Biogeosciences Discuss., 12, C154–C160, 2015
www.biogeosciences-discuss.net/12/C154/2015/

Biogeosciences
Discussions
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Interactive comment on "Relationship between N : P : Si ratio and phytoplankton community composition in a tropical estuarine mangrove ecosystem" *by* A. K. Choudhury and P. Bhadury

G Trommer (Referee)

trommer@biologie.uni-muenchen.de

Received and published: 11 February 2015

The manuscript intends to correlate phytoplankton community compositions with nutrient load and ratios in the Sundarbans mangrove ecosystem (West Bengal, India). Especially the conceptual idea of Redfields N:P 16:1 ratio is used to discuss occurrence and abundances of phytoplankton species. The paper includes interesting and unique data, which fits in the scope of BG and are worth and important to publish. However, not all of the data included in the manuscript are sufficiently discussed towards the scope of the paper in my opinion. The genetically derived "community composition" is not further analysed in terms of its relationship with N:P:Si ratios and serves as

secondary justification for diversity besides the microscopic counting. The genetic data should be more integrated and analysed with CCA similar to the microscopic counting data or removed and published in a separate manuscript. The title is appropriate if the genetic data are analysed accordingly.

I have two major concerns in terms of the interpretation of the data and the understanding of the Redfield concept and nutrient limitation: in the discussion the authors refer to the upper limit of 16:1 N:P as indication that the mangrove system is not nutrient limited, which is the wrong argumentation. A low N:P ratio clearly indicates a relative N limitation, although the absolute N amounts are high! Furthermore, they conclude that due to the dissolved nutrient ratios and the phytoplankton diversity, the ecosystem is not eutrophic. These conclusions are not supported from the data and the use of nutrient ratios for a trophic state assumption is not maintainable. Absolute amounts of nutrients as well as Secchi depth indicate strong eutrophication. These points should definitely considered and according sections paraphrased. Conclusions and abstract should be adapted accordingly. In terms of this major concerns, I recommend the book of Sterner and Elser (2000) "Ecological Stoichiometry" as literature for nutrient limitation patterns and the discussion of the Redfield ratio or divers article of RW Sterner and JJ Elser.

I strongly encourage the authors to make some amendments in the interpretation, since this is a very nice study and valuable data.

Details Introduction P 2308 L 24: Monod and Droop, 1968, 1983 is just Droop in the reference list.

P 2309 L 10: Broecker and Henderson 1998 is surely the wrong citation here. The article has nothing to do with N:P ratios and is just speculating about past ocean conditions, which were biologically questioned by Falwowski (2000). The entire sentence is not well understandable since elemental composition is objectively measurable but nutrient limitation is a subjective concept. L 15: Nitrogen is abbreviated with N, but not

consequently used afterwards.

P 2310: First paragraph sounds weird and references should be added. L10-11: sentence? Objectives are not very clearly formulated.

Methods 2.3 P 2313 L 28: Turner et al. 1998 says nothing about the used silicate method. Please refer to an appropriate method.

2.9 Sequence analysis and molecular phylogeny: I have no strong background to judge the applied methods.

In total, methods seem well thought-out and well documented by references.

It is a pity that no chlorophyll a was measured, since a lot more sophisticated methods were applied. There should be some literature to convert Secchi depth into chlorophyll a values. This would be informative and an important parameter to classify the ecosystem in a trophic state.

Results 3.2 P 2319 L 11-14: I do not see this for ammonia in Fig. 2b. L 17 “it equalled the 16 : 1 ratio during pre monsoon”? I see fluctuations between 7 and 31 in Fig. 3 a. L 18-19: which nutrient limited condition? N or P? The whole paragraph, especially L 11-21 is difficult to follow and should be clarified.

3.3 P 2320 L11: I would not talk about that water temperature was “regulated” by ait temperature. It can also be influenced the other way around. Better to say neutrally that water and air temperature were highly correlated.

3.4 It is astonishing that no cyanobacteria were found. This sounds almost impossible considering the high nutrient levels and low N:P ratio, which indicates N limitation and should promote cyanobacterial growth due to their ability to fix N. Any comment on that?

P 2321 L 8-9: Add Bacillariophyceae after diatoms and the table reference at the end of the sentence. L 13: Better name the dinoflagellate taxa (Dinophyceae, Noctiluco-

phyceae) in parentheses, since it is not immediately clear from the table for a reader who is not familiar with phytoplankton taxa.

3.5: P2324 L 20: I think this should be 2 (instead of 3) clones for Stn3_Aug13_. At least what I can see from the supplement. L 22: I do not agree that this is a “close” phylogenetic affiliation with Teleaulax. Maybe better to talk about the “nearest” phylogenetic affiliation with Teleaulax in this context.

This section is hard to read and includes a lot of descriptions, for which I can not extract a valuable information. Maybe this can be shortened or restructured, so that the reader does not loose track. For the reader a more logical order would be to group all counting related phytoplankton results together (3.4, 3.6 and 3.7) and finally the genetic results (3.5). At the moment the genetic results interrupt this structure.

