**Biogeosciences
Discussions**

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

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I found this to be an interesting paper, even though I disagree with the authors' main conclusion that a "vicious cycle" is possible, where increased N fixation above the major water column denitrification zones can lead to accelerating net N loss for the oceans. I believe the paper should be published in BG after some revision and modification.

The vicious cycle disappears in the simulation presented when a parameterized iron limitation is added to the model along with advective transport of semi-labile DOM. Both additions make the model more realistic, with respect to what we know about the real ocean, particularly the eastern tropical Pacific. I would suggest even shifting the emphasis of the paper from the vicious cycle to the importance of these two factors for correctly simulating the marine N cycle.

The lack of an explicit iron cycle is a real weakness of the model. The static parameterization of iron limitation is also problematic, in that it does not allow for important feedbacks that could influence the N cycle dynamics. For example, increasing export above the denitrification zones, would lead to increased scavenging loss for sub-surface iron, ultimately reducing iron inputs to surface waters and providing a negative feedback on the export production. Also, the prescribed iron limitation is likely too weak, and would vary in the real ocean no a number of factors including upwelling rates, among other factors. In the ocean biogeochemical model that I utilize, the CESM-BEC model (Moore et al., 2004), the growth rates of the diazotrophs are reduced by $\sim$50% in the equatorial upwelling zone (compared with the factor of 0.8 reduction used here for diazotrophs), but off the equator iron limitation reduces the growth rates even more to $\sim$20-30% of the maximum. The paper assumes a 50% reduction in growth for the non-diazotrophs, while our model would predict similar reductions for the small phytoplankton, but even large reductions for diatoms. Thus, the iron impacts on N fixation are likely significantly underestimated in their IRON case.

There are additional factors that I think should work against the vicious cycle in the real ocean. Additional detail on the model structure in the methods section and appendix would help clarify the significance of some of these factors.

1) One factor is the export efficiency of different phytoplankton groups. Is there any difference in export efficiency between diazotrophs and non-diazotrophs in the model? I would expect that a shift towards increasing N fixation in the upwelling zone at the expense of diatoms, would significantly decrease the export ratio, even if total production were increasing. Given the positive buoyancy of Trichodesmium species, the export of efficiency of diazotrophs may even be lower than other small phytoplankton species.

2) A second point related to the N fixation seen in their simulations above the denitrification zones is whether it is realistic for diazotrophs to comprise a significant fraction of the community in upwelling zones. I would think that the diazotrophs would be largely outcompeted by faster growing phytoplankton, unless the subsurface denitrification had

completely stripped out DIN. What percentage of the community do diazotrophs make up in terms of production and export in these simulations, and how does this vary spatially?

3) Given the high nitrate concentrations in the upwelling regions, the diazotrophs present would likely be obtaining much of their N through DIN uptake, rather than through N fixation (Holl and Montoya, 2005). Thus, the potential for N fixation to increase export into the OMZs would be greatly reduced. How is N uptake partitioned between DIN and fixation in the model, and what are the relative fractions from each source in different regions?

The total denitrification and N fixation in these simulations are very low compared to the observational estimates. Some discussion of this fact and its causes should be added to the paper.

Figure 4 should include a panel with the observed OMZ thickness in the corrected WOA data. The simulated thickness, particularly in the CONTROL simulation appears too thick relative to the observations. How much of a role does this play in the vicious cycle?