

## ***Interactive comment on “Stoichiometries of remineralisation and denitrification in global biogeochemical ocean models” by A. Paulmier et al.***

### **Anonymous Referee #1**

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Paulmier et al. show, based on the manipulation of stoichiometric equations representing different pathways of organic matter remineralization, that the amount of oxidant (namely oxygen or nitrate) required to oxidize a given amount of organic matter depends greatly on the amount of hydrogen present in the organic matter. Furthermore, they show that current implementations of a specific set of biogeochemical models do not take this effect into account adequately, which results in inter- and intra-model inconsistencies as to the assumed composition of the organic matter participating in the remineralization process.

That modelers have not explicitly stated hydrogen assumptions until now is unfortunate,

C2

but the realization that the hydrogen content of organic matter is an important variable modulating the oxygen demand in the sea is not a new finding. Hydrogen itself is rarely explicitly considered, as hydrogen measurements are quasi inexistent on bulk marine organic matter; Anderson (1995) and Hedges (2002) being notable exceptions. Instead, the effect of hydrogen is often partly captured in the O<sub>2</sub>:C ratio, which appears to be more tightly controlled than the C:H ratio for instance (see Laws 1991). As such, a discussion of the O<sub>2</sub>:C ratio as it pertains to the equations derived in this manuscript and some indications on how the O<sub>2</sub>:C ratio is treated in each model would be an interesting and valuable addition to this manuscript.

In a subsection of this paper (3.3 – Implications), Paulmier et al. also make interesting claims regarding the cycling of nitrogen in oxygen minimum zones (OMZ). Specifically, they write in section 3.3.2 "How much fixed N is removed during denitrification": "... if the newly fixed N is transformed into organic matter above suboxic areas, and upon sinking into the suboxic environment, is denitrified, an addition of new nitrogen at the ocean surface could generate an even greater loss of fixed nitrogen in the suboxic areas below". This statement deserves, in my opinion, further discussion. By the above statement, do Paulmier et al. imply that a sort of accelerating runaway effect exist whereby the ocean would lose its nitrate unless organic matter production above OMZ stopped (at least periodically) or N<sub>2</sub>-fixation rates increased? Due to the estuarine circulation cell that broadly characterizes the OMZ, waters upwelling near OMZ are relatively rich in phosphate (low N<sup>\*</sup>) making the environment suitable for N-fixation. As per Paulmier et al, however, this new nitrogen would further increase the N-loss below, and so further lower N<sup>\*</sup> and further encourage N-fixation above OMZ. If true, this mechanism would have important implications and this needs to be recognized. It seems unlikely that the runaway exist, so what could be the processes that limit this effect? Obviously nitrate supply to the OMZ would have to be considered and it implies that it is ultimately the nitrate supply into the OMZ that limits N<sub>2</sub>-fixation at the surface. Mass-balance suggests this should be the case.

C3

It seems that there also should be some correspondence between section 3.3.2 "how much fixed N is removed during denitrification" and 3.3.3 "How much N<sub>2</sub> is produced during denitrification". Yet, each section attempts to answer these questions with ratios (R(N:Norg) and R(N<sub>2</sub>:HNO<sub>3</sub>)) that seem unsuited to the questions posed in the subtitles. I think these 2 sections should be revisited.

Overall, I think this paper presents interesting arguments but also represent an unfulfilled potential by not fully discussing some of the most interesting points. I recommend that this paper be accepted pending major revisions.

Specific comments:

Section 1. Much of the paragraph between lines 9-24 of page 2542 describing the model could be split under the appropriate bullet points above that paragraph. Rather than emphasizing the origins of these models, this paragraph should emphasize the assumptions made regarding the assumed stoichiometry.

Section 2. If phosphorus is present as nucleic acid, phospholipids, etc. what is the rationale for using the form H<sub>3</sub>PO<sub>4</sub>? In section 2.1.2, NH<sub>3</sub> does not "dissociate" into NH<sub>4</sub><sup>+</sup>. The title for subsection 2.1.3 is awkward. Maybe use simply "Carbon, hydrogen and the oxygen demand for remineralization". It maybe relevant to discuss the results of Laws (1991) and Hedges (2002) in that section also. The subsections in section 2 are usually 1 sentence. The use of these very small subsections breaks the flow of the paper.

Sections 3 up to 3.2 is very didactic and generally clear. The reasoning for not accounting for the anammox reaction in section 3.2.2 is not clear to me, however. Why prefer equation 16 proposed by Richards (1965) when evidence for this reaction has not been found? Also, move the sentence "Note, that the C/N/P stoichiometries..." of section 3.1.3 after the second example. Change "oxix" to "oxic" in section 3.3.1, line 23 and clearly state the assumptions made (that is, a=106, b=175, c=42, d=16) to compute the example ratio R(N:Norg)=7. Also, correct the error "7 atoms" to "7 moles" right

C4

after equation 21.

Section 4 is also interesting, even if the interest seems to be largely limited to those people involved to the development of the specific models. Efforts should be made to maintain the fractional rather than the decimal notation in some of the equations, however; this makes it easier to follow for the reader.

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Interactive comment on Biogeosciences Discuss., 6, 2539, 2009.

C5