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Discussions

Interactive comment on "Nitrogen turnover in a tidal flat sediment: assimilation and dissimilation by bacteria and benthic microalgae" *by* K. Dähnke et al.

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Response to comment by anonymous referee #2

This reviewer disapproved the sediment slurry method we used, and stated that sediment mixing will alter process rates and would thus allow few conclusions on the relative importance of assimilation versus dissimilation.

Clearly, as this was also the main criticism of reviewer #1, we did not sufficiently emphasize that we were a) well aware of this phenomenon and the resulting limitations and that b) the impact of disturbance on process rates was something we deliberately wanted to include in the experiment. For a detailed explanation on why we chose this

setup and regard the use of slurries as justified in our experiment, we would like to point to our responses on this topic to reviewer #1.

In brief, we point to the fact that slurries are a common tool to investigate potential rates (e.g. Thamdrup and Dalsgaard, 2002; Risgaard-Petersen et al., 2005; Veuger et al., 2005; Risgaard-Petersen et al., 2004), and explain that mixing due to bioirrigation, external forces like e.g. trawling and especially due to tidal forcing can occur frequently in this environment, so the conditions imposed on our slurry are not entirely unrealistic to this natural system, as this will alter rates as well.

We agree that, as the reviewer suggests, the use of intact cores is a worthwhile next step in the investigation of N-cycling. However, for a first study on rates we regard the use of slurries as justified (for the reasons briefly outlined above and in response to Reviewer 1). Furthermore, the biphasic behavior of turnover rates that we find after the initial disturbance shows two things: First, all process rates are driven to a maximum due to disturbance, and second, after mixing sediments return to a stable state within less than 24 hours, with process rates reaching a plateau.

Another criticism raised by the reviewer was that slurry preparation would inhibit denitrification rates. We do not agree with this assumption: Especially for investigation of denitrification rates, slurries are a very common tool to investigate potential rates of both anammox and denitrification (e.g.. Trimmer et al., 2003; Dalsgaard and Thamdrup, 2002 and references therein) – and also in the initial phase of our experiment, there is no indication of inhibition due to slurry preparation. Denitrification rates are in fact highest right afterdisruption of biogeochemical gradients, and level off to steady state after 3-6 hours, when the availability of substrate and easily degradable organic matter ceases. An inhibition due to introduction of oxygen cannot be expected in sediments as reactive as ours. Oxygen consumption is high (almost 40 mmol m-2 d-1, unpublished results), and denitrification accordingly starts off rapidly within the slurries, it seems to be affected mainly by the supply of nitrate and organic carbon.

Overall, to meet the reviewers criticism on the experimental setup, we will, in a revised version of the manuscript, stress up front that the disturbance of biogeochemical gradients occurs in natural systems as well, and that the intention of our study was not to quantify realistic in situ rates, but that we were aiming for a first relative assessment of turnover pathways – something slurry incubations are frequentkly used for, also when evaluating the impact of benthic microalgae on N-cycling.

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