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

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## 1 Summary

Luo and co-authors use their recently compiled global data base of marine N-fixation to determine the environmental factors that might control N fixation in the open ocean. Using linear and quadratic regression models, they find solar radiation/sea-surface temperature to be the most important determinant of N-fixation, followed by the minimum O2 concentration in the top 500 m. Additional, but smaller contributions were found for the surface nitrate concentration and P*, i.e., the excess of surface phosphate relative to nitrate. In contrast, no statistically significant correlation emerged for iron deposition. The authors conclude that within the warm, well insolated low-latitude oceans,



the generation of fixed nitrogen deficits by denitrification is a key control on marine N fixation, with nitrate modifying this pattern. Extrapolating their regression model to the globe, they arrive at a global N-fixation rate of about 70 Tg N yr-1 (error range 50-100 Tg N yr-1).

## 2 Evaluation

Although marine nitrogen fixation represents a key process in the global cycling of fixed nitrogen within the ocean, our understanding of the processes that actually control its rate and distribution is relatively poor. This is in a large part due to the lack of adequate observations and process studies. The data compilation put together by Luo et al. in ESSD overcomes to a substantial part this data limitation and opens many new avenues for exploring the validity of competing hypothesis, such as the question of the importance of iron in regulating marine N-fixation. In this manuscript, Luo et al. capitalize on this potential and provide - for the very first time - a global analysis of the environmental factors that might control the rates of marine N fixers. This study thus makes an important and novel contribution to the field and is therefore of great interest to the readers of *Biogeosciences*.

The statistical approach chosen, i.e., fitting linear and polynomial regression models to the data, is entirely adequate for this first in depth analysis of the data, and the method is applied with care and full recognition of the potential pitfalls and problems (e.g., cross-correlations of the predictor variables, non-normal distribution of variables, etc.). The results are very interesting and relevant, and thoroughly discussed. The paper is overall also well crafted, clearly organized, and well illustrated. Thus, I am fundamentally very supportive of the eventual publication of this manuscript.

However, I have two major concerns that I would like the authors to consider before I can give my ok to this paper. The first one concerns a core issue of the paper, while the other is more of editorial nature.

1. Minimum oxygen as a key driver: I very much like the conclusion drawn by the authors that the nitrogen deficit created by denitrification is a key control in determining marine N-fixation, but I am not convinced that this conclusion can actually be drawn from the presented analysis. This conclusion hinges on two principal elements: First that the results are robust. Second, that there is a mechanistic explanation for the correlation. I see weaknesses in both elements:

   Regarding the robustness. A serious issue in the whole analysis is that the data coverage is rather limited, with a very (disproportionally) large number of samples coming from the North Atlantic. The North Atlantic data are also those with the largest variance, since the data from the other ocean basins show relatively similar rates. My suspicion is that this low variance in the other basins is very artificial, since people don't tend to make measurements where they don't expect N fixation to be important. This lack of a wider suite of measurements in the other ocean basins has consequences for the robustness of the results, since most of the trends will be driven by the high variance of the data in the Atlantic, with little influence of the data from the Indo-Pacific. This is rather important in this context, since this means that the finding that the minimum oxygen concentration is an important determinant for N fixation is largely emerging from the data in an ocean basin where oxygen levels never get low enough to actually cause denitrification. And this finding is not really challenged by the data from the Pacific, because no measurements were taken in the critical areas above the OMZ.

   I have additional concerns related to the mechanistic explanation of the correlation. The authors take the minimum O2 concentration within the upper 500 m as a proxy for the loss of fixed nitrogen from the ocean by denitrification. This is problematic, in my view, and this for two reasons. First, oxygen levels in general are not a good indicator of denitrification, as the relationship between them is extremely non-linear, given the threshold behavior of denitrification. Oxygen levels



   might be better related to denitrification created nutrient anomalies (negative N* or positive P*), but also these correlations are weak. Second, it is rather unclear how anomalies created deep in the thermocline can influence N fixation in the overlying surface waters. If they do, this usually happens rather far away from where the nutrient anomalies actually are actually generated. So I find it difficult to relate at a given location N-fixation at the surface with an oxygen concentration deep down in the thermocline.

   Thus, I need to put the author's conclusion about the importance of denitrification in controlling N fixation into question. A closer look at Figure 3b confirms this. The fit to the data suggest actually relatively constant N-fixation rates for any minimum $O_2$ level below 150 mmol m$^{-3}$ $O_2$. Only at relatively high minimum $O_2$ levels, N-fixation starts to decrease substantially. Based on first principles, one would expect a rather different relationship. With denitrification really kicking in only at $O_2$ concentrations of 10 mmol m$^{-3}$ or below, one would expect a convex relationship with low N-fixation rates at any $O_2$ levels above a few tens of mmol m$^{-3}$, rapidly increasing once the $O_2$ levels get low. What is observed is a concave relationship with high rates until a rather high $O_2$ level. So to me, this mechanistic explanation does not make sense.

