

Biogeosciences Discuss., referee comment RC1
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Comment on bg-2021-235

Ola Kwiecien (Referee)

Referee comment on "A modern snapshot of the isotopic composition of lacustrine biogenic carbonates – records of seasonal water temperature variability" by Inga Labuhn et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-235-RC1>, 2022

Dear Authors,

I have very much enjoyed reading your work. The contribution which looks back on what material we actually analyse, and on what are the inherent sample limitations is valuable and timely, in particularly now, when technological advances allow for more precise and more sophisticated measurements. The paper is informative and generally well written, and Biogeosciences is a most adequate venue for this work.

I have several general minor-to-moderate comments which (I hope) will improve the readability and the reception of the manuscript.

The language – please try to be as specific and consistent as possible. Dealing with isotopes and environmental controls, the vocabulary can be daunting, especially for less familiar readers. Please, when talking about 'precipitation' note each time if you refer to atmospheric (rainfall) or carbonate precipitation. Also, perhaps it is worth to explain once and upfront (but not as in the present version in the abstract) all the environmental factors influencing isotopic composition of carbonates and their direction. As of yet, provided explanation is correct but condensed to two long and complex sentences in the abstract. Again, please keep in mind readers less familiar with principles of stable isotope geochemistry and shrieking when 'fractionation' is mentioned. The fact that oxygen isotope fractionation is temperature-dependant, but the process happens (1) in the atmosphere and (2) in the ambient water, and drives the isotopic composition of water/carbonate in two different directions is probably best explained using a simple sketch? I do agree that a picture is worth a thousand words, and in this case a well-designed but simple figure could improve the clarification of processes influencing $\delta^{18}\text{O}$ in lacustrine carbonates. Such figure would be a great asset in the introduction. Shall you decide to leave out the sketch option, please explain the processes consequently starting with atmospheric temperature effect on rainfall oxygen composition and lake water composition (additionally through evaporation) and only then move to ambient water temperature influence on carbonate precipitation (modified by vital offsets).

In the chapter 'Material and methods' the 'material' is actually not described. An SEM image of *Candona*, an SEM or macro image of *Chara* elements and perhaps a macro image of *Pisidium* would be a good addition. Also, I would welcome a sketch of *Chara* components (branchlet and internode) as I am familiar mostly with oospores and it took me a while to understand what you refer to as 'encrustation'.

Field sampling. I wish to see a more detailed information on field sampling. How do one take a less than 1 cm surface sediment (with a small shovel) from a water depth of more than 1 m? I imagine that one needs to employ a diver? How was the water sampling in 2013 and 2014 done? With Niskin Bottles? How was the *Chara* sampled? I see no justification for sampling Lake Blaktjärnen – its *Chara* results are not well incorporated into the rest of the paper. Please, if you want to keep them make sure that the reader knows why they are relevant and how they fit into the general picture.

I feel awkward promoting my own work, but you may want to refer to the papers by McCormack et al., 2018 and McCormack & Kwiecien 2021; the most recent component-specific studies of lacustrine carbonates. While Lake Van setting and chemistry are very different from the lakes you are working with, these papers highlight the suboptimal suitability of bulk carbonate samples for paleoenvironmental reconstruction and elucidate which factors can compromise the bulk signal.

I really like that the conclusions loop back to the relevant goals listed in the introduction. Having said that I find the conclusion misleadingly presented. I agree that differences in vital offset -corrected d18O values of different carbonate components suggest different periods of formation and might point to the amplitude of seasonal temperature contrasts. This holds true only if several components are extracted from the same sedimentary layer and their isotopic composition is compared and contrasted (conclusion 1). However, this information is interwoven with influences of lake water d18O and temperature. By the time the reader reaches conclusion 2, the essential notion of comparison is already forgotten, and it reads like any seasonal change in water temperature is clearly reflected in d18O of any biogenic carbonate, and I cannot agree with this statement. The order of arguments provided in conclusion 2 does not strengthen it either. Please, streamline the arguments towards the conclusion, not away from it. Again, a well-designed sketch in the introduction, could help in making this conclusion more succinct. Conclusion 3, while correct, is very loosely formulated and, in its present form reiterates the findings of McCormack & Kwiecien 2021. Your work deals with a more complex example and is the first such comprehensive attempt of comparing carbonate components from shallow water, above the thermocline of an open lake (as explained in conclusion 2). I think that focusing conclusions on this particular case and making them more specific will be very beneficial.

