

Interactive comment on “The fate of fixed nitrogen in oligotrophic marine sediments: an in situ study” by Stefano Bonaglia et al.

Stefano Bonaglia et al.

stefano.bonaglia@gmail.com

Received and published: 12 December 2016

Here we present our answers (marked AC) below the original referees' comments (RC).

Anonymous Referee #2

RC - The authors present a high-quality dataset on nitrogen cycling in coastal sediments with a low carbon loading. The manuscript is generally well written and based on a high-quality dataset comprising in situ flux measurements, incubations experiments to partitioning nitrogen flows and some basic background data (ladderane lipids as biomarker for Anammox, burial of nitrogen using ^{210}Pb excess, etc).. The conclusions are largely confirming our existing view of nitrogen biogeochemistry in low carbon coastal sediments and such present a useful addition to the literature. I suggest the authors to articulate their DON flux findings a little more.

C1

AC – We appreciate the reviewer's acknowledgement of the merits of our work, and we thank them for their insightful and useful comments. However, we do not fully agree that our conclusions are largely confirming existing data, as our results contrast with previous studies suggesting that DNRA was negligible in cold and well-oxygenated sediments with low organic carbon loads. We also believe that the results concerning the DON data are intriguing and novel. We will put more emphasis on the high contribution of the DON flux to the total efflux of fixed nitrogen. We will also discuss some implications of this aspect. High export of DON to the water column may be a reason for the high activity of bacterioplankton and the dominance of heterotrophy vs. autotrophy found in the waters of the Gulf of Bothnia (Algesten et al. 2004 - Global Biogeochem. Cycles).

RC - Although the writing is generally clear, some fine tuning and precision of wording would improve this very good manuscript further. - insert hyphens for multi-word adjectives: e.g. bottom-water salinity. - one the one and on the other hand always come together - sometimes the logic of sentences needs improvement, e.g. p3, l. 9-10: pore-water chemistry is the result of N cycling processes; anammox biomarker reflect cycling processes but do not control it, etc.etc. Another example: p. 12, l. 25: our rates therefore represent in situ conditions. Rate reported are representative for the in situ rates. Rates do not represent conditions.

AC – We appreciate these corrections and we will reword the text accordingly.

RC - Oligotrophic marine sediments: is that the right term? Water column ecosystems are considered eutrophic or oligotrophic, but sediments are usually classified as low or high carbon loading systems. Nutrient concentrations are quite high in sediment, including the ones reported here. Moreover, can you use the term oligotrophic for sediments with an oxygen penetration depth of less than 2 cm? Not convincing. > 75% of the seafloor has larger OPD.

AC – We agree with the reviewer that “oligotrophic” is not the most suitable adjective

C2

to describe marine sediments, although it is commonly used in literature. We suggest the following alternative title: "The fate of fixed nitrogen in marine sediments with low carbon loads: an in situ study".

RC - The authors emphasize somewhat the peculiarities of low temperature conditions, e.g. p. 2, l. 19, but are all deep-sea systems not cold. Consequently there are quite some studies on DNRA in cold systems along ocean margins. Rewrite the text. Moreover, why should temperature matter so much? A permanently cold system will function well, in the end supply of oxidants and reduced substances set the stage.

AC – Temperate coastal sediments, except for those of the high Arctic/Antarctic, have seasonal temperature variations that may affect biogeochemical processes. In other cold Baltic Sea sediments, for example, temperature was shown to significantly affect nitrogen cycling processes and the partitioning between denitrification and DNRA rates (Bonaglia et al. 2014 – Biogeochemistry). Moreover, DNRA bacteria isolated in Arctic fjord sediments had their highest optimal growth rate at 18 °C, while denitrifiers had their optima at 0 °C (Canion et al. 2013 - Environ. Microbiol.). Even in the permanently cold (< 10 °C) GOB sediments we have temperature fluctuations, distinguishing them from the Arctic and deep-sea sediments. To date, we are not aware of any single study reporting on significant DNRA activity in year-round cold sediments, either from coastal setups or the open sea. This is further corroborated by the study just published by McTigue et al. (2016 – Nature Comm.), which showed that denitrification was one to two orders of magnitude greater than DNRA in Alaskan Arctic shelf sediments. Thus, one of the main messages of our paper is that significant DNRA activity cannot be excluded a priori in cold, oligotrophic systems.

RC - The material and methods section is very detailed and sometime too much detailed knowledge is expected from the reader: all the abbreviations, etc. Perhaps a few lines on explaining the principle of the approaches would better guide the reader through the details.

C3

AC – We believe that it is preferable to describe Methods in details rather than omitting important steps of the operations in this type of scientific works with novel and complex experimental setups. However, in the revised manuscript, we will shorten the 210Pb and ladderane parts, which are already been described in details by others before. We will also introduce the main principles behind each of these methodologies.

RC - On page 8, it is mentioned that C and N were measured before and after HCL treatment. Two remarks: (1) this is the wrong reference because Verardo et al. used sulfurous acid rather than HCl and (2) communicate to the reader that you report only total nitrogen and organic carbon in this manuscript. You made the right choice of not using Norg because of acidification artifacts.

AC – The procedure by Verardo et al. was referenced because of the type of detector used (flash combustor by a Carlo Erba elemental analyzer). We will specify that we slightly modified the sample preparation method and that only the Corg and N data are presented in the paper.

RC - Burial rates are based on sediment burial rates inferred from 210Pb excess measurements. Although you touch upon the issue of bioturbation in the material and methods sections and conclude that you can ignore it, lateron you present visual faune observations suggesting otherwise. Communicate to the reader that burial rates may be inflated because of bioturbation, in particular at stations.. Even better show the 210Pbexcess profiles in the appendix/supplementary info.

AC – We exclude that in this type of sediments bioturbation may have biased burial rates. The macrofaunal organisms retrieved in the benthic chambers and in the sediment cores were almost exclusively specimens of *Monoporeia affinis*, a small amphipod that was found either swimming in the water column or colonizing the upper 3-4 cm of the sediment. The abundances of the deep burrower *Marenzelleria* spp. were negligible and their effect on the 210Pb distribution was therefore minimal. Moreover, macrofauna was completely absent at RA2 and at the GOB stations sediments were

C4

laminated below 5-6 cm depth, which clearly exclude particle mixing below that depth.

RC - Minor corrections: - p. 1, l. 12: on the global - p. 1, l. 13: most scientific investigations have increased the last few years because the scientific community has grown. Reformulate. - P. 1, l. 17: burial rates were not experimentally determined: they were inferred from ^{210}Pb excess observations - P. 1, l. 24: clarify here that you mean total dissolved fixed nitrogen. - P. 2, l. 26: southern and central Baltic Sea are among the : : : - P. 3, l. 2: but do not report anammox - P. 4, l. 30: control or output? - P. 8, l. 11: an dimensionless linear sorption coefficient - P.10, l. 19: depth-interval weighted average porosities? - P. 12, l. 15: give the most accurate.. - P. 13, l. 17-19: why this role of latitude: is this the cause? I guess that coastal-deep-sea gradient is more important than latitudinal.

AC – We will be glad to consider these specific comments in the revised manuscript.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-344, 2016.