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Interactive comment on “Spatial variations in sedimentary N-transformation rates in the North Sea (German Bight)” by Alexander Bratek et al.

Anonymous Referee #2

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This is a well-written study of the sedimentary N cycling in the Southern North Sea. The novelty of this work is the quantification of ammonification rates and comparison of the nitrogen conversions in the different sediment types. Rates were determined with an isotope dilution method applied in laboratory incubations.

Key results include sedimentary ammonification and benthic N₂ production rates. The concentrations and rates determined in the study provide valuable data for the assessment of the nitrogen conversions in the North Sea, and the role of the sediments as sinks, sources and sites of nitrogen regeneration and N release from the system. The study contributes baseline data that can be used in future studies for the evaluation of potential changes in the North Sea N-cycle.

My two main concerns with the study are related to the methods used. The first concern

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is that the conclusions of the work are based on one sampling in summer 2016 (a year with record temperatures), which does not allow assessing the variability and representativeness of the concentrations and rates measured. This does not take away from the value of the data but weakens conclusions when extrapolating the results, e.g. when stating “Based solely on our data, we estimate a total nitrogen removal of 894 t N d-1 in our study area”. This needs to be made clear throughout the paper, including the abstract.

My second concern is the incubation method that is not explained in sufficient detail and that according to the authors did not adequately reproduce the influence of water exchange in the permeable sediments. Ammonification, nitrification and denitrification likely are affected differently through the changes in the water exchange. Since these exchange flows may carry organic matter and oxygen into the sediment, the N-concentrations and conversion rates hinge on this transport and therefore can be altered by changing this flow. The authors pointed this out in the discussion and also added a calculation based on primary production in order to estimate the upper limit of denitrification, nonetheless, the in-situ N-conversion rates therefore may differ significantly from those determined in the lab. This should be made more clear.

The authors may consider including the following references:

Villa et al. (2019) “Benthic nitrogen cycling in the North Sea” Continental Shelf Research.

Kitidis et al. (2017). Seasonal benthic nitrogen cycling in a temperate shelf sea: the Celtic Sea. Biogeochemistry

Detailed comments

Line 16: Dissolved inorganic nitrogen concentration (nitrate, nitrite and ammonium) in the water column showed low levels between 0.2 to 3.2 $\mu\text{mol L}^{-1}$. Was the sampling always done at the same daytime?

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Line 49: In spite of their putative relevance as an ecosystem service, very little is known about N cycling and N transformation rates in the sediment. Substantial work has been done on sedimentary nitrogen in the North Sea (e.g. see reference lists of the two papers mentioned above) and I suggest softening this statement.

Line 109: cores. . . were incubated in a gas tight batch-incubation setup for 24 hours. The setup of this incubation is critical for the interpretation of the results and the authors need to include more detail: Were all batch incubations done on board or back in the lab? What was the time delay between coring and incubation? What was the length of the sediment cores and were all cores of equal length? What was the height of the water column above the cores, and what were the starting and end oxygen concentrations? Was the water column fully mixed in the core liners? Was stirring the same in all cores? What was the fauna in these cores and how was its activity accounted for? What were the light conditions during incubation? Were all cores incubated with the same water or with the water they came with?

Line 114: NH₄⁺ and NO₃⁻ concentration of the added tracer solution was the same as the bottom water concentrations (Tab. 2). This is not clear. Does this mean that you added tracer while maintaining the original nutrient concentration, i.e. removed some water and then added a mix of water and tracer that had the same nutrient concentration as the original water? If that was the case, please specify how much tracer was added and if the different tracer additions (nutrient concentrations differed up to factor 20) could have influenced the incubation experiment. In Line 116 you mention that the label addition was calculated aiming for a maximum enrichment of 5.000 ‰ in substrates and products. How was this achieved if added tracer solution concentration was the same as the bottom water concentrations, and the incubations ran with the same volume of overlaying water?

Table 2: Random frictionless packing in sand produces a porosity of 0.39, and although lower numbers can sometimes be measured, a porosity of 0.29 seems unrealistic. Was the core fully water-saturated? Please check whether these numbers were reported as

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weight or volume ratios. Practical salinity is based on ratios and should be expressed by dimensionless number only

Line 118. Upon sampling, incubation water was filtered with a syringe filter (material, manufacturer, $0.45 \mu\text{m}$ pore size) Insert material and manufacturer.

Line 157. The surface sediment samples of the cruises HE 383 (06/07.2012) and HE 447 (06.2015) for NOAH-D were analyzed for total carbon and total nitrogen contents with an elemental analyzer (Carlo Erba NA 1500) via gas chromatography calibrated against acetanilide. Please be more specific: How deep was the sediment layer termed "surface sediment"?

Line 184: Three O₂ profiles were measured in one sediment core of each station. Please specify the conditions: When exactly where those profiles measured, i.e. how long after retrieval of the core? What were the conditions during the measurements e.g. was there a water layer above the sediment?

Line 195: The lowest oxygen flux was determined at the permeable sediment station NOAH-A with $-10.0 \text{ mmol m}^{-2} \text{ d}^{-1}$ (Fig. 2), the highest oxygen flux was measured at the impermeable sediment station NOAH-C with $-53 \text{ mmol m}^{-2} \text{ d}^{-1}$. The semi-permeable sediment station NOAH-D had an oxygen flux of -18.5 to $-30.6 \text{ mmol m}^{-2} \text{ d}^{-1}$. As pointed out by the authors in the discussion section, the fluxes in the permeable sediment vary with the flow above the sediment. For which flow setting were the fluxes reported here? The same question applies to the statement in Line 203: The lowest ammonification rates were measured in the semi-impermeable sediment at station NOAH-D

Line 284: In total, though, we estimate that benthic N fluxes support between 13 % (at a water depth of 38 m) and 61 % at 10 m depth (Tab. 3) of primary production As this is based on one time summer sampling only, I suggest softening this statement.

Line 307: Nitrification rates are relatively independent of permeability, in contrast to ammonification.

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This needs further explanation. In the discussion, you mention the potential importance of the flushing of the permeable sediment, which could transport organic matter and oxygen into the sediment. This would have direct implications for both, ammonification as well as nitrification. Why was nitrification relatively independent of permeability?

Line 311: Nitrification rates are lowest at Station NOAH-A. Here, oxygen penetration depth is highest, and the sediment has low organic matter content (Tab. 2), which obviously limits nitrification rates. This statement contradicts the statement on line 307, where you say that “nitrification rates are relatively independent of permeability”

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