Specific comments:

Abstract

Line 4: 'lake water and water temperature'

Lines 21-25: this info is correct but as a 'textbook knowledge' is unsuitable for the abstract

Introduction

Line 36: 'depending on the local context'

Line 40: remains of lacustrine organisms

Lines 40-45: open lakes are more prone to calcite than aragonite precipitation, but carbonate mineralogy also plays a role in bulk carbonate d18O composition. Please, check McCormack et al., 2018

Line 76: their - whose?

Material and methods

Line 177-178: were the valves visually checked for organic matter remains? Was the potential organic matter left intact?

Line 184-185: valves? I was under impression that gastropods have shells and operculum but not valves

Results

Lines 235-239: this is interpretation, not result

Line 268: 'surface sediment' is misleading if it refers only to encrustations collected from

the surface but not to the bulk surface sediment

Lines 267-275: information provided here is correct, but it is not a result

Line 286: and what about autochthonous carbonates? Can you exclude/ discuss their presence?

Lines 308-313: information provided here is correct, but it is not a result

Lines 326-329: information provided here is correct, but it is not a result

Line 326: exobiotic mentioned for the first time without explanation

Discussion

Lines 344-346: correct information but should be better explained in the introduction (see general comments)

Lines 449-453: without clear reference to a figure, I cannot see how your results demonstrate these two points. Also, the points are very vague - what do you mean by 'sufficiently large'? How do you know or how can you test what is an 'representative average'? Please, try to rethink this argument.

Figures

All figures are informative but with small adjustment they could convey the message more efficiently.

Fig. 2: Please, indicate clearly 8 m water depth mark (the deepest sampling point). If the grid is necessary in the figures, please, align the legend within the grid boxes. Also, please put the data points in the foreground not in the background. The present effect is visually unsettling.

Fig. 3: Please, align the legend within the grid boxes. I am not sure if the symbols in the upper left and right corner of figure 3a are intended?

Fig. 4: Please, make the data points in panel 4a larger, they are barely visible. Similarly, the triangles in panel 4b

Fig. 5: Please, unify the scales in fig a and b (panel b is visibly horizontally stretched, although the range of the values is the same) also the ticks on the d13C axis are suboptimally distributed, if taking the grid into consideration (with the grid values at -10, -8.75, -7.5 and so on).

Fig. 6: Please unify the scales in fig a and b (panel b is visibly horizontally stretched although the range of the values is the same). The legend is a bit confusing; it took me a while to figure out what am I looking at. 'Sediment sample' even if explained in the legend is misleading, why not calling it 'dead fragments' or 'subfossil fragments'?

Fig. 7: The grid is distracting. If the authors want to keep the grid why not stopping at full intervals (e.g.: -7.5, -2.5 for d13C and -8, -4 for d18O) rather than cutting it of randomly?

Fig. 8: What are exactly 'dead' and 'living' samples? Are the fragments of encrustation described as 'sediment sample' in the legend of figure 6 considered 'dead'? Please, define the term and use it consistently.

Fig. 9: The same comment as above about the grid

Table 1: The species, instar and the no. samples are the same for both panels, I suggest merging them into one. For the consistency, I would suggest adding all data presented in figure 7 (including 'fine calcite', 'fragmented encrustation from surface sediments' and *Chara* samples from Lake Blaktjärnen). Please, also indicate if these are measured or vital offset -corrected data. Last comment here - please try to keep the terminology consistent throughout the main text, figures and figure captions and the table.

To wrap up, I think this is a really valuable contribution showing pitfalls of using single carbonate component and highlighting the interpretational difficulties but, also benefits of multi-component analyses, and I very much wish to see it published. I hope that authors will find my feedback helpful.

Best wishes, Ola Kwiecien