Original comment no. 5: *Line 163: The authors mentioned that porewater was extracted at 2 cm interval from 5 cm to 11 85 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.*

Response of the authors: 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 90 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.

Counter Comment: *I agree with the increasing trend of porewater NH_4^+ in many coastal marine sediments but it is wrong to say that porewater NH_4^+ can be captured at 2 cm intervals. Well, let's say If you have a core of 10 cm long, you can extract porewater (by Rhizon tubings) at 0-1 cm, 1-2cm, 2-3cm, 3-4cm and so on and it would obviously represent porewater NH_4^+ of these 1 cm intervals. You can also extract porewater at 0-2 cm, 2-4 cm, 4-6 cm, 6-8 cm and 8-10 cm but it would not represent the porewater NH_4^+ of these entire 2 cm intervals rather it would represent the porewater NH_4^+ from 0.5-1.5 cm, 2.5-3.5 cm, 4.5-5.5 cm, 6.5-7.5cm and 8.5-9.5 cm respectively. So, it is OK to show/consider porewater NH_4^+ values at 1 cm, 3 cm, 5 cm, 7 cm and 9 cm in a vertical profile plot which actually means that there are some gaps in NH_4^+ values but nevertheless, it is OK as we get an overall increasing trend with depth.*

Original comment no.7: *Section 2.3.2: The authors have not given a diagram for diffusive experimental set-up.*

***Response of the authors*:** 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) 160 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.

Counter Comment: *None of the above 8 references cited by the authors has a figure of diffusive set-up. So it would be hard for the readers to visualize and understand the experiment method particularly while comparing to advective set-up. I suggest the authors to present a proper citation which actually has a figure of diffusive set-up or show a schematic diagram of the diffusive set-up.*

Original comment no. 17.2: *Please present few figures depicting increase in $^{15}N-N_2O$ and $^{15}N-N_2$ with time to support your conclusion on denitrification being a major N loss pathway. Similarly, if you find anammox and 340 DNRA upon re-analysis of the incubation data, then please show the proof in terms of additional figures.*

Response of the authors**:** The presence / absence of anammox, thus its significant /non-significant contribution to total N_2 production and the consequential role of denitrification in N2 production were investigated by 345 concentration series (Risgaard-Petersen et al. 2003), not in time-series.

In the concentration series, D15 (= the denitrification of $^{15}N-NO3-$) has to correlate with increasing tracer concentration to fulfil 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 350 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 N2 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), 355 as was the case in all incubations.

Counter Comment: *I think it would be better if the authors show these figures in supplementary section.*

Original comment 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.*

Response of the authors**:** 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 705 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.

Counter Comment: *For a comparative analysis, it would be better to reproduce porewater O_2 profile of Ore estuary (with proper citation) along with that of Vistula estuary*

Original comment no. 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 730 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.
***Response of the authors*:** 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.

Counter comment: *I could not see any clarification on depth ranges in section 2.1.*

***Overall comments & suggestions*:** *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.*
***Response of the authors*:** 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) 825 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.

Counter comment: *I could not find the section 4.2.4 in the revised manuscript. If the authors actually meant section 4.3 and 4.4, then it's OK.*

Minor grammatical/typographical mistakes in revised version

Line 259: *In coastal water column (river and river plume excluded)*.....When you say coastal water column that practically means shelf waters of adjacent sea and it is out of estuary. This would be confusing for the readers. Please use an appropriate word.

Line 334: .....*may* **"be"** *the reason....*

Line 436: ....by increasing **"the thickness of"** oxic-anoxic interface........

Line 456: Replace "In the two here studied estuaries..." with "In the two estuaries studied here..."

Line 457: ....*benthic processes* **"such as"** *nitrification,*....