Biogeosciences Discuss.,
doi:10.5194/bg-2016-344-RC1, 2016

Biogeosciences Discussions

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

Anonymous Referee #1

Received and published: 12 October 2016

General Comments The authors present a study in which they quantified the fate of fixed nitrogen in sediments of a cold, oligotrophic system. The authors used 15N tracers and a combination of in situ incubations using a benthic lander and ex situ sediment core and slurry incubations. The authors are the first to simultaneously measure rates of denitrification, anammox, and DNRA in oligotrophic sediments. They accomplish this using in situ lander incubations, which are logistically difficult to perform, but may actually provide more accurate estimates of in situ rates than traditional core or slurry incubations. The authors found that denitrification dominated N2 production, but anammox bacteria were also active, accounting for 18-26% of N2 production. The authors also measured detectable DNRA and found that DNRA rates were highest, and comparable to denitrification rates, at the shallow coastal station. A sediment nitrogen budget was constructed and indicated that, despite the N2 production measured at the

stations, the primary fate of sediment organic nitrogen in the summer is recycling and efflux as TDN back into the overlying water. Lastly, this study compared concentrations of ladderane lipids, a biomarker for anammox bacteria, to anammox rates and found no correlation between the two. These datasets are sparse in the literature, so this is an informative contribution to the scientific community studying anammox.

Overall, I think the authors addressed important questions related to sediment nitrogen cycling that will be of interest to many readers of this journal. The paper is very well written and organized clearly. I am comfortable with the conclusions and support publication of this manuscript with minor edits, as detailed below.

Specific Comments p.1, line 12 insert "the" before "global"

p.2, line 5 delete "to" before "∼45%"

p.2, line 8 define the abbreviation "DNRA" the first time it's used in the text body

p.2, line 13 insert "the" before "electron"

p.2, lines 23-24 It would be helpful if you mention briefly the link between Mn and anammox, since it is related to your hypotheses and your interpretation of your results.

p.2, line 28 define the abbreviation "GOB" the first time it's used

p.3, line 1 I suggest replacing "happen" with "occur"

p. 3, line 7 suggested change: ". . ..we hypothesize that we will measure low benthic N cycling rates. . ."

p.3, line 9 change to "porewater," (one word) to be consistent with the rest of the text

p.4, lines 27-28 It would be helpful here if you could define what the average (or range of) water height(s) above the sediment surface was for the lander incubations. No need to list it for every incubation, just give the reader an idea of how much water volume was involved in these incubations.

p.6, line 28 Is the 75uM concentration for the sum of 15NH4+ + 14NO3- or for each of the N species?

p.7, lines 18-20 For clarity, I suggest you present the r-IPT equations from Risgaard-Petersen et al. (2003) so that readers who are unfamiliar with them can understand how you get from p29N2 and p30N2 and ra to p14. This will also give you a chance to define p14 explicitly, and describe how it represents N2 produced without the 15N addition, i.e., actual N2 production. Many unfamiliar with IPT think that the added 15NO3- will stimulate denitrification and that those rates are included in your results, when in actuality the IPT approach allows one to separate p14 (actual) from total N2 production from 15N and 14N (potential).

Eqn. 2 Somewhere here in the text describing eqn. 2 you should state clearly that p14sl includes both water and sediment p14.

p. 7, lines 25-26 I understand why you have to use the same Fwc measured in 2014 for the 2013 calculationsâĂŤyou don't have the sediment core incubations from 2013. I'm just not convinced that the Fwc values would be consistent from 2013 to 2014. Your rates (denitrification, anammox, O2, TDN, etc.) as well as OPD show year-to-year variability, so it would not be surprising to me if the Fwc values were variable. Perhaps here (or elsewhere) you could defend this assumption in a bit more detail and discuss the potential implications for your calculated rates?

p.8, line 1 Since you use the term "ra" here, and it's a widely used term to describe the contribution of anammox to total N2 production, I suggest you use it throughout the rest of the text and tables/figures.

p.8, lines 15-16 I have read this section multiple times, and I still am unsure what this sentence means. I think you're saying that you have to use the Fwc calculated from the p14 values for this NH4+ calculation. If p15NH4+ was not detected in just one of the incubations (GOB1-3), why couldn't you use the p15NH4+ fluxes from all of the other incubations? At least they're still related to the parameter you're working with (NH4+).

How will using the Fwc derived from the p14 values affect the calculated NH4+ rates?

p. 10, line 13 Insert "from" after "ranged"

p. 10, line 24 The sentence "Between the four stations. . ..>GOB3." reads awkwardly. I suggest changing to "Downcore NH4+ concentrations were greatest in RA2, followed by GOB2 >= GOB1>GOB3."

p.11, line 5 Replace "GOB3" with "GOB2"

p. 11, lines 13-15 The sentence "The facts that . . .supported by DON." is awkwardly worded, making it difficult to understand its meaning.

p.11, lines 17-18 reword to ". . .with Fwc values of 0.26, 0.23. . ."

p.11, line 22 At the end of this paragraph, I suggest you present the ra values from the slurry incubations (also include in Table 2), since that's really the main point of doing the slurries. It's fine to keep the data in Figure 6 since it's relevant to the discussion of the other NRPs. But I think the data should be first introduced here to make it clear where that data come from.

p.12, lines 19-22 The sentence "The 15N isotope pairing technique. . .is low.." is awkwardly worded, making it difficult to follow.

p. 13, line 7 Delete "for" before "potential"

p. 13, line 18 Replace "Alike" with "Like"

p.13, line 32 Why did you pool all of the data from 0-4cm for the ladderane concentrations to compare to the rate values? The values are highly variable from surface to 4cm. Did you try just using data from anoxic sediments, where anammox may have been occurring? Or depths where NO2- and NH4+ were present? I wonder if you would have seen a better correlation between the ladderane concentrations and the rates. It would be helpful to include some discussion of this.

p. 14, lines 17-18 You mention here that H2S was never detected in the sediment porewater, but you do not present that data anywhere. I suggest you mention it briefly in the results section since you took the time to describe the microsensor method.

p. 15, line 5 Replace “process” with “proceeds”

p. 15, line 7 Replace “being” with “at”

p. 15, line 13 Delete “eventually”

p. 15, line 16 Replace “upscale” with “scale up”

p. 15, lines 15-17 I’m unsure what conservative method you are referring to. It would be helpful to explain briefly here since it’s important enough to bring up in your discussion.

p. 16, line 3 Reword to “The removal rate and the recycling rate were constrained by. . .”

p. 16, line 10 Replace “prove” with “suggest”

p. 16, line 14 Replace “basin-wise” with “basin-wide”

p. 16, lines 17-19 You briefly mention the contribution of DNRA to the TDN flux here, but I think it would be helpful to present the data in Fig. 7 so that the reader can get a feel of interstataion variability.

Figure 2 Make sure to note which symbols are N vs. C (black vs. white).

Figure 6 (c) The y-axis labeled “AAO contribution” should be changed to “ra”, as discussed above. Also, the caption for panel (a) should replace “Shaded” with “Hatched” so as not to be confused with the gray shaded bars (2014).

Figure 7 In the caption, replace “nitrogen cycling” with “TDN efflux” since that’s more accurate.

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