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# *Interactive comment on* "Manifestations and environmental implications of microbially-induced calcium carbonate precipitation (MICP) by the cyanobacterium *Dolichospermum flosaquae*" *by* Refat Abdel-Basset et al.

**Anonymous Referee #3**

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I have now carefully gone through the research article "Manifestations and environmental implications of microbially-induced calcium carbonate precipitation (MICP) by the cyanobacterium Dolichospermum flosaquae" authored by Refat Abdel-Basset et al. (MS No.: bg-2020-378), and so is in a position to make the following comments.

The work investigates whether the temperate cyanobacterium Dolichospermum flosaquae can induce calcium carbonate precipitation; if yes, then to what extent and under what conditions. According to the authors, microbe-induced calcium carbonate precipitation controls the availability of calcium, carbon and phosphorus in freshwater



lakes and simultaneously controls carbon exchange with the atmosphere; therefore, this topic of research and the information generated have considerable significance in biogeosciences. Technically, the work has been executed by following appropriate methods and practices, and the veracity of the data presented is also quite satisfactory. However, the study has certain structural and designing-level weaknesses which need to be critically addressed before the paper can qualify as a sound geomicrobiological research work.

The term "microbe-induced", as it is used for the calcium carbonate precipitation phenomenon, indicates that the phenomenon also occurs in the absence of microbes. Several studies have highlighted microbes-independent calcite precipitation in the context of mountain springs, cave waters, hot springs and other fresh water aquatic systems. The debate on the cause and effect relationships between live microorganisms, precipitation and petrifaction is long. I think the jury is still out on whether live microbes precipitate more calcite than any other non-living micro-particulate matrix present in the aquatic system in question, and if so then should the self-inflicted burial of the causal organisms not be the limiting factor of further precipitation/mineralization within the system. In view of these issues the ecological/geomicrobiological significance of the data obtained of the present study (i.e., the scale of biomineralization rendered by the test organism Dolichospermum flosaquae) should be evaluated in relation to the scale of mineralization that is observed under abiotic conditions. Towards this end, a proper review of literature should be presented and appropriate abiotic control experiments conducted involving non-living micro-particulate matrices for calcite precipitation under various physicochemical conditions.

In relation to the choice of the test organism the present manuscript provides no rationale (the source of isolation or procurement of the cyanobacterial strain used in the present study is also not mentioned in the manuscript), on top of which we do not also get to know whether the extent of precipitation observed is high or low vis a vis precipitation levels reported previously for other cyanobacteria, fungi or bacteria, or for that the extents/rates of calcite precipitation observed over time in temperate lakes across geographies.

My other specific comments are given in the marked-up PDF file of the manuscript.

Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2020-378/bg-2020-378-RC3-supplement.pdf

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