Biogeosciences Discuss., 11, C3638–C3642, 2014
www.biogeosciences-discuss.net/11/C3638/2014/

Interactive comment on "Understanding predicted shifts in diazotroph biogeography using resource competition theory" *by* S. Dutkiewicz et al.

Prof GRUBER (Referee)

nicolas.gruber@env.ethz.ch

Received and published: 21 July 2014

1 Summary

Dutkiewicz and her co-authors investigate the possible response of marine diazotrophs to future climate change using a global ocean biogeochemical/ecological model forced with output from an Earth System model of intermediate complexity. The model predicts a biogeographic expansion of the diazotrophs, particularly in response to a presumed increase in the atmospheric deposition of dust, but also in response to warming and the associated increase in vertical stratification and reduction in the vertical supply of macronutrients. Using concepts from classical resource competition theory, Dutkiewicz et al. show that these changes can be successfully predicted by changes in

the nutrient supply ratios, which alter the distribution of the niches where diazotrophs can successfully compete against the other phytoplankton.

2 Evaluation

Understanding and predicting the future evolution of marine ecosystems is one of the key challenges facing the marine research community. Of particular concern is the response of the lower trophic-level ecosystems, and particularly that of the primary producers, as they provide the basis of (nearly) the entire marine food web. Thus, Dutkiewicz and her co-authors address an issue of high concern, making this study interesting for a broader community. Of particular interest is their use of a theoretical framework to analyze and understand their model-based projections, which makes this paper stand out relative to most other studies that have looked at future changes in lower trophic-level marine ecosystems. The employed model is adequate for the intended task, the results are clearly described, illustrated and discussed, and the conclusions are solidly based upon the presented material. The paper is well written and generally easy to follow. In summary, this is a very good paper, whose publication I am glad to support.

There are, however, a few of major comments that I would like the authors to consider when preparing the final version of their paper.

- (i) *Strengths and limits of resource competition theory*: I am convinced by the author's arguments and the presented evidence in this paper as well as those by Ward et al., (2013) and Dutkiewicz et al. (2012). At the same time, I think the authors should also emphasize more the caveats and limits of this approach. Some of this has been discussed by Ward et al. (2013), i.e., strong bottom up control, higher Fe requirements and lower growth rates relative to "normal" phytoplankton, and steady-state assumption, but I think it would be good if some of this was revisited in the light of the 3-D simulations presented here and in light of potential future changes. But I would like to submit that the most important limitation is that the resource competition theory works relatively well for the biogeography of N-fixers, but is of limited use to actually predict the magnitude of N-fixation, which - in the end - is the more important quantity.

- (ii) *: Ocean interior changes:* The paper leaves the impression that all the changes we see in the surface ocean are solely driven by the response of the lower trophic-level ecosystem to changes in the supply ratio, thereby disregarding the fact that changes in the ecosystem might have important consequences on these supply ratios, i.e., leading to potentially important feedbacks. For example, Sarmiento et al. (2004) and others have shown that e.g. iron fertilization induced changes in upper ocean ecosystem structure (and physiology) in the Southern ocean have worldwide repercussions, as the changes in diatom growth there alter the (preformed) nutrient concentrations of the mode and intermediate waters that are exported toward the lower latitudes and fuel an important part of primary production there. Similar effects can occur elsewhere, e.g., by changes in the remineralization depth of the exported nutrients in response to changes in the nature (and timing) of the exported material. Therefore, I was a bit surprised to see no discussion whatsoever on how nutrients (and their ratios) change in the ocean interior. As written the text implies that all the changes are driven by changes in the physical transport, but not by changes in the concentrations (or their ratios). I doubt that this is truly the case.

- (iii) *Monitoring:* The authors suggest that the monitoring of surface nutrient concentrations could be a "clear and easily interpreted indicator of ongoing global change". I have very strong doubts. In fact, even the authors themselves downplay this later on in the paper, given the fact that other processes could completely mask any trend. Perhaps the most important reason for doubt is the potential flexibility of marine phytoplankton with regard to their nutrient stoichiometry (es-

pecially with regard to iron). While this does not cause the resource competition theory to fall apart completely, it does cause a substantial shift in the exact location of the transitions between the individual provinces. Furthermore, I have some doubts regarding the transferability of the resource competition theory to other phytoplankton functional groups, i.e., groups where grazing control, seasonal succession, etc, might be more important than for diazotrophs. Therefore, I would remove this aspect from the paper.

- (iv) *Biogeography as an emergent property:* Although the authors provide convincing arguments, I have not found a good answer to the question of whether the good agreement between model and theory is simply a consequence of the fact that the model was built according to the concepts of competition theory. Or in other words, that the good agreement between the modeled biogeographic pattern and the nutrient supply ratio is not a truly emergent property of the model, but rather a consequence of the design of the model. There are several elements that point in this direction, e.g., the lack of top-down control for the diazotrophs, the low growth rate and the high Fe demand, etc. This is perhaps more a philosophical comment than one that one can respond to in a straightforward manner. But I encourage the authors to reconsider their conclusions about the real-world applicability of their results."

3 Recommendation

I recommend acceptance of this manuscript with minor revisions. I do encourage the authors to consider my comments.

4 Minor comments

p7120, line 19: "remineralization of organic matter". I don't understand why this has to be included here. It is not really an external input to the upper ocean ecosystem, but an internal one. Please explain.

p7122, line 10ff: "growth rates of the plankton do not change". I am a bit puzzled here. First, why do the phytoplankton in the Fe limited regions of the Southern Ocean and the Equatorial Pacific not respond to the increased supply of Fe? Second, why aren't we seeing also changes in the nutrient distribution within the thermocline, driven by the Fe induced changes in production and export in the regions that determine the pre-formed concentrations of these nutrients. See my major comment (ii) above.

p7128, line 14: "potentially sensitive and powerful indicator". I disagree (see main comment (iii) above).

Figures: The figures have some room for improvement, e.g. better resolution, labeling of axes, choices of colors and relative line widths, etc.

Nicolas Gruber, July 2014

Interactive comment on Biogeosciences Discuss., 11, 7113, 2014.