Response to RC1

Dear Reviewer,
Thank you very much for your comments and suggestions. In the following you find the responses to your specific comments (bold) and changes in the manuscript text (italic).

Best regards,
Ines Bartl, Dana Hellemann and all co-authors

The authors should explicitly state how they define the BBL, since the BBL figures heavily in the analysis. They do mention it is based on density stratification and provide some references, but they should say exactly what criteria they used.

The reviewer is right and we will add more detailed information to the manuscript, section 2.1:

The BBL is generally defined as the water layer directly above the sediment (Richards, 1990). It is characterized by turbulent boundary layer flow 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).

This is the criteria that was chosen in our study (see Figure R1).

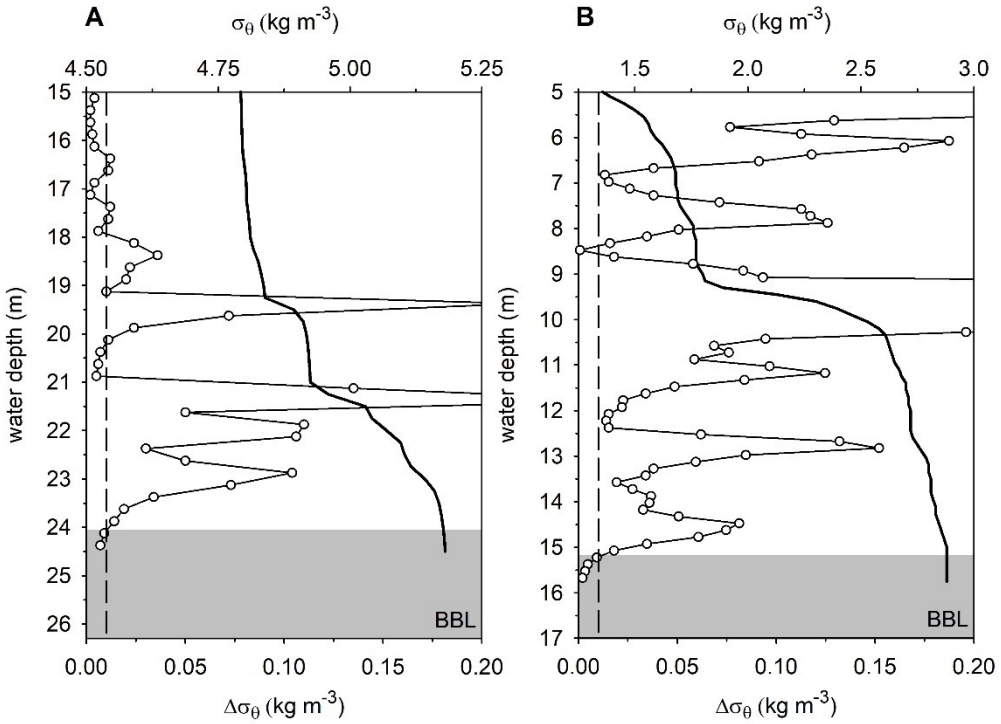

Figure R1: Vertical profiles of the potential density σ_θ (black thick line) and the variation of the potential density $\Delta\sigma_\theta$ (white circles) and the threshold of $\Delta\sigma_\theta$ at 0.01 kg m^{-3} (dashed line) of station VE05 from the Vistula estuary (A) and N3 from the Öre estuary (B). The difference between the bottom depth and the depth at which $\Delta\sigma_\theta$ exceeds the threshold makes the vertical extent of the BBL (grey box).

I find the presentation of the sediment characteristics in Table 3 a bit confusing. Why are the LOI values not provided as a mean and standard deviation like the OPD and NH4+. Also it seems that the range of LOI values is quite wide in some cases, (ie. Ore Spring 1.9-12.8 dw%) this implies to me there are many different sediment environments grouped together. Likewise the variability in the ammonium pools within sites is also quite large. It seems there might be more information here that could prove useful if the authors looked at this variability in greater detail. Presumably the high LOI values and high ammonium values come from the same sediment cores. Also I find the per area inventories of NH4+ a little bit of a strange way to present this, I think pore water ammonium profiles would useful to see as well. Why go through the effort of section cores and extracting porewater profiles with Rhizons if you are not going to show the changes with depth.

