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Biogeosciences Discussions

Interactive comment on "Heterotrophic denitrification vs. autotrophic anammox – quantifying collateral effects on the oceanic carbon cycle" *by* W. Koeve and P. Kähler

Anonymous Referee #3

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The paper provides a relevant theoretical analysis concerning the effect of the coexistence of heterotrophic denitrification and autotrophic anammox on the carbon cycle and the CO2 release in the oxygen minimum zones (OMZs). The paper has been motivated by Voss and Montoya (2009), arguing that the nitrogen loss led by denitrification and/or anammox should induce different collateral effects: anammox should especially induce an increase of the efficiency of the biological pump by reducing the net production of CO2. Contrary to expectations, the authors demonstrate that the combined effects of these processes are always clearly heterotrophic in the suboxic layer of the OMZ, even considering a higher contribution of anammox to the total N2 production. In general, the paper is very interesting, well structured and clear, and the arguments

are supporting the interpretations and conclusions. I will ask the authors to be more cautious stating the assumptions at the beginning of the paper, and not only during the Discussion section. In particular, the analysis is relevant mainly in the suboxic layer, and without considering the autotrophic photosynthetic activity in the OMZ layer (e.g. in situ organic matter production associated with the secondary peak of fluorescence and with Prochlorococcus and Synechococcus: e.g. Liu et al., 1998), and of course in the surface layer. The analysis is based on no accumulation of NH4+ but in accumulation of NO2-, which makes sense in general for the OMZs, but configurations with accumulation of NH4+ and/or no accumulation of NO2- could also occur in the OMZs. In addition, intermediate chemical forms of the nitrogen cycle, other than NO2- (e.g. hydrazine) could play a non-negligible role in the coupling of DNRN and anammox, for instance. What would be the consequences in terms of degree of heterotrophy and ΔCO2:ΔN2 ratio? Also, because the paper deals with a theoretical study, comments or comparisons with "real" observations will be appreciated.

Abstract: - "Here, we ...in marine oxygen minimum zones (OMZ) ....": mention that the study are focusing on the OMZ core (and not the oxycline where, for instance, nitrification is a very important process coupled to denitrification, known as nitrifier-denitrification). - Be more explicit with the ΔCO2:ΔN2 ratio: e.g. CO2 release versus N2 produced (here, the authors are not considering nitrogen fixation). Also with the term "nitrogen conversion": e.g. nitrite accumulated versus nitrate consumed.

1. Introduction: - A general important comment. There is two different and distinguishable effects of the nitrogen loss on CO2: 1) a direct effect corresponding to the topic of the paper, i.e. the autotrophic versus heterotrophic consuming and producing CO2; 2) an indirect effect, through the nitrogen deficit, inducing less primary production (locally and/or at global scale), and then less CO2 sequestration and carbon export. Whether the second effect could be largely more significant than the first one, is also a key-question. In your introduction, specify how the "Temporal changes of the nitrogen removal flux, .. are thought to influence the level of oceanic production and associated

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CO2 fluxes", according to the authors mentioned (Altabet et al., 1995; Ganeshram et al., 1995: Codispoti, 1995). I remembered that these authors are mentioning the second indirect effect, and not the first direct effect here analysed. In addition, related to the second effect, the predominance of denitrification, and DNRA over anammox could also have an indirect effect on the local surface primary production (PP), beneficial to a NH4-stimulated PP rather a NO3-stimulated PP. - Line 6, correct the typo "intoN2" into "into N2".

2.1. Background and definitions: - Denitrification and DNRA are not always heterotrophic. Also maybe here, or in the conclusion, you could mention that recent studies suggest that anammox bacteria could reduce itself nitrate into nitrite from organic acids (e.g. Den Campf et al., 2006), i.e. could be heterotrophic. In that case, DNRN will be still performed heterotrophically, but by anammox bacteria and with different stoichiometry. In addition, some denitrifying bacteria could have anammoxosomes (e.g. Hu et al., 2006) and could use ammonium and nitrite, i.e. a scenario similar to scenario II (DNRN+A+DNRA). This is not affecting the conclusion of the paper. - About the historical presentation of the anammox, you can add that Hamm and Thompson (1941) are the first to write the anammox chemical equation. - Lines 14-18, pages 1817: here, the hypothesis and its statement are very strong, even if this hypothesis is discussed latter in the Discussion section (Cf my general comments). For instance, Lam et al. (2009) estimate that 33% of the nitrite is produced by nitrification, and a large part of the ammonium by micro-aerobic respiration. This hypothesis is correct, if you specify that this analysis is focused on the suboxic OMZ core layer.

2.2. Stoichiometric constraints: - R1: why not a more simple equation with HNO3 and CO2, as in Table 1, and since you are not commenting any carbonate effect. - Lines 23-24, pages 1818: here, the hypothesis and its statement are again very strong, even if this hypothesis is discussed latter in the Discussion section (Cf my general comments). - Line 14-15, page 1819: instead of "indistinguishable", "not significantly different" is maybe more correct. - Line 25, page 1819: "inefficiency" seems more correct than

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"efficiency". - Line 29, page 1819: not directly clear on Fig. 2a. Clearer on Fig. 5. - Line 2, page 1820: after "... N2-production", add maybe Fig. 2a (or Fig. 5). - Line 3, page 1820: instead of Fig. 2b, would be clearer on a figure "N2 fraction from anammox versus N2 fraction from Norg". - Line14, page 1820: "assuming" is perhaps better than "using".

2.3. Alochthonous substrate sources: - Line 25, page 1822: kinetic grounds are also important. - Lines 28-29, page 1822: anammox and nitrification often are co-existing in the OMZs (e.g. Lam et al., 2008).

3. Discussion: - The discussion, focused in the aphotic zone, is very interesting. Maybe add at the end (Lines18-22, page 1825) "in the aphotic zone".

References: - Bange et al. (1996) and Silva et al. (2009) are not cited in the text.

Tables and Figures: - Table 1: why S in the bulk organic matter, without comment about S in the text? In addition, because the paper is focused on the effects on CO2, it will be better to include CO2 in equation (3) of the anammox, even if the effect is negligible and not visible on Figures 2a, 3b and 5. - Table 2: in the caption, add after "scenario I", "with DNRN, denitrification and anammox". For the 2nd column, why are you not using the notation "DNRN:den" instead of "den: DNRN"? For the 4th column, write "N2-anammox:Total N2-production" instead of " Anammox:N2-production". In footnote a, use the same notation than for X-axis of the figures (cf remark for the figures): I suggest "NO2- produced (DNRN) to mol NO2- CONSUMED (denitrification)" instead of "NO2- produced (DNRN) to mol NO2- USED (denitrification)". - For the figures, give information, if it is possible, about where the "real system" is. And also be more explicit with the axis title, using the same notations than in the text. E.g., use "NO2 accumulated / NO3 consumed", instead of "NO2 (accum) / NO3 (deficit), since NO3 deficit classically deals with a deficit involving directly the phosphate concentration. Here, I understood that it deals with the moles of NO3- consumed for 1 mole of phosphate released.

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