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Biogeosciences
Discussions

Interactive comment on "Data-based assessment of environmental controls on global marine nitrogen fixation" *by* Y.-W. Luo et al.

Anonymous Referee #1

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Within the current manuscript the authors use a recently complied data base containing observed rates of marine nitrogen fixation collected over reasonably large spatial scales to investigate the environmental controls on this important biogeochemical rate. The controlling factors on marine nitrogen fixation are currently a matter of intense research and debate. Within this context the manuscript represents a welcome and significant contribution which would likely be of broad interest to biological/chemical oceanographers/biogeochemists. I am thus broadly supportive of the eventual publication of the manuscript in Bigeosciences, however, as outlined below, at the current stage I have some caveats with the approach, some of the data treatment and in particular with the strength of some of the conclusions drawn by the authors on the basis of the analysis presented. I would be interested in reading the authors' responses to the points raised below. I would further suggest that many of the caveats raised be-

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low should perhaps be more explicitly acknowledged within a revised version of the manuscript, alongside discussion of implications for the strengths of conclusions which can be drawn at the current stage.

Specific points

The authors use simple correlation analysis between the complied data base and various other data compilations and modelled environmental forcings. The nature of the environmental information is variable and the potential importance of this should be acknowledged. For example, for the macronutrients (N and P and hence P* etc.), observed (or rather extrapolated from a database) standing stocks are used within correlation analyses, whereas modelled dust fluxes are used for the Fe related environmental variable. If nutrients were an important driver of marine N2 fixation, relationships of rates to fluxes and standing stocks will differ. Additionally, as later acknowledged (Page 7383, line 5, Page 7387, line 25 etc.), dust fluxes would not be expected to be equivalent to overall Fe fluxes or indeed standing stocks. Similarly, if excess P* is a major driver, then P* fluxes (or convergences), rather than standing stocks would likely be expected to provide a better predictor variable. I think these caveats need to be tightened up and acknowledged up front rather than buried within the discussion. Arguably the assumptions made in drawing conclusions (e.g. see Page 7387, Line 17) should be explicitly stated in advance of the data being presented such that a reader has these in mind while considering the analysis performed

Related to the latter point, the caveat that nutrient drivers may not be expected to be simply correlated to the processes they influence is acknowledged in the case of P* (Page 7387, Line 13, Page 7370, Line 15), however the associated potential interpretive weakness introduced in any correlative analysis could be more explicitly acknowledged. I would recommend the authors consider the generic thought experiment presented in Figure 1 of Cullen (1991, L&O 36 1578-1599). The same idea would be expected to hold for N2 fixation and P* and presumably the standing stock of Fe, but crucially not necessarily the flux(es), again see previous point.

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One principal conclusion the authors draw, which is highlighted in the abstract, is that N2 fixation is coupled to regional N loss, as evidenced by the observed relationship between surface N2 fixation rates and minimum thermocline O2 concentrations (Fig 3b). I had a number of issues with the strength of this conclusion on the basis of the data presented. Firstly, examining the complied data (Fig 3b), the observed relationship between these two variables appears to be dominated by a sharp decrease in observed rates of N2 fixation in waters overlying thermoclines with minimum dissolved O2 >∼150 uM. Such dissolved oxygen concentrations are well above the levels which might be expected to be directly influencing water column denitirifcation/annamox and hence it seems unlikely that local coupling of N2 fixation to fixed N loss would be responsible for such a relationship. The associated spatial gradients in N2 fixation revealed in Fig 3b must be occurring across regions where the thermocline is already well oxygenated. Additionally, although the authors point out that surface P* could/should both drive and respond to N2 fixation, P* within the thermocline (i.e. associated with the O2 minimum) should provide a clearer potential indicator of excess P supply. Thus if local/regional subsurface fixed N loss were the dominant factor driving spatial patterns in surface N2 fixation, I would expect this to be reflected in the subsurface P* at least as clearly (and arguably more clearly) than in a rapid drop in N2 fixation in surface waters above regions with minimum O2 concentrations > ∼150 uM. In conclusion, as the authors themselves acknowledged (Page 7387), sub-surface P* distributions appear to contradict one of their principal conclusions and personally I feel these caveats should be expressed more explicitly, particularly if the abstract is to state them as strongly as present. As one further note, the authors suggest that patterns of excess P considering organic nutrients may provide a different perspective. They might wish to consider a recent modelling study which suggests that organic nutrient cycling could act to spatially decouple N2 fixation from local fixed N loss (Landolfi et al. 2013 Biogeosciences 10 1351-1363).

