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Biogeosciences Discussions

Interactive comment on "Response of Nodularia spumigena to pCO₂ – Part 2: Exudation and extracellular enzyme activities" *by* S. Endres et al.

Anonymous Referee #2

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This is an interesting manuscript, describing the potential effects of elevated CO2 on the extracellular material associated with the cyanobacterium, Nodularia spumigena. The exudation and turnover of organic matter by phytoplankton is poorly understood and the subject area is clearly appropriate for publication in Biogeosciences. Whilst I think the manuscript is of merit, I have some major concerns about the robustness of the findings and the interpretation of data. In particular, I disagree strongly with the major conclusions of their manuscript.

1) Replication. The manuscript describes a single growth experiment with three CO2 treatments. Although replication within the experiment was high (12 replicates per

treatment), I am extremely concerned that no attempt has been made to replicate the findings of this single experiment. This can be difficult for certain studies (long term evolutionary studies, field studies, etc) but for a short term laboratory study there is a clear need to ensure the findings are reproducible and robust. Given that laboratory studies of the effects of elevated CO2 on Nodularia have already produced conflicting results (Czerny et al, 2009), surely there is a requirement to ensure that the findings presented here are sufficiently reproducible? Clearly, the authors do not need to replicate their extensive analysis in its entirety but they must present an indication of the robustness of their major findings.

2) Increased exudation. In the abstract the authors conclude that high CO2 leads to increased exudation. However, when normalised to biomass (POC) the total concentration of mucinous substance is not correlated to CO2. Surely this means that exudation has not increased and that the increased in exudates measured is explained by the increase in biomass? The authors do state that 'cell-specific rates do not change' and again in the discussion they mention that 'we cannot confirm a stimulating effect of elevated pCO2 on exudation'. Therefore, it is not clear what their conclusions are. Does exudation increase or not with increasing CO2? This has to be clearly addressed as it is a major conclusion of the manuscript.

In the abstract the authors state that more mucinous substances accumulated in the 'growth phase' but they present the data for the concentrations reached on day 15, after the growth phase. This should be clarified.

3) Enhanced recycling of organic nutrients promotes faster growth. In abstract (p5110 ln 28) and the final paragraph of the discussion (p5131 ln 18), the authors state that their results reflect enhanced recycling of organic nutrients. However, there are no data presented in the manuscript to support this conclusion.

For example, APA was greatest in the high CO2 treatment at day 9. However, biomass is greatest is the high CO2 treatment and the increased APA may just reflect this greater

biomass. Indeed, APA demonstrated a positive correlation with POC and chl a, but did not show a correlation with CO2. Furthermore, there was no significant difference in DOP concentration between CO2 treatments, so there is no evidence for increased DOP uptake at elevated CO2. How then, can the authors conclude that elevated CO2 leads to enhanced recycling of organic nutrients which in turn promotes growth? As the cultures were P-limited this is one possible explanation, but their results may simply demonstrate that the increased availability of carbon at high CO2 leads to enhanced growth.

The authors also state that elevated CO2 leads to faster growth of Nodularia. However, they do not measure growth rates and for the data presented it appears that growth rate is very similar initially, but at elevated CO2 the cells grow to a higher density.

In summary, I think the data presented in the manuscript does not support the authors' conclusions and because of this I think the manuscript in its current format is potentially misleading. There is not sufficient evidence for increased exudation or increased uptake of organic nutrients, and there is little evidence to suggest that these processes are responsible for increasing growth at elevated CO2. These issues have to be addressed before the manuscript is suitable for publication.

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