   Considering the two elements together, i.e., most of the trends coming from the Atlantic where there is no denitrification to drive N-fixation, and a relationship that is difficult to explain mechanistically, I have serious problems seeing how the author's conclusion can be supported by the presented evidence.

   I need to emphasize that I am actually of the opinion that nutrient anomalies created by denitrification are critical in determining N-fixation, I just don't think that these data demonstrate this convincingly. For me, a strong correlation with the transport convergence of P* or something like that would be much more convincing. And of course, N-fixation data from the OMZ regions would be great!

2. Writing: The manuscript could benefit substantially from a serious effort to tighten

it up. There are various repetitive elements such, for example, the first paragraph of section 3.4, where the authors explain again how the global estimate of N-fixation was achieved, despite the fact that they described it in the method section. The authors also tend to describe first the various choices they considered and only then write about what they actually did. I suggest to tighten this up and simply state what the authors have done.

## 3 Recommendation

I recommend acceptance of this manuscript after a moderate revision. I suggest that the authors reconsider their main conclusion about the role of denitrification controlling N-fixation.

## 4 Minor comments

p7370, line 13, "P*". Please give credit to Deutsch et al. (2007) for introducing this derived tracer.

p7370, line 23, "role of wind". I am surprised to read that someone seriously would have argued that wind is important because it pushes oxygen into the surface ocean. In fact, oxygen is nearly everywhere where diazotrophs live supersaturated, i.e., higher winds would have actually caused oxygen to go down rather than up, except when the winds go into the wave breaking regime, where bubble entrainment begins to matter. However, I suspect that the physical turbulence induced by wind is key, rather than the oxygen concentration changes. So I agree with your conclusion, but I am actually questioning whether this should be brought up here at all.

p7371, line 16 and elsewhere, "linear regression and multiple linear regression" I am



sure that there is a lot one can potentially criticize about the authors using essentially linear models, but I find it appropriate for such a first look at this important data set.

p7372, line 6, "N15 assimilation and C2H2 reduction methods" The two methods do not measure the same thing. Wouldn't it be more prudent to statistically model these two sets of measurements separately?

p7372, line 14, "surface radiation" I would have used the mean radiation over the mixed layer depth as well. Why wasn't it considered? An other interesting alternative is the photoperiod.

p7373, line 6, "annual climatologies". (also later comment on page 7388). I find it peculiar that the annual means gave better results than the match-ups. This deserves perhaps a somewhat deeper discussion than what is provided on page 7388, line 9.

p7377, first paragraph: Most of the information in this paragraph is textbook knowledge. Shorten it to the essential element, i.e., the relatively high correlations between some of the independent variables.

p7388, first paragraph: Shorten. Simply state what was done.

p7379, all of page: Shorten, Simply state what was done.

p7380, line 22, Figure 5: The importance of the Atlantic data in determining the skill of the statistical model is particularly evident in this Figure. Without the Atlantic data, the skill of the model would be essentially nil beyond the prediction of the mean rate.

p7381, first full paragraph: This paragraph can be largely deleted, as it repeats much of the information already provided in the method section. What needs to stay (but perhaps in the method section) are the limits and how these were determined, exactly.

p7381, line 23 and Figure 6: I found it hard to compare the two figures since they use different scales (linear with exponential color scale) versus (log with linear color scale). Please make them consistent.

p7381, line 26 "high errors in the OMZ regions". This is another line in the argument for why the conclusions about the critical role of denitrification is not really tenable. It turns out that the errors are the largest exactly in the region where one would expect to see the strongest feedback to the denitrification.

p7384, line 16 "Note that ammonium ..." This and the following sentences read like they were added later. They are not well integrated into the text.

p7386, line 17, "In summary, our study suggests that within the warm, stratified, high solar radiation band in the subtropics and tropics, the major factor governing spatial variations in marine NF is the regional fixed N loss induced by low-level dissolved oxygen." As explained above, this is the conclusion that the authors cannot draw from their results. It does not mean that the statement is incorrect, it simply means that the data neither support nor refute this statement.

p7387, line 23, "transport convergence of P*" This is what I think is needed to back up the conclusion. As usual, the story won't be that straightforward given the observations that low-latitude phytoplankton tend to have higher N:P ratios (Martiny et al., 2013).

p7388, section uncertainties: I liked this thorough discussion.

p7389, line 18: you might want to consider adding the recent estimate of Eugster and Weber (2012) (GBC)

p7391, line 19: I couldn't agree more with this recommendation.

p7411, Figure 7. Please explain how you computed these "anomalies".

Nicolas Gruber, Zurich, August 14, 2013

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