about denitrification rate during spring?

We discuss in 4.1.3 in detail the observation, that the permeable sands of the Vistula Estuary were temporary lacking advective pore-water flow during our sampling campaign in summer 2014 (presumably due to low near bottom current velocities in summer). In the absence of advective pore-water flow, mass transport of permeable sediments is governed solely by diffusive and faunal induced fluxes, similar to cohesive sediments. We believe that the same mass transport led to the same denitrification rates, very likely due to the resulting similar transport velocity of substrates to the denitrification layer which was situated at a similar sediment depth.
In spring, the permeable sands of the Vistula estuary experienced advective pore-water flow, as expected for this sediment type. Denitrification rates were however low, likely due to low availability of labile organic carbon, as well as due to problems in the incubation set-up. This issue we discuss in detail in section 4.2.3.

37. Line 457-458: Despite their.....What about spring?

For the spring season, we are only able to calculate the N removal efficiency for the Vistula estuary, since no denitrification rates were detectable in the Öre estuary. The Vistula estuary removed 0.2 % of the riverine TN load via denitrification in spring (March 2016).

38. Line 461-463: These two statements are contradictory. How would the authors reconcile these statements vis-a-vis their observation?

The reviewer is right, that the statements of the cited studies are contradictory.
Changes at line 461 ff:
Asmala et al. (2017) estimated from a compilation of coastal denitrification rates that ~16% of the riverine TN load entering the Baltic Sea is removed by coastal denitrification, and concluded that the Baltic Sea coastal zone is an inefficient N filter compared to the open Baltic Sea. In contrast, based on isotopic data and long-term nutrient concentrations, Voss et al. (2005a, 2011) suggested that most of the riverine N is sequestered and removed within the Baltic coastal zones. The anticlockwise circulation pattern in the Baltic Sea, resulting in alongshore coastal jets and restricted cross-shore mixing (Radtke et al., 2012), may support coastal N retention. In this case, the coastal N filter efficiency would depend on the transport and residence time of riverine N within the Baltic coastal zone, providing time for N retention processes to recycle N until its eventual permanent removal. Accordingly, N removal efficiency alone, e.g., via denitrification rates, relative to riverine TN loads, as estimated by Asmala et al. (2017), may not be a sufficient indicator of the N filter efficiency in river dominated coastal zones.

We aimed to reconcile these statements with our observations in section 4.3. However, we see that our formulations may have not been clear enough. We will overwork section 4.3 for the revised manuscript.

39. Line 465-468: What about DNRA? That would show that how much riverine N is preserved in estuarine sediments through DNRA. It is necessary to discuss that here.

The reviewer is correct that the role of other N-transformation processes that retain N in the estuary, like DNRA, should be addressed here.
Changes at line 468 ff:
Accordingly, N removal efficiency alone, e.g., via denitrification rates, relative to riverine TN loads, as estimated by Asmala et al. (2017), may not be a sufficient indicator of the N filter efficiency in river dominated coastal zones. Instead, holistic approaches are needed, which also address the role of N retention processes such as nitrification or N uptake in the water column, and nitrification or DNRA in the sediment as they facilitate potential preservation of N in the coastal system.

40. 471-473: Through close.....It is not necessary that only POM controlled benthic nitrification. What about benthic NH_4^+ efflux?

This is correct. However, we could check this relationship for a few stations where Thoms et al. (2018) measured NH_4^+ efflux from the sediment. These fluxes do not correlate with nitrification rates in the BBL. However, since the number of replicates is very low, we did not want to include the result in our manuscript.
Nevertheless, NH_4^+ effluxes do supply the BBL with this nutrient which may act as important substrate source for nitrification, especially under conditions of reduced NH_4^+ production from POM degradation, e.g. in winter/early spring.

41. Line 482: What are the DNRA rates?

We did not measure DNRA rates in this study, please see response to comment no. 17 and response to the overall comments.