The reviewer is correct in the assumption that in the Öre estuary two types of sediments were grouped together: silts and silty fine sands. While these are two distinct sediment types with different LOI values due to different organic matter sorption capacities (Mayer 1994a, Hedges & Keil 1995), they shared the same mass transport mechanism (diffusion and fauna-induced fluxes), as both had a too low permeability to enable advective pore-water flow. Thus, we grouped these two sediment types together as "non-permeable sediments". This was also supported by the similar oxygen penetration depth and denitrification rates of both sediment types (in detail explained in Hellemann et al. 2017). Probably only a small amount of the organic matter measured as LOI was labile, resulting in similar process rates despite different organic matter contents. As the different sediment types were discussed in detail in Hellemann et al. 2017, we did not want to repeat those results. We will change all values in Table 3 into mean and standard deviation.

The reviewer is also correct in the view, that some information is lost when calculating depth integrated element pools rather than showing actual pore-water profiles. However, our aim was to compare the NH_4^+ inventory in the sediments of the two estuaries, which give indications of long-term organic matter accumulation in the estuarine benthic system and the trophic condition in the estuary. Per area inventories of pore-water NH_4^+ have also been used to investigate different sediment types e.g. in the North Sea (Ehrenhauss et al. 2004).
Indeed, the NH_4^+ pools exhibit some variability within each estuary and sediment type ("permeable", "non-permeable"). This is due to the strong patchiness of sediment properties, well-known for coastal sediments. High LOI values co-occur with high pore-water NH_4^+ values only in the Öre estuary (Figure R2, grey symbols). However, similar to LOI, the different pore water NH_4^+ concentration did not influence rates of coupled nitrification-denitrification in the surface sediments there. In the Vistula estuary high LOI values do not always co-occur with high pore-water NH_4^+ values, likely due to high turnover of organic matter e.g. macrofaunal influence (Thoms et al. 2018).
We are of the opinion, that including the pore-water NH_4^+ profiles (Figure R3) in the manuscript would not add additional information in respect to the scope of our study. However, if the reviewer recommends adding such figures, we are happy to add them to the supplementary material.

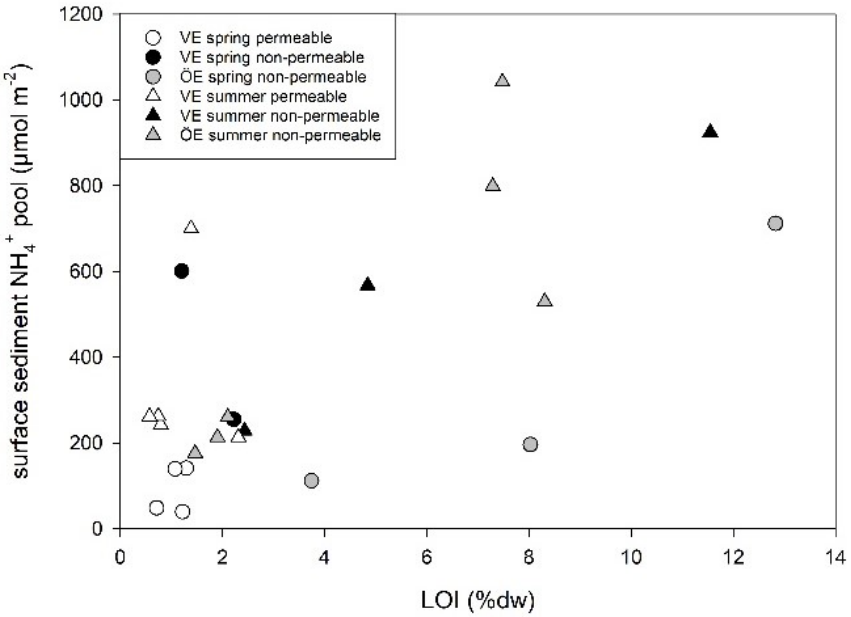

Figure R2: The pore-water NH_4^+ pool (0-2cm) plotted against the organic matter content (LOI) in the surface sediment (0-2 cm).

