Referee comment on "Nitrous oxide (N₂O) synthesis by Microcystis aeruginosa" by Federico Fabisik et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-1153-RC3, 2022

Review of „Nitrous oxide (N₂O) synthesis by Microcystis aeruginosa“ by F. Fabisik et al.

In this manuscript, the authors investigate the production of nitrous oxide by the cyanobacterium Microcystis aeruginosa and find that the addition of oxidized Din species, and especially nitrite, fosters the production of N₂O. The paper as such is well-structured and clearly written, and I find that the results as such are novel and clearly deserve publication as a letter in Biogeosciences.

However, I do have some comments regarding the biogeochemical relevance of the investigated process. I think addressing this may increase the impact of the manuscript. Generally, I would like to urge the authors to think towards environmental consequences and applications of the mechanisms they investigate – i.e., how likely is N₂O synthesis under environmental conditions? Do you expect it at all, given that nitrite additions in the treatments by far exceeded environmental concentrations? And if so, what regions may be most sensitive or prone to N₂O production? Do you expect N₂O production to increase in the light of increasing oxygen minima, which may lead to increased environmental [NO₂-]? Are there industrial applications where this N₂O production needs to be considered (although I do not really expect M. aeruginosa in WWTPs)? Not all these questions need to be answered, but including this line of thought would in my opinion make the manuscript much more accessible to the readership of Biogeosciences.

In addition, I have a few specific comments listed below:

Lines 58/59 and Figure 3 – this is about the only mention (and use) of enzyme kinetics and characteristics. I think that in Biogeosciences, this would either need some more information, or it may be moved to the supplementary material to make room for discussion of environmental consequences. You do not really discuss the kinetics anyway, and I think a supplement would not harm the overall scope of the manuscript. In Figure 3, please indicate vmax and Km.
Typos – please change nmole to nmol (Fig 1), and check for typos, such as numerous brackets opened and not closed, e.g. lines 91, 92

Line 76 – “intracellular No2- was not possible...” Odd wording. Additionally, I am not sure whether it really is “not possible”, given that cyanobacteria may always come up with O2 from somewhere. Please rephrase.

Lines 77 – 81 – I am not sure what the authors want to say here, why is the regulation with regards to light relevant? Especially given that there is so little difference in N2O production? I cannot really see what the (environmental) applications would be.

Lines 94/95 – this is your result, correct? The mix of results and discussion section makes this sometimes hard to distinguish, please clarify.

Line 107 – as a biogeochemist, the allelopathic response is unclear to me. Please add a short explanation/definition.

Lines 108 – 110 – is this hypothesis yours, or can you back it up with references? The reference to further research should be deleted here, this is rather suitable for conclusions.

Line 113 – which groups of microalgae have been found to synthesize N2O? Please specify.

Generally, I think this section 2.4 might be expanded, please see my general comments above.

Conclusions section – as it is, this section is a bit weak and merely a repetition of the abstract. This would be another good location to discuss environmental consequences.