42. Line 490-492: We thus hypothesize..... How do the authors say it is a coast parallel transport? Is there any reference? The riverine flow may be perpendicular to the coast into the Baltic.

The reference is Voss et al., 2005b.
Changes (underlined) from line 488-492:
Furthermore, the open shape of the estuary and its unrestricted bottom topography may well enable the transport of riverine DIN and suspended estuarine POM out of the estuary and parallel to the coastal zone throughout the year (Voss et al., 2005b). We thus hypothesize that the coast-parallel transport of nutrients and estuarine POM extends the estuarine filter of the Vistula estuary to the adjacent coastal zones (Fig. 7), where microbial N retention and N removal could take place over a larger area and a longer time scale.

43. References: Holtermann et al. (2014), Risgaard-Petersen et al. (2004) and Schultz (2000) are not cited in the text. Schultz (2005) is missing in the reference list.

We thank the reviewer for checking the reference list and will correct it accordingly in the revised manuscript.

44. Table 2: The authors need to show C:N in a column here.

C:N ratios are given in section 3.1.1, lines 258-260, and in Figure 4. We think it would be too much repetition to add them in Table 2 as well.

45. Table 3: How have NH_4^+ surface pool and NH_4^+ deep pool been defined? Up to what depth you consider it as surface pool? Please mention clearly in the table caption.

The sediment NH_4^+ pools are defined in lines 167-169. We will add this information in the caption of Table 3.

46. Table 4: I don't see any denitrification rate in permeable sediments of Öre estuary. Was it not measured or it is not detectable? "–" symbol doesn't mean anything. Please clarify.

All sampled sandy sediments in Öre estuary were non-permeable (permeability = 0.1-0.2 × 10^{-12} m^2, Table 3), which is discussed in detail in Hellemann et al. (2017) and mentioned in the current study in line 102, and 262-263. But, we see that in this respect, Table 4 is misleading. We will replace "-" by an appropriate abbreviation and explain it in the caption of Table 4.

47. Figure 2 & 3: The PON plots for Öre estuary are reproduced from Hellemann et al (2017). So please mention the reference clearly in the figure captions.

We thank the reviewer for pointing this out and we will add the reference to the figure caption.

48. Figure 4: This figure contradicts the data in Table 2 and Table S1. If we calculate POC:Chla from Table 2 and S1, they range from 5.4 to 140. How come Fig.4 shows such higher POC:Chla values then?

Please see the answer to comment no. 22.
Although we calculated the POC:Chl.a correctly (Table R3), we found a copy&paste error in figure 4C. We apologize for this and added the corrected figure below (Figure R7). The corrected values do not change the results and discussion in the manuscript.

