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Biogeosciences Discussions

Interactive comment on "Overlooked runaway feedback in the marine nitrogen cycle: the vicious cycle" *by* A. Landolfi et al.

Anonymous Referee #1

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Synopsis:

The authors highlight, using sensitivity studies with a global ocean model, the possibility of a positive feedback in the marine nitrogen cycle. If nitrogen fixation is stimulated directly above an oxygen minimum zone due to the upwelling of denitrified waters, the resulting local export expands the OMZ and increases upwelling of denitrified waters, enhancing local nitrogen fixation, and so on... If the nitrogen fixation, stimulated by excess phosphorus, is delayed and does not happen directly above the OMZ, then the positive feedback is significantly reduced. This is the case in the current observed ocean.

Comments:

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Interactive Discussion

Discussion Paper

I liked the central idea of this paper. It's an interesting concept and, to some degree, the sensitivity studies are a nice way to illustrate it. It's short and to the point. I feel that the presentation and some details of the execution could be improved; at present they are distracting and, for me, detract from the central message which is interesting and seems robust. I feel that the central message is interesting but the presentation and model illustrations need some work. My itemized comments and questions below outline these concerns:

1. On that note, in discussing the N2 fixation pattern of the (slightly confusingly named) "CONTROL" simulation (p. 8910, lines 5-6) the authors remark that it is in close agreement with the inferences of Deutsch (2007). This reads as if the authors believe this to be a good thing, whereas current evidence suggests that the inferred N2 fixation patterns of Deutsch et al (2007), based on the divergence of the tracer P*, are rather implausible. See for example the compiled observations of Breitbarth and LaRoche (2005) and the more recent compilation of Luo et al (2012 - the MAREDAT compilation). Mills and Arrigo (2010) explain how variable elemental ratios in the phytoplankton can lead to this difficulty of interpreting P* divergence.

2. The 150 year timescale of the model runs is rather too short for a manuscript which discusses the large-scale biogeochemical balance in the nitrogen cycle. There a strong transient in 3 of the 4 model runs but eventually P* must settle down on a global scale. How can we be sure this isnt just an initial transient because the initial conditions of the experiments dont match the system being integrated? In the case of the runaway feedback would we eventually see a global build up of P*? Why isnt the excess P used up somewhere else? I feel that at least one case should have been run for thousands of years to see what the long term development is? Its hard for me to clearly envisage what the final state would be and this model is certainly coarse enough to integrate for much longer timescales. If possible I feel that all cases should be integrated for at least a millenium. The Atlantic and Pacific basins are not communicating very much on this centennial timescale. I find the short integrations a significant concern as it muddies

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the waters considerably when talking about global balance/imbalance.

3. The choice of a low temperature cutoff for diazotroph growth may be convenient, and may not really matter for the key point of this paper, but I think it serves to continue to promote the idea that this is a real physical limitation. While Trichodesmium may not grow at lower temperatures, diazotrophy is certainly possible witnessed, for example, by the activity of other nitrogen fixers in the Baltic. See, for example, Staal et al (2003) for a discussion. So, I do feel that this temperature limitation is a bit of a hack which, though perhaps not critical to this message, promotes the use of an unrealistic parameterization.

4. Even in the "most realistic" case, the simulated nitrogen fixation distribution does not capture key features of the observed compilations (e.g. Luo et al 2012) where there is intense diazotrophic activity in the tropical and subtropical Atlantic. Is this to do with the short integration times and transients in the N cycle? Is it to do with the physical resolution?

5. I felt that the abstract and the progression of the text dont do the spirit of article justice. The abstract seemed a bit too "abstract" to tell what was coming. I think a more straightforward and concise description of the feedback mechanism and influence of decoupling would have let me know where the manuscript was going and I would have been able to appreciate more quickly the idealized experiments, rather than having the light come on half way through the paper.

Detailed comments:

p 8907, line 9 "light-lit layer" should be "sun-lit layer"

Interactive comment on Biogeosciences Discuss., 9, 8905, 2012.