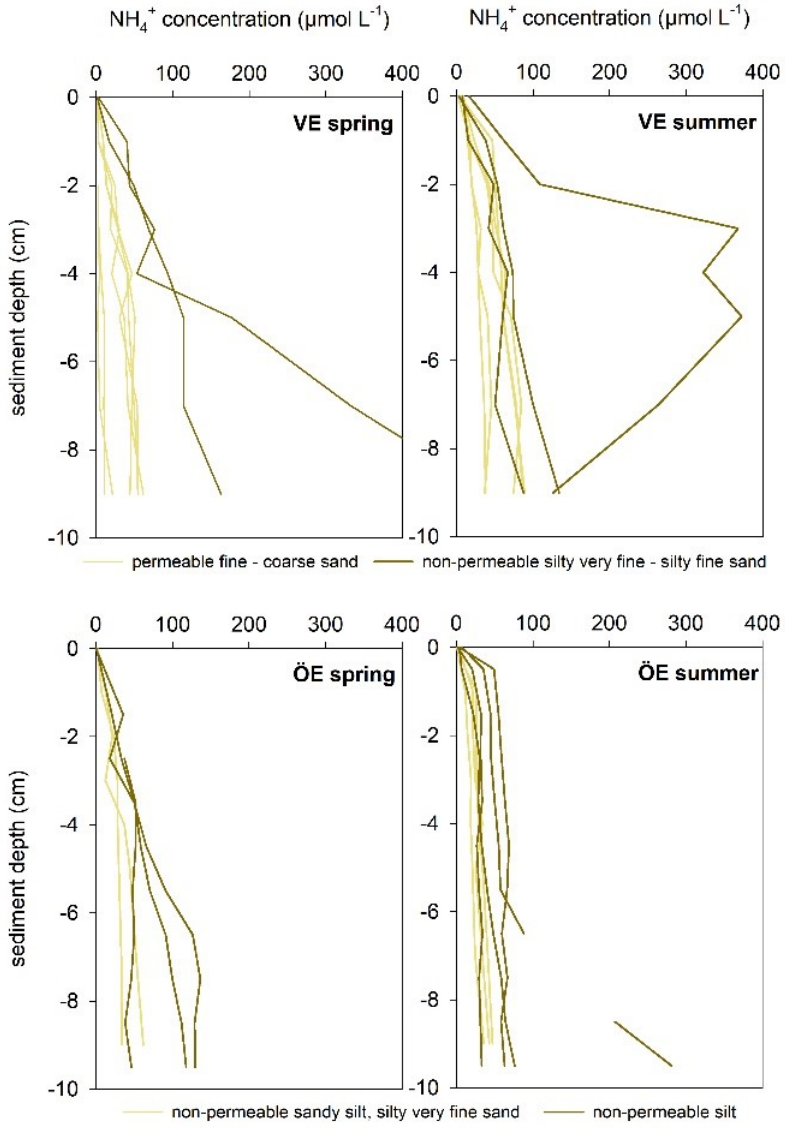

Figure R3: Pore-water ammonium profiles of the Vistula (VE) and Öre estuary (ÖE) in spring and summer. Please note the different colour legends for the two estuaries. All sediments of the Öre estuary were non-permeable.

The authors mention that the NO3-+NO2-, PON, POC concentrations in the BBL were significant higher in Ore than in the Vistula estuary (again no statistics) they mention this is due to the long particle retention time of the Ore estuary compared to the more open unrestricted bottom topography of Vistula. However one of the most striking features of the BBL chemistry in Figure 3 and Table S1 is the accumulation of NH4+ in the BBL of Vistula. I am wondering how the authors reconcile these two points.

The reviewer is right that the high summertime NH_4^+ concentrations in the BBL of the Vistula estuary contradicts the interpretation of the distribution of NO_3^-+NO_2^-, PON, and POC concentrations (line 302-310). We suggest that in the Vistula estuary thermohaline stratification reduced the vertical mixing and led to low bottom water flow-velocities (see sect. 4.1.3). In consequence, this led to low lateral transport and likely allowed the accumulation of NH_4^+ in the BBL in summer, despite the estuary´s open shape.

Changes from Line 306 onwards:
The accumulation of elements in the benthic system of the Öre estuary is favored by the basin-like bottom topography and restricted bottom-water exchange, which allows a long particle residence time of more than one year (Brydsten and Jansson, 1989). Not only the coastal bottom topography, but also lateral bottom water flow or vertical mixing could influence the accumulation of dissolved nutrients or POM. The open shape and unrestricted bottom topography of the Vistula estuary may not allow accumulation in the benthic system. However, thermohaline stratification reduced the vertical mixing, and led to low bottom water flow-velocities (see sect. 4.1.3). The resulting low lateral transport likely allowed the accumulation of NH_4^+ in the BBL in summer, despite the open shape of Vistula estuary.

Statistics were made as described in section 2.4 (lines 230-235). We decided to define the significance level in this section rather than adding it after every comparison/sentence in the results section, as we thought this would disturb the reading flow. If the reviewer recommends to change this, we are happy to do so.

The authors make a big deal about the difference in bottom topography and the role it plays in differences in N-cycling, however the estimated bottom topography in Figures 2 and 3 does not look that different to me. The authors should explain these differences in bottom topography in more detail to make a more convincing argument. In section 2.1 the authors state that the "deep waters of the [Ore] estuary are confined by a small elevation (~30 m water depth) at its southern border." This to me implies there is some sort of sill that restricts the exchange of bottom water. But I do not see any such feature in the map in Figure 1 or the bottom topography of Figures 2 or 3 that would restrict flow, 30m seems to be the deepest water depth and it appears to occur right at the estuary mouth. The authors need to explain this a bit better, and provide stronger evidence for the restricted circulation.

