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Biogeosciences 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 #1

The reviewer was very critical regarding the general approach, and came to the conclusion that our experiment did not result in any insights into nitrogen cycling. We see that some shortcomings in focusing the manuscript may have led the reviewer to his/her conclusion, but do not regard this criticism as justified, and would below like to outline why.

The major criticism was that the experimental design was flawed and did not allow any conclusions on natural rates. We did indeed perform a slurry experiment, and were fully aware of the fact that biogeochemical gradients will be disrupted, and light conditions will change, as we clearly state in the manuscript at various locations. We did consciously choose this setup instead of whole-core incubations for two very specific reasons:

(1) Our study is a first approach to investigate nitrogen incorporation into ammonium, nitrate, organic matter, sediment bound ammonium, and, via modeling, canonical denitrification, in both light and dark in this tidal sediment. The use of sediment core incubations will doubtlessly provide valuably insight into in situ rates, but to get a first impression of turnover and uptake within sediments, slurries are widely used. In mechanistic studies that aim at comparing different assimilative and/or dissimilative processes among each other, the use of slurries is in fact a widely used standard method, which has been applied to investigate both canonical denitrification and concurrent algal uptake in sediments (e.g. Thamdrup and Dalsgaard, 2002; Risgaard-Petersen et al., 2005; Veuger et al., 2005; Risgaard-Petersen et al., 2004).

(2) The strength of whole-core incubations also represents the one serious draw-back they suffer from: cores and sediment surface remain undisturbed and are not exposed to any kind of turbulence. In some environments, like the tidal flat we investigated, turbulent mixing can, however, not be ignored: The site is exposed to strong tidal forcing, and turbulences and the resulting mixing of the sediment surface layer (including burial of surface-colonizing diatoms in deeper sediment layers, cf. Kamp et al. (2011) will occur frequently due to currents, waves and also as a side-effect of intense trawling and fishery in coastal regions (Trimmer et al., 2005). We aimed at investigating the impact of these effects on nitrogen cycling as well, but admit that this is apparently not sufficiently clear in the current version of the manuscript. We do mention the implications for natural systems later on in the discussion, but agree that the importance of turbulent mixing in this environment should have been emphasized up front, which we will do in a revised version.

Regardless of this unfortunate omission, which clearly misled the reviewer, we would





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like to point out again that in process-oriented studies that aim to compare different turnover pathways among each other, the creation of slurries is a standard technique. The comparison of slurry-derived rates with whole sediment cores may be a further step in analyzing nitrogen cycling, but our experiment serves as an approximation of these rates relative to each other nevertheless.

Thus, our approach was not to infer natural rates in this tidal sediment; we were interested in the equilibrium between different turnover processes. Due to the slurry preparation and the disruption of gradients, we created equal starting conditions and were able to trace the magnitude and relative importance of turnover and uptake from there – after disturbance due to physical forcing, as it can occur in natural systems, especially coastal ones, as well.

Later on, the reviewer states that "The major insight from the experiment was that 15NO3 was being preferentially assimilated over 14NO3-". This is a misunderstanding, as it would indeed violate the general assumptions on stable isotope chemistry. We did at no point in the manuscript aim at suggesting that preferential 15N assimilation took place. Our point was that diffusion-driven processes follow different rules: If diffusion is the driving process, diffusion (including ion-exchange between pore-water, sediment and biota) will preferentially act on heavy isotopes upon addition of a labeled substrate until isotopic equilibrium is achieved. Diffusion-driven uptake of nitrate by diatoms has been suggested by different studies, which we cite in the manuscript, and this is precisely why we assume this might play a role in sequestering 15N. We did not mean to suggest assimilation was the driving process. In a revised version, we will stress this difference more clearly in the discussion of diatom nitrate removal in a revised version of the manuscript.

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