

Interactive comment on “Vertical activity distribution of dissimilatory nitrate reduction in coastal marine sediments” by A. Behrendt et al.

Anonymous Referee #1

Received and published: 18 June 2013

GENERAL COMMENTS

This manuscript presents results from an interesting study that addresses a key question in work on benthic N cycling. I like the comparison of distinctly different sediments from various locations. However, after reading this manuscript twice I was still left with quite a few question marks :

The comparison of DEN versus DNRA in the intact cores relies on two rather different methods that are both indirect (fluxes inferred from depth profiles) and appear to rely on quite some assumptions and conversions. For the slurry incubations, a different method was used for quantification of denitrification (tracing ^{15}N from $^{15}\text{NO}_3$ directly into N_2 , simultaneously with tracing ^{15}N into NH_4 for quantification of DNRA). The latter provides a more direct comparison of the two processes and therefore inspires

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more confidence to me. The fact that different methods were used to analyze denitrification in the cores versus slurries means that the comparison of results for intact cores versus slurries also contains a potential bias from this difference in methods. This should be addressed in the manuscript.

It is reported that DNRA only occurred at one location (Janssand) because that was the only location where a depth profile of 15NH_4 could be measured that was suitable to infer a 15NH_4 flux from. However, cores from three of the five other stations also showed high levels of 15NH_4 production (just not the suitable profile) which means that DNRA also occurred here. This is addressed in the discussion, but not included in the main results (Fig. 2). It seems that these results are not fully exploited because they do not fit the micro profile methodology. Thereby, the actual importance may be underestimated. See further specific comments below (e.g. 8077, 24-26).

Another point is the fact that some potentially relevant processes are not, or scarcely, addressed. This is particularly relevant for nitrification and anammox. Both appear to be relevant at least in some of the sediment given the high concentrations of NH_4 . If these processes were not relevant, this should be explained in the text. Also see some specific comments on this below.

Something that is not addressed is the fact that high concentrations of nitrate were added. These were much higher than ambient concentrations (except for Limfjord), meaning that measured rates/fluxes would actually be potential rates/fluxes. This should be addressed in the paper. Moreover, this may also bias the results with respect to the balance between DEN versus DNRA which appears to be dependent on the availability of nitrate as addressed in the discussion (see comments to relevant parts of the Discussion).

SPECIFIC COMMENTS

-8066, 8: Indicate range (59-130% rather than >59%).

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-8066, 11: It would be better to move the section between brackets to end of the sentence.

-8066, 18-20: Maybe include a sentence saying that you expect the real contribution of DNRA to have been in between that in the intact cores and that in the slurries. The current sentence is really method-focused.

-8066, 23: Not sure if “predisposition” is the correct term here.

-8067, 1-4: Here, anammox is addressed, but then it sort of disappears. It should be made clear that anammox was not included in the DEN measurement (assuming that the acetylene inhibition method is specific for canonical denitrification) and it should be explained why anammox was ignored.

-8068, 15: No need to start new paragraph here (text is direct continuation of the preceding).

-8069, 18: Overnight incubation at 15 degrees is quite a strong deviation from in situ temperatures for Arhus Bay and the Mississippi delta. Potential implication?

-8070, 25-27: So the inhibitor is specific for denitrification, meaning that anammox is not included in these measurements ? > this should be made clear (see other comments on this).

-8071: The calculations of DEN from the microprofiles appears to be based on quite some assumptions and coefficients. It is difficult to get a feeling for how strongly the outcome of these calculations is influenced by these factors. Maybe there is a way to clarify this. This also applies to the DNRA calculation on page 8073.

-8072, 12-13: Explain why spikes of 14NH_4 were added (used as carrier because ambient NH_4 concentrations were too low for detection?) and that this was later corrected for (I assume).

-8072, 18-19: Just different concentrations or also different atom% ^{15}N ?

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-8074: 18: Explain how NH_4 adsorption was subsequently calculated.

-8074: Write CNS in full.

-8075, section 2.7: It makes more sense to present this experiment before presenting all the analytical techniques.

-8076, 3-4: The KCl addition seems like a good method to overcome possible adsorption of 15NH_4 to the sediment. Then why was the extra adsorption experiment performed? It seems like results from this experiment are not needed for calculation of the DNRA rates when results were not biased by adsorption.

-8077, 24-26: If I understand correctly, it is stated that there was only DNRA at Janssand because that was the only sediment with a usable shape of the sediment profile of 15NH_4 . However, when I look at Fig. 1, I also see clear production of 15NH_4 for Arhus, Mississippi, and Limfjord, with most in the same range as for Janssand. It seems strange to me to interpret these results as no DNRA. If no fluxes can be calculated from these profiles, alternative ways to derived DNRA rates from these data should be investigated. Total 15NH_4 production should provide a good indication of DNRA rates. If there is some reason not to do this, then this point should at least be addressed more explicitly in the discussion. (also see general comment).

-8078, 4-5: (“No N_2O was detectable”) > There is N_2O present in Dorum sediment without acetylene in Fig. 1f > explain.

-8078, 6-13: See previous comments to 15NH_4 production. Moreover, this section includes some interpretation and speculations that would be more at place in the discussion.

-8078, 11-13 and 20-22: Wasn't adsorption of 15NH_4 to the sediment prevented by addition of the KCl to the sediment samples for these analyses? This would mean that there is no bias from adsorption here. (see previous comments).

-8079, section 3.5: I think that results from this experiment deserve to be presented in

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a figure (stacked bar or similar).

-8080, 10-12: It should be better explained how these factors may have influenced these results. Moreover, the contribution by anammox would only apply when anammox was included in the DEN measurements, which appears not to have been the case (see other comments on this).

-8080, 18 (“In fact, in all four sediments, the measured 15NH_4^+ concentrations clearly exceeded the natural abundance of 15NH_4^+ usually found in the pore water of coastal marine sediment”): This means that DNRA did occur !? (see other comments on this).

-8080, 20-22: These mechanisms also involve DNRA, right? (see comment above and others).

-Section 4.1: What I found missing in the discussion here is an evaluation of how the high added nitrate concentrations (compared to ambient concentrations) may have affected the outcome here. This seems particularly important according to this statement from section 4.2.1: “Supposedly, DNRA is the favored pathway under nitrate-limited conditions, while DEN is the favored pathway under nitrate-replete conditions”. (see general comment).

-8081, 2-4: When KCl was added to the samples, then adsorption was not an issue here ? See other comments on this.

-8081, 17-27: What about nitrate production by nitrification in the sediment as an additional source of nitrate ? I was also wondering how this would affect the various depth profiles and their interpretation. If nitrification was relevant indeed, this should be addressed.

-8082, 11-24: This includes quite some repetition of results (not necessary).

-8082, 26: Inhibition by what? Sulfide? > explain.

-8083, section 4.1.3: This section can be merged with the previous

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-8084, 9: “partitioning” can be removed.

-8086, 20-25: See previous comments on this.

-Figure 1. This figure is nice. However, as printed now it is too small. This should be a full page figure. In addition, some colors could be added for better distinction between the different profiles. pH profiles are shown in this figure but are not addressed in this manuscript. I would suggest to either address them (when relevant) or remove them completely from the figure.

Interactive comment on Biogeosciences Discuss., 10, 8065, 2013.

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