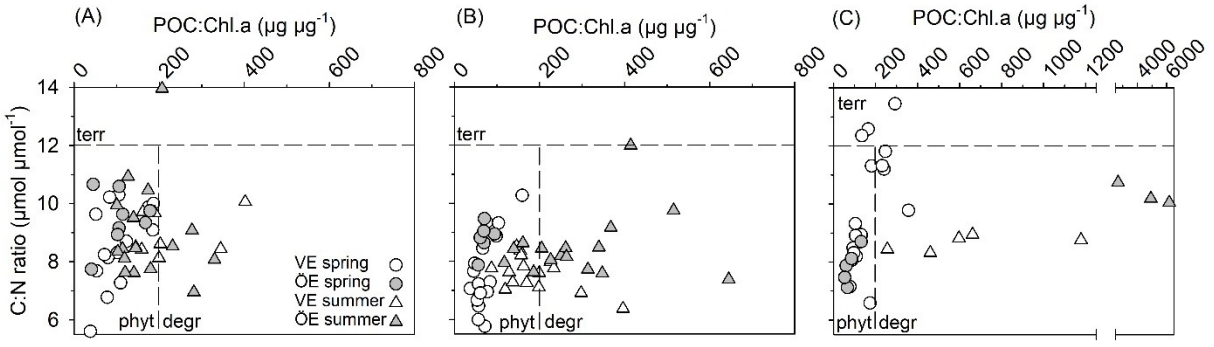


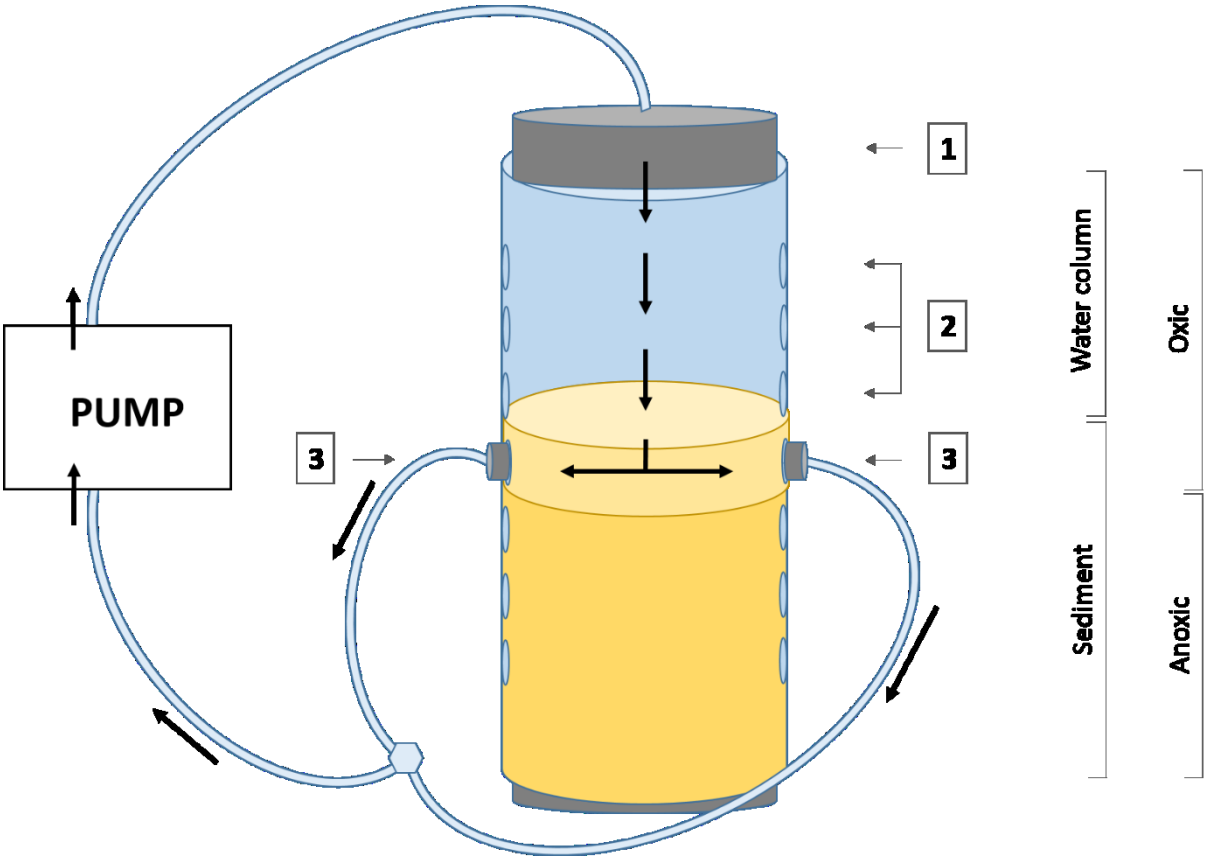
Figure R7: Corrected Figure 4: Particulate C:N ratios plotted against POC:Chl.a ratios from the surface water (A), intermediate water depths (B) and bottom boundary layer (BBL, C) of the Vistula and Öre estuaries in spring and summer. Data at intermediate water depths are water depths of 10 m and 20 m in the Vistula estuary, and 5 m and 10 m in the Öre estuary. C:N ratios: terrestrial POM (terr) > 12 according to Savoye et al. (2003); POC:Chl.a ratios: newly produced phytoplankton POM (phyt) < 200 < degraded 5 phytoplankton POM (degr) according to Cifuentes et al. (1988). Note the different scale of the POC:Chl.a ratios in panel C.