3.6 P2325 L9-11: This opposite pattern is observed consistently only for Navicula and not in Nitzschia from Table 4.

3.7 P2327 L17-18: I doubt that pH explains much of the abundance and is not the primary determinant of their abundance. According to Fig. 1 the variability of pH is very low (0.2!) and ecologically insignificant for an ecosystem which is characterized and adapted to larger ecological fluctuations and water movements like an estuary. This could be an artifact and should be discussed with care. Better talk about “slight preference towards” higher pH or something like that.

Discussion Si:N ratios are not well discussed, although announced in the title and the introduction. The genetic data are not discussed in terms of nutrient ratios at all.

P 2328 L 27-28: I do not understand the context of this sentence. Decreasing oxygen in post monsoon season?

P 2329 L 3: Low ammonia concentrations do not necessarily mean they contradict eutrophication. Ammonia is the preferred (and only) N source for many phytoplankton taxa, and as long as there is ammonia it is consumed immediately. This accounts also

for some of the found diatom species Thalassiosira and Skeletonema (Dortch 1990).

P 2329 L 8-9: In order to exclude misunderstanding: N:P below 16:1 does NOT mean that there is neither N- nor P-limitation! Around 16:1 does not indicate any limitation. During spring and summer 2013 at Stn. 3 N:P was way below 10 (Fig.7), which indicates N limitation! Above 30:1 one could talk about P limitation, but this was not the case during the observed year according to the data. For me, the community looks largely Si limited, since Si:N is until post monsoon mainly below 1. Only when Si increases, a diatom bloom occurred.

P 2329 L15-16: According to average phosphorus levels, the Sundarbans mangrove ecoregion is even hypereutrophic! Lowest P concentrations are at 1 μM indicates eutrophic conditions and above 4 μM hypereutrophic conditions are defined according to OECD rules. Also in terms of Secchi depth (Table1) the system is highly eutrophic (Lee et al. 1995). The conclusion that the mangroce system is not eutrophic cannot be supported by the shown data. The phytoplankton diversity as a measure of trophic state are rarely used, since they represent a very weak predictor of trophic state (Rakocevic-Nedovic and Holler 2005). Intersting references for this comment: Anderson et al. (2002) Harmful Algae Blooms and Eutrophication: Nutrient sources, Composition and Consequences. Estuaries, 704-726. Rakocevic-Nedovic J, Holler H (2005) Phytoplankton Community and Chlorophyll a as Trophic State Indices of Lake Skadar (Montenegro, Balkan). ESPR – Environ Sci & Pollut Res http://dx.doi.org/10.1065/espr2005.04.241. Lee, G. F., A. Jones-Lee, W. Rast, (1995) Secci Depth as a Water Quality Parameter, Report of G. Fred Lee & Associates, El Macero, CA Vollenweider, R.A., and Kerekes, J. (1982) Eutrophication of waters. Monitoring, assessment and control. OECD Cooperative programme on monitoring of inland waters (Eutrophication control), Environment Directorate, OECD, Paris. 154 p. Jones RA, Lee GF (1982) Recent advances in assessing impact of phosphorus loads on eutrophication- related water quality, Water research, 503-515. Lee G. F. & Jones R. A. (1981) Application of the OECD eutrophication modeling approach to estuaries.

Estuaries and Nutrients. pp. 549-568. Humana Press, Clifton, NJ.

P 2329 L 18: Delete “become eutrophic” since the system is already eutrophic. See comment before.

P 2331 L 5: Samanta and Bhadury, 2015: missing in the reference list

P 2331 L 29 – P 2332 L 3: Two almost identical sentences.

P 2332 L 5: stoichiometry does not determine the community: other way around!

References: The number of references is appropriate, although I would recommend literature of RW Sterner and JJ Elser. There are a lot of references, which are not cited in the text e.g. Butterwick et al. 2005, Foy and Gibson 1993, etc.

Table 1: What are the standard errors or standard deviations in the table and out of how many data points are these calculated?

Table 4 The shape column with the exact shape parameters is not necessary in my opinion. For the unfamiliar reader it would be more informative to just indicate centric and pennate as shape.

Figures: Fig. 1: Maps and plots are too small and very hard to read. I definitely cannot distinguish between Stn. 1 and Stn. 3. Maybe better to split in two figures, one with maps and locations and one with environmental parameters. Temperature scale could be adapted e.g. 15/20 °C to 40°C. Maybe it would be nice to highlight the monsoon phase in the plots by underlying a grey box.

Fig. 2: spelling error: Stn. 1 (nitate) What are the error bars and of what? Standard errors? Of weekly/two weekly measurements (N=?)?

Fig. 4 and 5: Is the legend “Active” necessary?

Fig. 5: It is not convenient to look up the species numbers in the text. I would place them in the figure caption in numerical order. Also add the percentage of explained

variance on the CCA axes.

Fig. 7 D: N:P is shown in squares, which is diamonds in all other plots.

Fig. 8: It would be nice to have the species numbers in the figure caption, as well as the percentage of explained variance of the axes.

Supplement The amount and quality of supplementary material is appropriate.

Interactive comment on Biogeosciences Discuss., 12, 2307, 2015.