

Interactive comment on “Atmospheric deposition of nutrients and excess N formation in the North Atlantic” by L. M. Zamora et al.

Anonymous Referee #2

Received and published: 21 December 2009

Review for manuscript Atmospheric deposition of nutrients and excess N formation in the North Atlantic By Zamora, Landolfi, Oschlies, Hansell, Dietze, Dentener

The manuscript analyses the destiny and fate of nitrogen deposited to the tropical Atlantic Ocean by means of a modelling approach. Since nitrogen from anthropogenic sources is supposed to be generated at increasing rates and therefore possibly leading to considerable deposition in the open ocean, the effects on the oceanic biogeochemistry can potentially be large. The authors try to isolate several possible scenarios of particulate nitrogen production and degradation in the water column and find plausible explanation for the DINxs in the thermocline. Underlying their approach are three assumptions that shall be separated: a scenario with uptake of nutrients in the Redfield Ratio, the 2. approach assumes preferential remineralisation of phosphorous and

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the 3. approach considers the so-called non-Redfield nutrient uptake ratios. These approaches are generally meaningful and plausible to answer the question. There are, however, several major problems with the modelling, which may partly come from an incomplete description. For me as a non-modeller it is hardly possible to follow the description and evaluate the necessary details of the different models. Here some clarification in the text is necessary. – The authors use the coarse grid model (70years) and not the finer resolution (42 years) because they want to save computation time. This statement is not quite satisfying because the difference may have important implications for the model results – if that is not the case it needs to be better demonstrated. – It seems that a globally homogenous forcing was used even for the atmospheric deposition which is certainly heterogeneous. It is written on p 9855 l 20-24 that it is for conceptual simplicity. I have the feeling that it is over-simplification. – The homogeneity suppresses horizontal gradients which are a typical feature for atmospheric deposition and necessary for the specific questions of the manuscript. – Why is oxygen a modelled variable? It does not appear anywhere and seems not to be necessary. – No lateral advection is produced in the water column with this approach if I understood this correctly. But advection is essential for the whole modelling exercise and the particle distribution and degradation (see page 9857 l 26). Furthermore lateral advection results are later discussed for the modelled ocean box. – The authors assume uptake of all atmospheric excess N (page 9857 l 18). How does this work under a Redfield scenario – all excess N becomes DON? And the fact that everything is remineralized but only from 100 onwards. Why is that, when remineralisation already starts in the euphotic zone in the real world? To me it sounds as if part of the expected results must come out under these assumptions eg the result that preferential remineralisation results in a surplus of P. – The deposited nutrients are immediately distributed also into the remineralisation zone. I am not sure that the assumption is in accordance with a proper reproduction of the natural processes. – The fact that lateral transport is ignored for sinking particles is also critical since it contradicts the natural processes. That means that spatial inhomogeneous distributions due to advection are neglected.

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Under chapter 2.4 the problems of DOM exclusion are named. DOM is a major sink for atm. deposition and it is a much larger fraction than DIN or particles. How can such a major pool be neglected is still not clear to me. (page 9858 l 10-13) What is the mass balance technique page 9858 line 21 Export seems to be a problem in the model too (besides DOM) because the flux is not well constrained (see also page 9867 line 13-14) causing variations in the estimates by more than an order of magnitude. How critical is that for the overall model results?

The authors explain why they did not consider nitrogen fixation (page 9858 l 13-20). But it is well known how important nitrogen fixation is in the tropical Atlantic Ocean, especially under light of all the unicells and proteobacteria nitrogen fixation activities. To omit this major source of excess nitrogen is problematic. And nitrogen fixers – at least the colonial species like *Trichodesmium* - are known to release a major fraction of the fixed N as dissolved organic N which is automatically also not considered. Moreover it seems problematic to use the WOCE data set for comparison in which nitrogen from fixation is included.

Some other comments: It would be nice to give a short summary of the hydrography/currents of the modelled area. P 9853 l 26: I guess it is not possible to compare production rates because the production by nitrogen fixers is not considered. P 9854 l20: a distinction between a coarse and fine fraction is made here for P. But later the two fractions are not used. Coarse and fine is used in two different ways – for the model and for P-particles. It may be better (clearer) to use different wordings. Page 9866 l 19-25 where is the estimate of N fixation to sustain the observed nutrient pattern in steady state?

I like the conclusions which are very clear and interesting to read. Unfortunately all the questions in the model approach and the numerous assumptions make me question how reliable these conclusions really are. The manuscript would certainly benefit from a concise and clear description of the approach.

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Interactive comment on Biogeosciences Discuss., 6, 9849, 2009.

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