

General comments:

The article by Glock et al. titled "The role of benthic foraminifera in the benthic nitrogen cycle of the Peruvian oxygen minimum zone" brings awareness to the understudied and relatively recently discovered aspects of denitrification, namely examining the role of benthic foraminifera in the nitrogen cycle. Although the topic of the study is highly interesting and relevant to BG journal I feel that the actual study presented here was not executed in very accurate manner and data is relatively speculative. Part of this may be due to sampling scheme (may be this study was not the focus of original cruise) and this study/manuscript has been put together mainly using exiting published data without real additional measurements to support the foraminiferal denitrification rates and/or nitrate pool.

Below some more detailed comments why I question the scientific impact of this manuscript:**Methods:**

The study presents data on foraminifera, in situ fluxes and pore water profiles. To me it is currently unclear if the data for these different components actually came from exactly the same sample sites, or close by sites? I.e. do you have a flux measurement, pore water profile and foraminifera counts for each site? Or is some of your environmental data from near by sites? In your Table 1 you only list the sample locations for your foraminifera but not for the other parameters. Also if I look at the appendix with the pore water data, all of the station names do not seem to match with the foraminiferal station names. The same is for the in situ flux data.

Is all the insitu flux data taken from the paper of Bohlen et al. 2011? Or are the data new, which are reported here? In your results section p. 17785 line 13 you refer to some model calculations. These calculations are not explained anywhere in the manuscript. Are these the modelled data of Bohlen et al?

Pore water pressing: I would think that a large proportion of the cell bound nitrate is actually due to presence of *Beggiatoa* and *Thioploca* sulfur bacteria as outlined in Bohlen et al. I think it will be very difficult to try to separate the cell bound nitrate content of forams vs sulfur bacteria based on this method.

Foraminiferal nitrate pool calculations. I think (unfortunately) that it is not possible to calculate such an average nitrate pool as presented here. From previous studies we know that the nitrate pool of foraminifera is highly variable (e.g. Pina-Ochoa et al. 2010 MEPS and Koho et al. 2011 FEMS) and taking an average value and multiplying this by number of living population is thus not correct. The values reported for average nitrate pool/per species of foraminifera in Pina-Ochoa et al. 2010 PNAS are also often based on very few individuals so the averages are probably not completely representative. Furthermore, the standard error reported in Pina-Ochoa et al. 2010 (PNAS) also illustrates this high variability in the intracellular nitrate content. More actual measurements are needed on the size of the foraminiferal nitrate pools to better estimate this, including various species.

I think that rose Bengal staining is a valid method for identifying the numbers of living foraminifera in ecological studies. But as authors must be aware a care should be taken when working with specimens from low oxygen sites. I think authors should at least

acknowledge this potential over estimation in the size of the living population. The overestimation in the size of the living population would also lead to overestimation in the foraminiferal denitrification rates and nitrate storage.

The approximation A and B used in the study sound reasonable but they should be reported more clearly. A supplementary appendix should be added to the manuscript where denitrification rates for each taxa are shown and explained where the value came from.

I can appreciate that in the OMZ sites where the bottom water oxygenation is very low $<2\mu\text{M}$ foraminifera rely on denitrification. However, it becomes very difficult to estimate how much they contribute towards denitrification when oxygen content increases even a little bit. We do not know at which oxygen concentration forams switch to denitrification. Perhaps they continue to respire on oxygen even when the amount is very little, for example couple of micromolar or even less?

Data in table 4 and section 3.2 is very confusing and somewhere must be a mistake. You report 3 columns of data for foraminiferal denitrification rates, and if I add up approx A and B together I get the values you report in the text in the section 3.2 but these are not the values reported in your table 4 as the total foraminiferal denitrification!

Section 4.2 First sentence. Its not true that foraminiferal denitrification has only been estimated in Sagami Bay. And you also contradict this sentence several times later in this section. For example, Pina-Ochoa et al. 2010 (PNAS) also estimated foraminiferal denitrification in Skagerrak, Bay of Biscay and Arabian Sea OMZ. Also Hogslund et al (2008) has estimated foraminiferal denitrification in the OMZ off the coast of Chile.

Conclusions section from lines 19 to end of paragraph. Nowhere before this have the nitrogen isotopes been discussed in the manuscript and no data is presented on this. How can you conclude about something you have no data on?

Some other smaller commenst/issues that I feel should be revised and/or addressed.

Abstract: In the first sentence you imply that foraminifera use nitrate as an energy source. Is this true? I thought that foraminifera are heterotrophic organisms. They can use nitrate/oxygen for their respiration but for as their primary energy source.

Abstract, line 3: elsewhere in article you also mention that diatoms are also able to denitrify? I also though some flagellates are also known to use nitrate, although they do not reduce all the way to N_2 . If you mention one example here you should mention them all?

I would constantly refer to your stations with water depth. Rather than sometimes saying the shallowest, deepest etc. Use names consistently and it is much easier for a reader follow the text.

I would add bottom water oxygen content in Table 1. Also if your environmental data is not from these sites they should also be listed. And the implication of this should be explained and whether you can then actually compare the data?

p.17781 line 15. Reference to Murray 2001? This sentence should be modified or reference changes. I doubt Murray discusses nitrate utilisation in his article from 2001?

I feel that the reference to “approximations A and B” in the results and discussion is a chaotic. I feel that if this is well explained in the methods, it may not be necessary to confuse the reader with these assumptions throughout the text. Alternatively, a study limitations paragraph where this limitation is explained could be added and it would not need to be discussed more than that. I also think that “assumption” would be a better word than “approximation” to describe these study limitations.

p. 17786 lines 22-24. I do not understand the link with the rest of the paragraph

p. 17789 lines 17-26. I don't know how relevant the discussion of Globobulimina is here as it is not present in the study region.

p. 17789 lines 27-29. As this sentence reads now it implies to me that Bohlen et al modelled foraminiferal denitrification rates. I think this is not true so sentence should be modified.

p. 17790 line 6-7. I would think that denitrifying bacteria are the dominant denitrifiers at these sites!