

Biogeosciences Discuss., community comment CC1
<https://doi.org/10.5194/bg-2021-312-CC1>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on bg-2021-312

Zuzi Koscikova

Community comment on "Investigating the effect of nickel concentration on phytoplankton growth to assess potential side-effects of ocean alkalinity enhancement" by Jiaying Abby Guo et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-312-CC1>, 2022

The review of the paper "**Investigating the effect of nickel concentration on phytoplankton growth to inform the assessment of ocean alkalinity enhancement**" by Guo, J.A. *et al.* was done in the cooperation of 5 students of Ecological and Environmental science Guillemette Gandon, Mathis Gillio, Lubin Grosbuis, Zuzi Koscikova, and Louise Litrico.

General comments

The paper has an interesting topic, clearly identifying the knowledge gap and highlighting the importance of the study.

In the *Introduction*, a bit more clarification could be added to how increased ocean alkalinity can increase ocean capacity to store more CO₂ (Line 40).

Additionally, there is no smooth transition to the questions – there could be a connection between the mentioned two functions of Ni (L64-70) and the research questions (L77-79). We suggest an additional explanation of how these functions can affect your hypotheses.

Similarly, a deeper explanation of why seawater from 15 m from the Southern Ocean was chosen could be included in the *Introduction* or beginning of *Materials and Methods* to improve the accessibility to a broader readership.

The paper is missing an explanation for the choice of phytoplankton species used as well as unequal representation of functional groups (3, 4, 3, and 1 for each group respectively). Could you elaborate on this?

We would recommend restructuring some parts of the *Materials and Methods* section and bringing essential explanations at the beginning, such as explaining EDTA usage and the choice of seawater from the Southern Ocean from 15 m deep.

Similarly, limitations could be more fully discussed in the *Discussion* section, such as using an average medium of all ocean compositions to portray the variation in nickel's effect on each species (mentioned in 2.1 part) or unpredicted light cycle (L109-114).

Although the figures (Figure 3 and 4) are straightforward, it would be reasonable, and probably improve clarity, to group them into 4 phytoplankton groups. Original figures

could be still presented in *Appendix*.

We would also recommend developing more on the species-specific response as it is one of the paper's main findings. More information about studies' observation for each species and its implications can help.

Minor comments:

Line 48: Can you please clarify "appropriate scale"?

Line 69-60: Please, can you explain how the increase of Ni concentration with depth affects phytoplankton?

Line 89-91: Please provide the chemical composition of Aquil medium or a link to its definition. (Could be in Appendix).

Line 139: Deeper explanation of EDTA usage should be at the beginning of the *Materials and Methods* section.

Line 184: Please, clarify if it was a standard sample size of phytoplankton samples added to the FRR fluorometry cuvette.

Line 188-193: Could you clarify the choice of channels for the fluorometry? Why did you use 3? A short explanation of why you chose these would be helpful. Similarly, you could clarify how these channels coincide with the photosynthetic pigments across the phytoplankton function groups.

Line 232: "Every strain was able to grow in all Ni concentrations in Aquil media for at least 3 batch cycles" – should be moved to the *Materials and Methods* section.

Line 252: Again, the part about an additional experiment should be moved to the *Materials and Methods* section.

Section 3.3 (Line 304-307): Restructuring the paragraph with a better opening statement would be helpful. The paragraph could start with specific species instead of "Like in Aquil media...". Moreover, additional information about the actual comparison between Aquil media and the natural seawater media is needed to increase the value of the paragraph.

Line 304: "*P. tricornutum* growing in the natural Southern Ocean seawater media..." – this should be explained earlier in the text with the definition of natural Southern Ocean seawater media.

Line 311: Restructuring the statement: "...do not have a strong effect." As it seems there is an effect for some species, we recommend using instead "effect varies for species" and/or "some species are affected while others not."

Line 312, Line 327: Restructuring the statement "only a few" and "most species". Please, use real numbers instead.

Line 310, Line 326: Consistency in terms with the identical meaning: "FRR fluorescence" and "photosynthesis performance".

Line 329-330: It could be helpful to add some more literature to support the statement, as there are only 2 species (*Oscillatoria* sp. and *Synechococcus* sp.) common to both Glass

and Dupont (2017) study and your study.

Line 388: Please, provide more than one reference for the statement "earlier studies".

Line 412: Argument about generalising the findings more widely to natural communities of phytoplankton seems too strong. The paper touches on the issue of different climate regions (in the paper are only phytoplankton species from temperate regions) and community competition very slightly.

Line 414-418: Doesn't Ocean seawater media naturally contain other organic ligands? Couldn't these have been quantified?

Appendix A; Line 456: Please, add some clarification for the calculations statement: "The free ion concentrations were calculated based on the total ion concentrations together with the added concentration"

Thank you for considering these comments.

Citation: <https://doi.org/10.5194/bg-2021-312>