

Interactive comment on “Modeling the impact of iron and phosphorus limitations on nitrogen fixation in the Atlantic Ocean” by V. J. Coles and R. R. Hood

K. Fennel (Referee)

katja.fennel@dal.ca

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General comments:

In this manuscript the authors Victoria Coles and Raleigh Hood extend their previous North Atlantic modeling work by presenting a coupled physical-biogeochemical model that explicitly includes phosphate and iron dynamics (in addition to nitrogen cycling). As previously, their main focus is on N_2 fixation in the North Atlantic, but the novel contribution here is the use of N^* to constrain their model-simulated N_2 fixation rate (which is now possible because phosphate is explicitly included). Gruber and Sarmiento's (1997) estimates of N_2 fixation are based on this tracer, but to the best of my knowl-

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edge this is the first modeling study where N^* is used to constrain N_2 fixation. I'm sure this approach will prove extremely powerful for improving biogeochemical models and feel that this paper is a very valuable contribution in this regard.

I do have one major concern, though: the authors neglect denitrification in their model and, except for a brief mention of denitrification on page 1413 (line 25), also neglect to discuss the potential implications for the distribution of N^* . The N^* pattern is to the first order the result of both N_2 fixation and denitrification processes. The way the model is set up currently, negative N^* can only be imported from the northern and southern boundaries, where nutrient concentrations are nudged toward climatology and, thus, negative anomalies in the climatology can propagate into the domain. If a large denitrification sink within the model domain is neglected, this would affect both the pattern and magnitude of N^* . In fact, in a recent modeling study (Fennel et al. 2006), a North Atlantic continental shelf denitrification flux with a magnitude rivaling or exceeding N_2 fixation estimates was derived. This large denitrification flux is consistent with observational-based estimates (Seitzinger and Giblin 1996) and is also apparent in the distribution of N^* . N^* values of -10 to -20 are abundant on the Mid-Atlantic Bight shelf. A hint of this can be seen in Fig. 11b, off the Canadian and U.S. east coasts, where N^* values decrease dramatically from positive values around 3.5 in the subtropical gyre to negative 1. In order to match the observed N^* pattern in the North Atlantic, the authors had to assume strong differential remineralization of detrital nitrogen versus phosphorus (i.e. phosphorus is remineralized twice as fast as nitrogen). Such differential remineralization does produce a strong vertical N^* signal, as can be seen in Fig. 14, but is to the best of my knowledge not supported by observations (e.g. Christian et al 1997). It is likely that this strong differential remineralization is simply needed to compensate for the missing denitrification signature. It is important to acknowledge, however, that including shelf denitrification into a basin-wide model with relatively coarse resolution (at least when compared to shelf scales) is non-trivial and probably only possible in the form of a parameterization (e.g. similar to Yool and Fasham's 2001 approach to parameterizing continental shelf carbon fluxes in a global model). In summary, I don't

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consider the omission of denitrification a fatal flaw, but I think it is necessary to discuss the potential implications of that omission upfront in this paper.

Specific comments:

Intr., general comment: It would be useful to state that the model in this study differs from that used in previous studies by the same authors in that P and Fe are now explicitly included.

Intr., P. 1393, line 5-8: The citation of Broecker's idea about the lack of biological carbon export in a steady-state ocean is not in the right context here. Biological export also affects the partitioning of carbon between the ocean and atmosphere in a steady state ocean, even if processes occur in perfect Redfield proportions.

Intr., P. 1394, line 27-28: The comment that Fennel et al.'s (2001) study did not provide a mechanism for enhancing P supply to the surface is unclear. If the authors are thinking of active vertical migration by *Trichodesmium* colonies, this is not included in any of the other cited models (Moore et al. Hood et al. etc.) or this study either. If they are thinking of differential remineralization, it would be useful to say that explicitly.

P. 1398, line 11: Dust probably should not be categorized as a form of detritus. Detritus is of organic, dust of mineral origin. I'm not suggesting the choice in the model is bad, just that the formulation is awkward.

P. 1399, line 9-11: The mechanism for incorporating river run-off should be explained better.

P. 1399, line 24: Mention of shelf denitrification should be included here.

P. 1400, line 2: I would be curious to see how long it takes for the subsurface N* signal to deviate from the initial conditions. Since NO_3 and PO_4 are both initialized based on climatological data, the model will initially match the observed N* pattern almost perfectly. 60 years is probably sufficient, but it might be a good idea to include a figure to demonstrate how quickly the patterns drift apart and how quickly the model

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achieved steady state in terms of N^* distribution. One could plot, for example, the RMS difference.

P. 1400, equation: It wouldn't have been necessary to include the arbitrary factor and offset from Gruber and Sarmiento's original paper. It doesn't hurt, but makes this look a little clumsy.

Section 3, general comment: It would be a good idea to include some more quantitative comparisons between model-simulated fields and data (e.g. RMS, correlation coefficient etc.).

P. 1403, lines 19-25: Explain more clearly how the NOLIM simulations were carried out.

P. 1405, line 25: typo: Najjar (r missing)

P. 1406, line 17-18: How exactly was the "degree of limitation" calculated?

P. 1408, line 21-22: This would be easier to see if a difference plot of the two fields was included also.

P. 1410, line 11-20: The authors discuss what affects the N^* pattern (N_2 fixation and remineralization length scales) but neglect to mention denitrification. Denitrification is of first order importance in determining the N^* pattern.

P. 1410, line 28: Again, a difference plot of the two fields would make it easier to judge where they agree, how well, and where they deviate.

P. 1411, line 7: Note the pronounced decline of N^* toward the U.S east coast. It's most likely due to shelf denitrification.

P. 1412, line 11-28: The point of isopycnal maps not being the ideal approach to comparing the fields could be made by comparing z-level maps.

P. 1443, line 15: "to" missing

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P. 1443, line 22: Say explicitly how deep the N* maxima are located (i.e. 600-800m versus 200-600m in the data).

P. 1414, line 3: "initiating from the south" Does that mean from the southern boundary?

P. 1416, line 1-12: Again, this would only be true if there was no denitrification (which I don't think is a good assumption). I would recommend revising this paragraph.

P. 1421, line 22: Note Fennel et al.'s (2006) estimate for North Atlantic shelf denitrification of 2.3×10^{12} mol N yr⁻¹.

P. 1437: There appears to be a mismatch between the figure and the figure caption.

P. 1446: Typo in caption: Bergquist (not Berquist)

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