49. Figure 5: Shows vertical O2 profile of Vistula estuary sediments. But what about that of Öre estuary sediments? The authors should show that also.

The example profiles of the permeable Vistula Estuary are displayed, because they show a striking difference in O_2 profile curve between spring (sigmoidal curve) and summer (parabolic curve), which we explain with presence and absence of advective pore-water flow (4.1.3). Example O_2 profiles in sediments of the Öre Estuary are given in Hellemann et al. (2017) and are thus not repeated here, as the focus of Figure 5 is the presence/absence of advective pore-water flow. Nevertheless, if the reviewer feels that the manuscript benefits from showing the O_2 profiles from the Öre estuary, we are will add them. Alternatively, we could add the reference for pore-water oxygen profiles of the Öre estuary in the caption of Figure 5.

50. Figure S1: The authors should point out the ports through which water sample was collected. Please point out the water above the sediments.

We updated the graphic S1. However, no water samples collected during incubation. Water circulation through the upper sediment layer was applied to mimic advective pore-water flow, and samples for N_2 isotope analysis were taken at the end of incubation, after sediment and water was carefully mixed into a slurry.



1 = inflow port, 2 = potential outflow ports, 3 = actual outflow port ~5 mm above the oxic-anoxic interface

Figure R8: Updated Figure S1: A schematic of the incubation design used to measure sediment denitrification with advective pore-water flow. Site-water spiked with 15N-NO3- tracer is pumped into the core from the top and drawn out from two sides of the oxic sediment layer (light yellow), as an approximation of the layer affected by advective flow. The outflow ports have a resolution of 5 mm, chosen according to the previously determined oxygen penetration depth.

51. Table S1: Looks a bit confusing and unexplained. River plume very much prevails within these two estuaries and occupies a depth range of up to 3m in case of Öre estuary and up to 12m in case of Vistula estuary. So when we say river plume here that actually means surface water of estuary. So, why can't the authors consider the depth from the river plume till bottom? If they do so, then I believe the so-called surface here would actually be a depth of 3m in case of Öre and 12m in case of Vistula. The authors should clear the confusion and mention terms in a logically correct way. Additionally, I believe a column for POC:Chla is necessary in this table.

We agree with the reviewer, that the given depth ranges cause confusion. The depth range of the river plumes, Öre River 3m and Vistula River 12m, which are given in section 2.1, are ranges found by previous studies (Cyberska and Krzyminski, 1988; Forsgren and Jansson, 1992). During our field campaigns, the depth range of the river plumes was ≤ 5m in both estuaries (see section 3.1.1, line 240). Within this depth range we took samples at 0m (bucket) and from the surface water with the CTD-water samplers (sampling depths: 1m-2.5m). The water samples from the remaining coastal surface (not river plume) were taken in the same depth range. Hence, water

from below 5 m, belong to the mid water column. We will clarify depth ranges given in section 2.1 and in Table S1 in the revised manuscript.
POC:Chl.a ratios are given in lines 255-257 and in Figure 4. We think that adding the values in Table S1 would be too repetitive. However, if the reviewer still recommends to add them, we are happy to do so.

Overall comments & suggestions:
I suggest the authors to be careful about not repeating the description of sampling methods, analysis/experiment methods and results which are already reported by Helleman et al. (2017) and Bartl et al. (2018) for these two estuaries. For example: Do not describe the water column sampling methods, sediment sampling methods, analysis methodology, denitrification experiment method and their results in details for Öre estuary because these are already published by Helleman et al. (2017). But you can retain everything about NH4+ assimilation and nitrification in Öre estuary. Similarly, for Vistula estuary, avoid detailed description of column sampling methods, analysis methods and ammonium assimilation and nitrification experiment methods in BBL and their results because these are already published by Bartl et al. (2018). But you can retain everything about sediment sampling and analysis methods, denitrification experiment methods and their results. However, the authors can use the published data and their own generated data for the discussion since it's a comparative account study.

