

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

Anonymous Referee #1

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General appreciation

Interesting manuscript, generally well written but lacking in clarity at several points. This makes the conclusions stretched in view of the inability of the reader to properly assess what was done, and how. Perhaps a bit rushed in several instances (missing information, repeated words, misplaced words, reference list seemingly too long compared to actual citations in text, etc). Some important statements are inadequately substantiated. I would suggest a careful re-write, addressing the main concerns below – interpretation of data, discussion (including scale-up to whole basin) and conclusions seem unassailable without this being done.

Main concerns

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- Lines 71-74. It is suggested that the isotope dilution method employed in this study can 'unravel several N-processes like ammonification, assimilation, nitrification, denitrification (...)', DNRA...etc within sediments. This requires demonstration, which is not clearly apparent either after this statement or in fact within the rest of the manuscript. The authors seem to use ^{15}N isotope dilution methods as first developed by Koike and Hattori (1977). This is a powerful technique, but not without issues. These have been well documented by the Aarhus group in the 80's and 90's. The main issue is the underestimation of both nitrification and denitrification due to the inability of the technique employing only $^{15}\text{NO}_3^-$ to adequately account for coupled nitrification-denitrification, even if this is paired with other methods – such as the acetylene inhibition technique or any kind of budget modelling (as in fact supports the study cited by the authors as Nishio et al 2001b). The solution attempted in some cases (Nishio, et al., 1983) passes through using the $^{15}\text{NH}_4^+$ dilution technique of Blackburn (1979) in parallel, requiring the simultaneous incubation of two core samples – one amended with isotopically labelled nitrate and the other ammonium. Denitrification could be then estimated as the sum of denitrification rates measured in the two cores, and both nitrification and coupled nitrification-denitrification from the $^{15}\text{NH}_4^+$ labelled core. Indeed, it seems that was what was done (Lines 113-115), with two main consequences: one, affecting the overall evaluation of uncertainty of the results of the various rates on evidence in this manuscript because fewer replicates of each incubation were available in reality (2 of each label for each station, instead of 4 mentioned in lines 107-109, and in the case of station D, only one, Line 113), making the account of propagation of uncertainty challenging (I will return to this point later); and two, an added uncertainty introduced by the amount of $^{15}\text{NH}_4^+$ added to the cores (as the authors state, NH_4^+ concentration in bottom waters are usually very low, Lines 234-235) and how this would affect the accuracy of estimates of nitrification and coupled nitrification-denitrification (see for example, Henriksen & Kemp, 1988 and Rysgaard et al., 1993), particularly because these cores were quite bioturbated (Lines 256-260). I wouldn't find it surprising under these circumstances that it is difficult to explain the relatively high ammonification

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rates compared to the benthic oxygen consumption (page 9) and the lack of correlation between gross ammonification and nitrification rates (Line 317).

Based on this general assessment, I would suggest the following issues addressed:

- Present a clearer description of the methodology used to unravel all the process rates mentioned. Specifically, the differences between net and gross rates, and the process by which sediment-water fluxes measured in core incubations are translated into process rates, perhaps with a diagram of the steps taken to discriminate between different rates. It should also be explained that the sediment-water exchanges measuring method would imply that these are steady state fluxes being measured. Are the fluxes measured in the same cores that were amended by the isotope labelled compounds? Etc – clarity on this front would help clarify some of the potential queries rising from reading the manuscript in its current form.

- Propagation of error and significance of differences encountered between different rates at different locations needs to be discussed appropriately, and concrete results presented in this regard. Rates are presented without appropriate attention to this point – and often calculated on the basis of sums or differences between terms with significant individual uncertainty (see above for total denitrification, which would be estimated from the sum of two rates obtained from two different core incubations, of which there is only one replicate in 75% of the cases and none for 25%). For example, in Table 3, in the second column, the sum of three process rates is presented with relatively low uncertainty – is this from a flux measurement and therefore this is an aggregate? (see above). Another example: see formula 3 (page 6). In NOAH -C (Line 201) to calculate assimilation, the difference between gross and net ammonification alone if propagation of error is accounted for would already be a highly uncertain number: $(8.3 \pm 2.3 - 6.8 \pm 2.3 = 1.5 \pm 3.25)$ and we haven't yet subtracted the gross nitrification rate. The chosen mode of representing uncertainty is also not explained – how is it calculated (looking at the size of the error bars and the spread of results, I presume as the standard error of measurements, with $n=2$ or 1 ? Or $n=4$ or 3 ?), but has to be made explicit

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– are we dealing with the standard error ($\sqrt{\sigma/n}$), σ , $2^*\sigma$, $3^*\sigma$? This step is in my view critical to properly asses the results in context.

- Present an explicit discussion of the potential pitfalls of using the ^{15}N dilution technique without isotope pairing (Rysgaard, Nielsen et al). In particular, the issues with underestimation of denitrification and nitrification associated with the coupling of both processes and how they could affect interpretation of the results, not mentioned on the manuscript.

Additional points to address

Line 49-50: “very little is known about N cycling and N transformation rates in the sediment”. This is a very strong statement and has to be amply justified although I see no evidence of such being the case in the literature. At least, it should be contextually set in a much better way. The justification coming afterwards is rather confusing – are the authors suggesting nothing is known about benthic N cycling in the North Sea because previous studies failed to distinguish between mineralization and nitrification?

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