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Biogeosciences Discussions

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

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We thank the anonymous reviewer for a careful and thorough review. We have made most of the suggested changes. Here we detail these changes and respond to the various questions and comments.

Specific comments:

1. The reviewer requests information on the performance of the physical model (mixed layer depth or temperature profiles). This study builds on previous studies (Coles et al 2004, Hood et al 2004) which explicitly include mixed layer depth comparisons between the model and observations, as well as surface layer velocity fields. We have not emphasized the physical model results in this study in an effort to keep the manuscript

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length reasonable, i.e., we discuss only how the model fails to capture the sharp salinity front located along the North East Equatorial Countercurrent where the Atlantic Intertropical Convergence Zone is located, and also problems with Gulf Stream separation. These both impact comparisons between the observed and modeled biogeochemical fields and they are both related to low horizontal resolution. Following the reviewer's suggestion, in the revised manuscript we have included two new vertical sections (Figures 9d and 9e) that show a comparison of observed and modeled temperature. These help to explain the error in the phosphate vertical section, and also demonstrate the reasonableness of the model thermocline.

2. N and P inputs from dust deposition are not accounted for in our model. We now state this explicitly in the last paragraph of section 2.1. Deposition of P via dust is ˜0.2 Tg P yr-1 in the North Atlantic Ocean (Prospero et al. 1996). However, this input is small relative to the P flux from the deep ocean. Thus we assume that P inputs from dust do not have a significant impact on N*. Moreover, this simplification of accounting for only the Fe deposition from dust is justified on the grounds that dust contains about 30 times as much Fe as P (3.5% versus 0.11%) whereas phytoplankton and diazotrophs require ˜ 30 times less Fe than P (Mills et al. 2004). In addition, ambient Fe concentrations in upwelling seawater are typically about 1000 times lower than the P concentrations (i.e., ˜ 1-2 nM Fe vs. 1-2 uM P). Thus, we assume that Fe inputs from dust are much more likely to stimulate phytoplankton and diazotroph growth and are more likely to be significant compared to P inputs from dust. These arguments also apply to N inputs, which are even lower in dust compared to Fe and deepwater N sources (Prospero et al. 1996).

3. We have added more discussion regarding denitrification to the paper following the concerns of the other reviewer (see also our response to that review).

4. It is true that the model does not include temperature dependence. It is not included for the simple reason that it does not appear to be necessary to do so in order to reproduce the observed patterns in primary production and nitrogen fixation. In a

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much earlier version of the model (leading up to the Hood et al. 2001 publication) an Eppley temperature control curve was incorporated into the phytoplankton equation in the model, but this did not significantly improve the performance of the model at BATS, i.e., comparisons of observed versus modeled primary production. As we have pointed out in previous publications (Hood et al., 2004), it also does not appear to be necessary to include temperature control of the Trichodesmium growth rate to reproduce the observed patterns of biomass variability. This is in spite of the fact that many previous studies have asserted that the low latitude distribution of Trichodesmium is determined by temperature. Instead, in our model, this distribution is determined by high latitude deep winter mixing, i.e., Trichodesmium cannot grow at high latitudes because the light levels are too low in the winter to allow persistence. Since mixed layer depth and temperature are correlated, it is difficult to unravel their codependence. So, following Occam's razor, we have opted to not include temperature control.

5. The half saturation constant for dissolved inorganic nitrogen (DIN) uptake was indeed too high in this simulation. For the revised manuscript we reran the model with a much smaller value (changing it from 0.5 to 0.1mmol m-3). This did not substantially change the nutrient limitation pattern. However, we did need to change the phytoplankton growth rate slightly to compensate. The differences between the new simulation with more realistic half saturation constants, and the old version can be seen in comparing the NSTAR figures in the manuscript with the previous version. The parameter values have been updated in Appendix B. DIN vertical sections were presented in our previous study, so we do not include them here. The addition of P and Fe limitations did not dramatically alter the DIN vertical distributions from our previous simulations. However, as expected, lowering the half saturation values for DIN uptake did lower the surface DIN concentrations.

5. (b) Increasing iron deposition rate will increase the nitrogen fixation rate. However, it will also tend to increase iron concentrations in the upper ocean to levels that are inconsistent with observations. We have not emphasized iron as a limiting nutrient

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for nitrogen fixation for two reasons. First, even though phosphorus and iron co-limit over most of the domain, the Fe limitation is relatively modest, inhibiting the maximim growth rate by 20-40%. In contrast, in the regions where phosphorus alone limits nitrogen fixation, this limitation is severe, reducing the growth rate by 60-100%. The other reason that we focused on phosphorus is practical, i.e., when phoshorus is included in our model some mechanism for maintaining phosphorus in the upper ocean has to be included or nitrogen fixation cannot be brought up to reasonable levels (see also Moore et al, 2004). In order to solve this problem we incorporated preferential remineralization of P relative to N. In contrast, we do not face this fundamental problem with Fe, which is supplied via dust deposition to the surface waters and removed via scavenging by detritus. Moreover, there is substantial surface dust and Fe deposition in the North Atlantic. Thus, from the standpoint of modeling, relieving phosphorus limitation, and modeling N* are the major challenges.

6. This should be corrected in the next version, it was a publishing error, but we didn't catch it.

7. Yes: Fixed

8. We reworked the color scale. The model does well at the surface. As you can see in the new figure, we have added a difference map based on suggestions of the other reviewer. This highlights the region of the thermocline in the mode water formation region as having the greatest error. This is also a region where the thermal structure is poorly represented in the model (see the temperature vertical section added in response to your comment 1.), so the error is primarily due to model physics rather than biology.

9. This is a plotting error. The Pacific is not represented in the model.

Mills MM, Ridame C, Davey M, La Roche J, Geider RJ (2004) Iron and phosphorus co-limit nitrogen fixation in the eastern tropical North Atlantic. Nature 429 Prospero JM, Barrett K, Church T, Dentener F, Duce RA, Galloway JN, Levy H, II, Moody J, Quinn P (1996) Atmospheric deposition of nutrients to the North Atlantic Basin. Biogeochemistry 35: 27-73

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