We thought a lot about how to structure the section 'Materials and Methods' in this manuscript, because, as the reviewer points out, it is partly repetitive to Hellemann et al. (2017) and Bartl et al. (2018). However, we came to the conclusion that it is necessary to repeat the methods shortly to ensure a comprehensive section. We do not want the reader to look up the other two publications to understand the methods we used. Instead we would like the manuscript to stand for itself. We will go through the section 2 ('Materials and Methods') again to shorten it where possible.
The same holds for the presentation of the results. Some of the results reported by Hellemann et al. (2017) and Bartl et al. (2018) are given here again (always with reference), because they are needed to discuss the combined dataset in the context of our manuscript's focus.

The authors have not measured DNRA rates and have not discussed its role in transforming riverine N to NH_4^+ in the estuarine sediments. They have not also measured sedimentary nitrification rates which is very important. I did not see any discussion on benthic N (NO_3^- uptake or NH_4^+ release) exchange. All these could have made the discussion on benthic N cycling robust. However, the authors should use the published data (if any) on benthic nitrification, benthic DNRA and benthic N exchange and thoroughly discuss the interplay of all N cycling processes in relation to net N loss/ immobilization in these sediments in the discussion section in general and section 4.2.4 and section 4.3 in particular. I suggest the authors to relook into the classic integrated discussion on benthic N cycling in the Gulf of Bothnia by Bonaglia et al. (2017).

We agree with the reviewer that it is a drawback not to have data on DNRA and nitrification rates in the sediment. DNRA rates from coastal sediments are scarce in the Baltic Sea: muddy sediment

(Jäntti et al. 2011 and 2012, Bonaglia et al. 2014, Hellemann et al. in prep), sandy sediment (Hellemann et al. in prep). These rates differ strongly between different study sites and study times. In situ nitrification rates in Baltic coastal sediment were to our knowledge so far only measured by Jäntti et al. (2011) in muddy non-permeable sediment. Further, Bonaglia et al. (2014) gives estimations of nitrification rates in muddy sediments of the Himmerfjärden (see response to comment no. 6.2). However, no data are available for sandy, permeable sediments, which comprise >50% of the area of Vistula estuary. It is thus very speculative to apply the DNRA or nitrifications rates from the literature to our study sites, but we will evaluate their potential role in the coastal benthic system of Vistula and Öre estuary.
Furthermore, measurements of DNRA in coastal sediments of the Baltic Sea and a lake suggest that DNRA rates are higher at low bottom water oxygen concentrations, especially under hypoxia, and can dominate over denitrification (Bonaglia et al., 2014; Jäntti et al., 2012; McCarthy et al. 2016). During our field campaigns we did not encounter hypoxic conditions or bottom water oxygen concentrations low enough to potentially enhance DNRA over denitrification in neither of the two studied estuaries.
Nevertheless, under oxic bottom water conditions coastal DNRA rates range from 1 – 487 µmol m^{-2} d^{-1} (Bonaglia et al., 2014; Jäntti et al., 2012), and might play a significant role in coastal N retention by recycling NO_3^- to bioavailable NH_4^+ which in turn may be further recycled within the coastal benthic system. Thus DNRA contributes to the residence time of N within the coastal zone.

The reviewer is correct, that we did not discuss the exchange of N (nitrate, ammonium) across the sediment water interface. We did not measure such fluxes ourselves, but we will check the literature to find values of such fluxes in the southern (e.g. Thoms et al., 2018 for Vistula estuary) and in the northern Baltic coastal zone and will evaluate their meaning for the manuscript's scope.

