

Biogeosciences Discuss., referee comment RC2
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Comment on bg-2021-146

Anonymous Referee #2

Referee comment on "Manifestations and environmental implications of microbially-induced calcium carbonate precipitation (MICP) by the cyanobacterium *Dolichospermum flosaquae*" by Refat Abdel-Basset et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-146-RC2>, 2021

This paper investigates the capability of cyanobacterium *D. flosaquae* to induce MICP. The results of the study rely on the measurement of the growth parameters, and a few solution chemistry parameters (Ca, pH, TA, NH₃). Based on these datasets, the authors claimed that the *D. flosaquae* induces precipitation of CaCO₃ (calcite). I find that assertion poorly supported by the presented data and paucity of other (necessary) datasets (saturation state, microscopy/spectroscopy observations, abiotic controls) needed to support the findings claimed in the paper. In light of these issues, I find that study is not up to the standards of the journal and will be more appropriate for another journal. Below are my more elaborate comments:

- 1) The introduction lacked a clear motivation for the study. Several statements in the introduction were not properly referenced, which is surprising given that MICP is a very well-studied field.
- 2) Lack of any abiotic control in the experiment design makes it difficult to infer whether the formation of CaCO₃ was due to *D. flosaquae* or merely due to changes in the solution chemistry.
- 3) No information on the saturation state of the solution with respect to carbonate and other minerals is included in the results and discussion.
- 4) The time evolution data on the growth parameters, and dissolved Ca concentrations are not included, which again makes it challenging to fully understand the evolution of the system under different treatments
- 5) In the current design of the incubations, the effect of Ca concentrations and anions cannot be decoupled. This results in confusing interpretations of the Figures.
- 6) Lastly, the authors refer to the possible CaCO₃ phase as calcite, but yet provide no data that confirms whether the carbonate is calcite.

Specific comments:

Line 41: references missing.

Line 52-55: The phrase is difficult to understand at first. Recommended to revise it

Line 56: why is "ate" in carbonate in parentheses?

Line 56: The use of co-precipitation to define CaCO₃ is incorrect. Co-precipitation implies that 2 or more phases precipitating simultaneously. In CaCO₃, it's just one phase.

Line 58: "Calcium and phosphate also coprecipitate..." phrasing unclear. Do Ca-phosphate co-precipitates with CaCO₃ forming a solid solution? Please revise

Line 62: it would be helpful to add some references or some examples of key industrial applications.

Line 64: misuse of word co-precipitation (see comment on Line 56)

Line 76-77: Why is the cyanobacterium *D. flosaquae* chosen for this study? What is its environmental importance? A brief description of the cyanobacterium itself and reference to any previous studies would be helpful to better understand the context of this manuscript.

Line 87: where was the strain procured from? Is it axenic?

Line 90-93: There is no sterile control for the experiment (i.e. BG11 with no cells). This is a crucial problem as the authors are proposing the *D. flosaquae* is able to induce CaCO₃ precipitation.

Line 151-153: The way the incubations are set up, it's not possible to discriminate between the effects of Ca concentrations and the effect of citrate, acetate, or chloride anions, on the growth of *D. flosaquae*. Using perhaps, sodium salts to assess the effect of anions would be a more logical choice.

Line 160: how was pH set?

Line 260-261: I don't agree with the authors that solely based on the alkalinity levels, pH, and Ca concentrations; it could be asserted that *D. flosaquae* induces MICP. These datasets don't eliminate the possibility of the formation of other Ca phases (such as phosphates). A simple saturation index calculation (for e.g using Visual Minteq or PHREEQC) would be easiest to confirm what mineral phases would be over-saturated under the experimental condition. Alternatively, microscopic observation could also confirm with confidence what mineral is actually precipitated.

Line 311: "The high ability of *D. flosaquae* to shift the pH to alkalinity ..." high compared to what? There's no comparison of *D. flosaquae* versus other cyanobacteria inducing extracellular carbonates. It would be useful to clarify how authors calculate or assign the "high ability" to *D. flosaquae*.