The bottom topography of the Öre estuary is well described in several studies (e.g. Brydsten et al., 1992: Brydsten and Jansson, 1989; Forsgren and Jansson, 1992; Malmgren and Brydsten, 1992) and as the reviewer recommended we will describe this in more detail (line 98):

The Öre estuary is located on the Swedish coast of the Quark Strait, northern Baltic Sea (Fig. 1). It is partly separated from the open sea by an archipelago to the east and by land to the west,

and has a basin-like bottom topography with a hydrography depending on local wind conditions and river discharge (Brydsten, 1992, Fig. 1). The outlet of the Öre estuary in the south is relatively wide in the surface but becomes narrow at water depths >20m (Brydsten, 1992; Malmgren and Brydsten, 1992). A small elevation at ~30-25m depth separates the estuarine bottom waters from the open sea (Brydsten, 1992, Fig. 1). The Öre estuary covers an area of ~71 km^2 and has a volume of ~1 km^3 (SMHI, 2003). Inputs into the estuary originate from the Öre River, whose mean discharge is 36 m^3 s^{-1}, creating a river plume of 2–3 m vertical and ~10 km horizontal extent (Forsgren and Jansson, 1992). The water turnover time (estuarine volume/river discharge) is ~9 days (Engqvist, 1993). The sediments, covering ~20% of the estuarine are consist of silts, very fine and fine sands, all non-permeable (Hellemann et al., 2017).

The sampling transect of the Öre estuary shown in Fig. 2 and 3 did not include the southern estuarine outlet (see Fig.1 in the manuscript), where the small elevation exists (compare to Fig. 1 in Brydsten 1992). We agree with the reviewer that Figures 2 and 3 should include this elevation to better visualize the differences in bottom topography between Öre and Vistula estuary. Example of transect including the elevation is shown in Figure R4.

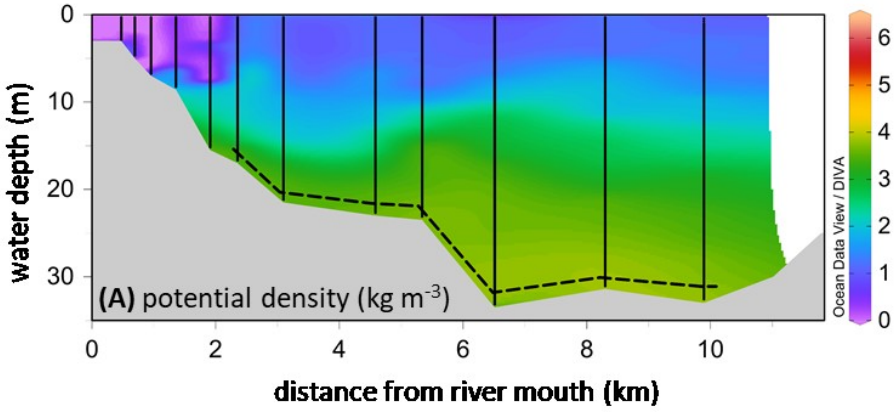

Figure R4: Updated transect from river mouth to estuary outlet of Öre estuary.

In Section 2.2.1 the authors mention the high CDOM content of the Ores estuary and that they needed to do a correction to account for this in their nutrient analysis. If this is the case I think it is likely that this CDOM would interfere with the in-situ Chla measurements using the optical sensor. If the optical properties of the water in the both estuaries are different (due to CDOM levels) how accurate/comparable are the chlorophyll a cross-sections in Figures 2 and 3?

The optical properties of the water are indeed different in the two estuaries, with higher cDOM levels in the water of the Öre estuary. Figures 2 and 3 show Chl.a values which were measured with an optical sensor in the Öre estuary and manually with the fluorometric method in the Vistula estuary. The Chl.a-transects given in Figure 2 and 3 are only used to illustrate the presence or absence of phytoplankton in the water column and should not be compared to each other, which is now also explained in the figure captions (here as example for caption of Figure 2):

Figure 2: Environmental variables of the water column along a sampling transect from the river mouth to the outlets of the Öre (left) and Vistula (right) estuaries in spring. Please note, due to different optical properties of the water and different measurement methods, the Chl. a

values are not directly comparable between the two estuaries, but provide qualitative information on the presence/absence of phytoplankton. Bottom topography was estimated from the water depths of the stations. The dashed line represents the vertical extent of the BBL. The plots were derived from 12 (Öre estuary) and 4 (Vistula estuary) profiles using DIVA-gridding in Ocean Data view (Schlitzer, 2015).