We thank the reviewer for these valuable suggestions and we will implement the role of other benthic N transformation processes and N fluxes on the coastal N filter function (retention vs. removal). However, we restrain ourselves from using rate data from other coastal sites (even though available) to calculate a benthic N-budget for our investigated estuaries as we find this too speculative. Especially, because the Baltic coastal zone is highly variable and so are the rates. Instead, we would like to adapt Figure 7 of the manuscript to show benthic N pathways in more detail, highlighting their roles as well as highlighting the gaps in our knowledge/ missing rate measurements.

In order to show the efficiency of these two estuaries as coastal filters, the authors should mention how much % of riverine N is ultimately lost in estuarine sediments through denitrification and/or anammox (if any), how much % is immobilized in sediments through DNRA and how much % is transported out of estuary to the coastal sea.

Please, see section 4.2.4, line 458, for how much % of riverine N is lost in estuarine sediments through denitrification. Unfortunately, we cannot estimate how much % N is retained in the estuarine sediments of Vistula and Öre estuary, because there are no DNRA rates available for our study sites.

For the Bay of Gdansk in which the Vistula estuary is situated, model results showed that ~46 % of the riverine TN inputs (Radtke et al., 2012) or ~77 % of the total TN inputs (riverine, lagoon, atmospheric) are transported out of the bay. However, the resolution of the model used by Radtke et al. (2012) is too low to resolve coastal N processing, and we doubt that some of the model assumptions in Witek et al. (2003) are realistic, especially regarding the N transformation rates and the water residence time. Furthermore, no estimates are available for the actual Vistula estuary, neither did we find results from the Öre estuary. We definitely agree with the reviewer, that it is important to discuss, how a coastal N-filter efficiency should be quantified and evaluated. We will use the valuable suggestions of the reviewer to improve our discussion in section 4.2.4 and 4.3.

Overall, I would suggest the authors to revise the manuscript by showing novelty of their study objectives, approach and findings which would make it appear as different from studies by Helleman et al. (2017) and Bartl et al. (2018).

New data that are presented in this manuscript are:
1) permeability, porosity, OPD, and pore-water NH_4^+ pools, and denitrification rates from the sediments of the Vistula estuary;
2) density stratification, δ^{13}C-POC, POC:Chl.a ratios, contributions of terrestrial or phytoplankton POM from the water column of the Vistula estuary;
3) pore-water NH_4^+ pools from the sediments of the Öre estuary;
4) density stratification, oxygen and nutrient concentrations (except bottom water, which is given in Hellemann et al., 2017), and POC:Chl.a ratios from the water column as well as nitrification and ammonium assimilation rates from the BBL of the Öre estuary.
These new data were combined with published data from Bartl et al. (2018), and Hellemann et al. (2017), as the reviewer correctly states. Through this combination, we gained an extensive data set covering both, water column and sediment. This facilitated a holistic comparison of the environmental conditions in two contrasting Baltic estuaries which together with the here presented N transformation rates facilitated an approach of evaluating the coastal N filter function. We are convinced that this is a novelty compared to the studies of Bartl et al. (2018) and Hellemann et al. (2017). We are certain, that with the valuable suggestions of the reviewer, we can emphasize this novelty even more in the revised manuscript.

For your information, we summarized below, the main messages of the two published studies:
Bartl et al. (2018) focused specifically on the regulation of nitrification rates in river plume and BBL of the Vistula estuary and offshore Bay of Gdansk through seasonal differences and short-term events (e.g. storm). This is a process-based study and does not discuss the role of nitrification or ammonium assimilation in the coastal filter function. Hellemann et al. (2017) focused specifically on the N-removal process denitrification under oligotrophic conditions and emphasized the role of cohesive (non-permeable) sands, which stand in contrast to the permeable sands of the southern Baltic coast. Furthermore, the authors indeed discussed the role of denitrification for the coastal N filter function of the Öre estuary, but did not present rates of N retention processes (e.g. nitrification). The suggestions of a coastal filter function via temporary

preservation of N within the Öre estuary are resumed in our manuscript and further supported by the new data presented.