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Discussions

Interactive comment on “The fate of new production from N_2 fixation” *by* M. R. Mulholland

M. R. Mulholland

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Response to comments by Maren Voss: Thank you for the suggestions and the additional references that you brought to my attention. They have substantially improved the manuscript.

Comment 1. The reviewer is absolutely correct that there has been much debate over the “theoretical” versus actual ratio that should be used to convert acetylene reduction to N2 fixation rates. This was a subject of active research (also see Mary Scranton’s work in the 1980’s) and is discussed at length in Mulholland et al. 2006, 2004, and Mulholland and Bernhardt 2005. I refer the reader to those discussions but have added to the parenthetic pointing to these discussions as I don’t want to be redundant.

Comment 2. References (and a sentence in the text) have been added to the Lomas & Glibert and Lomas et al. papers.

Comments 3 & 4. I have added sentences and reference to the Ohlendieck et al. paper.

Response to comments by Reviewer 1: Thank you also for your valuable and detailed suggestions. I think that they have substantially improved the manuscript.

Regarding new and regenerated production, the author has added a reference to Dugdale and Goering's seminal paper in 1967. I don't want to get too distracted by this point as it has been defined elsewhere and numerous times. By definition, N2 fixation in the systems reviewed is "new", the point I'd wanted to make is that much of this "new production" may be uncoupled from "export production (Eppley & Peterson 1979). I have tried to add clarifying sentences where possible.

How to address the unresolved questions experimentally? More direct measurements and a better understanding of physiology. I have added a sentence to the abstract and elsewhere in the manuscript to reflect that.

Qualified the paper as a review both in the abstract and introduction. The 2006 paper motivated this more in-depth examination of many of the physiological variables discussed here but that were beyond the scope of the other (which was not a review).

I have added relevant references and included them in discussion in the appropriate sections.

I agree that the title was less than optimal (I'm very bad at titles). That said, I changed it only slightly (replaced N2 fixation with diazotrophy). I did want to imply it was the fate of the recently fixed N and C that is of interest. By definition N2 fixation is new production (as the reviewer states) but, its contribution to export production (vs. fueling regenerated production and potentially loss through denitrification) is less clear (e.g., the fate of the new production from N2 fixation). I try and make the argument that N2 fixation, while "new production" may not contribute much to export production, rather be involved in a "futile" cycle of input and loss that is "a wash" in the end. I have added some discussion about denitrification. It is the balance between new and export that is of interest. Is the ocean gaining or losing N?

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I have indicated which data is unpublished in the tables.

Almost all of the specific technical corrections were made to the text. With the following exceptions:

Trichodesmium spp. was left plural throughout. Many field studies don't distinguish species.

I added more citations to my physiological work including N release and fixation with physiological state (I was trying to reduce self-citation but there is still way too little physiological work that has been done). For example, the Mulholland et al., 2004a paper was about verifying that the difference between acetylene reduction and 15N2 uptake could be used as an index of N release. The Mulholland et al., 2004b paper shows direct uptake of release products by co-occurring organisms. I have added a citation for a similar study done in the Baltic during cyanobacterial blooms (thanks to Maren Voss) to bolster those observations.

On page S420, the first complete comment, as the reviewer notes, all forms of N can potentially be new or regenerated, therefore I hesitate to ascribe particular compounds to a particular category. We are learning that the paradigm itself may be outdated.

The reviewer lists some great questions on page S421 (and elsewhere) and has hit on some million-dollar questions that are as yet unresolved. I have tried to include some of these in the manuscript as they point to important future directions.

Table 1. I tried to clarify the table legend. I put things in hourly rates because most of the rate measurements were not done over a complete 24 hour cycle (in only one paper could I not find hourly rates)! I did not want to multiply by 24 if that was not appropriate (and it is still unclear whether there is diel periodicity to diazotrophy for some of the recently identified groups). So, what I did instead was divide the daily rate report (Dore et al.) by 24 and indicated that with the footnote.

Figure 1 is revised and the mistakes that the reviewer pointed out are corrected. Panel

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Interactive Comment

B was meant to be a traditional view of the fate of new production, in this case from diazotrophy.

Interactive comment on Biogeosciences Discuss., 3, 1049, 2006.