For the calculation of the POC:Chl.a ratios in the Öre estuary, we used Chl.a concentrations manually measured with the HPLC analysis, kindly provided by Lumi Haraguchi (Aarhus University). Unfortunately, the manual Chl. a measurements were not done at all stations from the Öre estuary, so the data is sparser than for the Vistula estuary. We compared Chl. a concentrations from the optical sensors with the manually measured Chl.a concentrations and found the majority of values being similar (Figure R5). This shows that the optical Chl.a data from the Öre estuary given in Figure 2 and 3 are likely not strongly biased by cDOM.

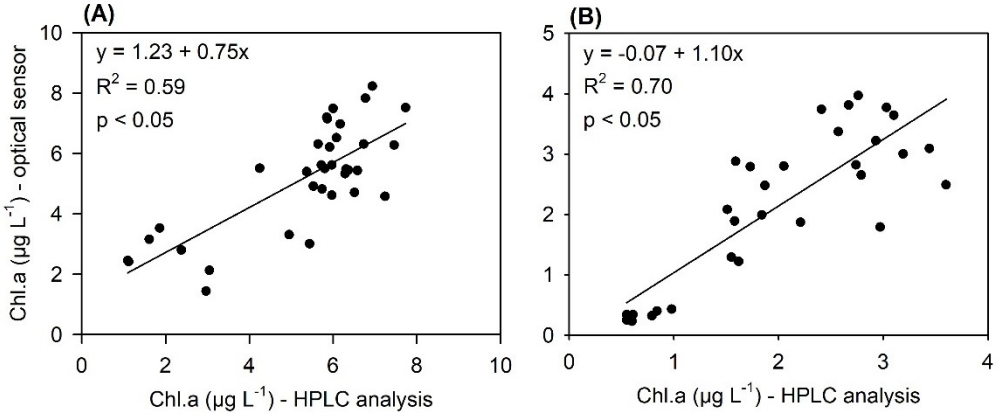

Figure R5: Chlorophyll a (Chl.a) concentrations measured by HPLC analysis vs. Chl.a concentrations measured with an optical sensor in the Öre estuary in spring (A) and summer (B).

On line 385 the authors mention temperature as the factor determining higher ammonium assimilation rates in the summer, which could very likely be a contributing factor, but couldn't this also just be a concentration effect since NH4+ concentrations are so much higher in the summer (Figure 3).

The reviewer is correct in that the higher NH_4^+ concentrations in the BBL in summer than in spring may contribute to higher summertime NH_4^+ assimilation rates in both estuaries. In summer, i.e. at higher temperatures than in spring, degradation of organic matter to NH_4^+ is enhanced, and thus the combination of both environmental variables likely control seasonal variation of NH_4^+ assimilation rates. We changed the text at line 385 accordingly:

Because ammonium assimilation is a substrate- and temperature-dependent process (Baer et al., 2014; Hoch and Kirchman, 1995), the combination of both, high NH_4^+ concentrations and elevated temperatures in summer enhanced ammonium assimilation rates in both estuaries.

Line 401 states: "*In summary, the magnitude of nitrification and ammonium assimilation in the BBL was not influenced by the different trophic state or by seasonal differences. However, the regulation of those two processes differed depending on the trophic state, i.e. the availability of organic N from POM.*" I do not understand this statement. How is the magnitude of nitrification and ammonium assimilation not influenced by differences in

trophic state, when figure 6 shows a clear correlation between these rates and the concentration of PON.

We agree with the reviewer that this statement is not clearly formulated. We decided to remove this statement from the manuscript text.

Line 429, The authors state the dominance of the NO3- source is controversial what is controversial about it? The authors should elaborate on this a bit more.

This statement refers to the nitrate source of benthic denitrification in permeable sediments and is discussed in more detailed in 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).

I think Figure 7 would be more effective if numbers were put to the various arrows, it seems the authors have constrained at least some of these flows, and would be valuable to indicate which ones were known.

We intended to present a conceptual view and thus had left out numbers in Figure 7. However, we agree, that it would be valuable to indicate what is known and what remains unknown, either by numbers or by question marks. Of the arrows presented in Figure 7, we measured the rates of ammonium assimilation, nitrification and denitrification. Transport rates are unknown, particle residence time is only estimated for the Öre estuary, and numbers for primary production and sedimentation rates may be found in the literature. We will revise Figure 7 and upload it latest in the revised manuscript.