Page 7371. The N2 fixation data used are depth integrated. Was the integration depth consistent throughout the studies? If not, how was it defined/chosen within individual studies and would a different choice have influenced results? Similarly, would similar conclusions be reached if surface volumetric (rather than depth integrated) data were used? I would suggest that the authors perform simple sensitivity analyses, re-running their correlation analyses with both depth integrated rates calculated over different integration depths and surface volumetric rates. This potentially relates to points above, some of the environmental drivers used could be considered areal (e.g. dust fluxes), while others are volume specific (e.g. N, P, P* etc.). The authors should acknowledge that in some cases they compare a depth integrated rate with an areal flux, while in other cases they compare with volumetric standing stocks.

Page 7373-7374. Use of a quadratic to describe the response of (log transformed) variables could be considered arbitrary. Significantly, this occasionally resulted in fitted responses being non-monotonic, which surely means that the performed analysis sometimes contradicts the expected responses/hypothesis? e.g. see Figs. 3 b & d? The authors acknowledge this in the case of dust deposition (Page 7378), but effectively dismiss it in the case of thermocline O2 (Page 7380), why should these be treated differently?

Page 7387, Line 25. Although the authors claim to have set up an observationally based assessment, the dust flux used is already a model. Consequently I would suggest that using the P* convergence is at least as appropriate and, given the conclusions reached, a direct comparison with the kind of analysis performed by Deutsch et al. (2007) would seem entirely appropriate.

Regarding the modelled dust flux, the authors could also have used modelled soluble atmospheric iron fluxes as well as potentially providing comparisons with other atmospheric dust models. Additionally, I wonder whether model based estimates of other potential Fe sources could also be included. Irrespectively, as the Fe related variable is based on a model product, some sensitivity analysis would certainly seem appropriate.

Finally, as stated at the start of the review, despite the potential caveats I highlight, I appreciate the effort represented by this study and would expect it to make a significant contribution to the field. The authors make some suggestions at the end related to additional data and data coverage which might allow a more comprehensive analysis to be performed in the future. I believe these points are well made and I will look forward to seeing similar follow up analyses performed over the coming years as global data coverage for e.g. biological rate measurements (N2 fixation and denitrification/annamox), trace metals (e.g. through GEOTRACES) and other potential forcing factors (e.g. pCO2) increase.

Additional minor points

Page 7387, Line 23. remove 'the', '. . .with global warming'

Page 7369, Line 9. Surely nutrient supply and coupling to regional fixed N loss (and hence excess P) are fundamentally related?

Page 7370, Line 2. Maybe 'As diazotrophs are not limited by fixed N supply'.

Page 7370, Line 24, Suggest: '. . .diazotrophs must have evolved mechanisms for alleviating oxygen inhibition.'

Page 7372, Line 17. As discussed above, I am sceptical that minimum O2 in the upper 500m will robustly represent subsurface fixed N loss.

Page 7374. Line 26, Page 7378, Line 2. I would suggest 'hypothesised' is more appropriate than 'theoretically derived' in these sentences.

Page 7387, Line 24. I think caveats need to be included here. Maybe the authors could state: 'On the basis of the available data and analysis performed it appears that minimum dissolved oxygen. . ..are the best currently available predictors for the global patterns of marine NF'. I think such a statement encapsulates what the authors demonstrate. Subsequent linkages to causality, i.e. 'environmental control', will inevitably remain more speculative.

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Page 7388, Line 3. See also Noble et al. (2012, L&O 57 989-1010), for further evidence that OMZs are likely sources of sub-surface Fe.

Page 7388, Line 28. '. . .as a function of. . .'

Page 7390, Line 6. 'Influenced' or 'Controlled' might be better than 'Limited'.

Figure 1b. Did the authors do a test for log normality?

Fig. 3. Why are plots not included for the other environmental variables?

Fig. 6. It wasn't immediately obvious how to compare these colour scales. Can the same scales be used for both sub-panels?

Interactive comment on Biogeosciences Discuss., 10, 7367, 2013.