

Interactive comment on “Vivianite formation in ferruginous sediments from Lake Towuti, Indonesia” by Aurèle Vuillemin et al.

Anonymous Referee #2

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A review to the manuscript by Vuillemin et al.

In general, it is a nice research from this ICDP project team, and I appreciate the hard work on 100 m of sediment. The topic of vivianite formation in lake sediments is also important and thus fits BG. The authors examined the formation processes in a ferruginous lake, and there are some nice findings. I have, however, some critical comments that should be addressed before this paper can be considered for BG. If the authors cannot address these comments, I would suggest publishing the paper in more specific mineralogical or paleo oriented journal:

Major comments:

The authors should clarify and emphasize their findings. For example, the title is not

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informative and strong enough. Just “vivianite formation in sediments” is not enough for justifying publication in BG, and the authors should present an important finding regarding this formation already in the title. Some combination of the sentence in the end of the introduction with results may be used for the title? Please note the same for the abstract and later on. It is important to show what is novel here beyond previous publications of other groups regarding the formation of vivianite (e.g. Slomp, Paytan, Marz, Kasten and more).

It is hard to judge the vivianite formation in response to paleo conditions, because: 1) There is no quantitative investigation of the vivianite (amounts) 2) There is no context of the layers of vivianite to the redox conditions of the porewater. Where are the other porewater profiles? At least Mn²⁺, sulfate, Methane? Is there any SMTZ? Is the vivianite correlate to any of the redox sensitive elements (besides iron...)? 3) I'm confused with the paleo interpretation (P. 12). When was the vivianite precipitated in 20-50 m? Thousands years ago at the bottom of the lake or now due to current diagenetic processes? Again more data and discussion (as diffusion rough modeling) are needed to support the first option. It is hard thus to suggest any environmental interpretation without the context of current diagenetic processes or quantification of processes. The authors should add the data.

Additional specific comments:

Abstract: The first sentence is not relevant as it refers to ferric iron and phosphate adsorption and not to vivianite. I would write a general sentence instead that states that ferrouginius lakes are important to the phosphorous cycle because of X, Y etc. . . .

Abstract: L 34 is trivial. Add “active” reduction to L 35 to make it also non trivial. It is clear that the redox state is very low in this system to precipitate vivianite, the iron isotopes may suggest its active reduction in this zone. Be careful also with stating it is microbial reduction, as the isotope composition can be light also with abiotic reduction.

Introduction: I do not agree that vivianite is not a studied mineral in sediments, please

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correct.

Methods: P. 5: Can the DIC calculated indeed by this approach? How can the authors be sure the alkalinity is mostly carbonatic in this organic rich sediments? Have they measured the carbonate alkalinity or the total alkalinity?

Methods: P. 7: I do not understand how the authors know that they isolate vivanite for the isotope measurement. Please clarify, also in consideration to the fact that diagenetic minerals are sometime more reactive to dissolution than detritus ones (look at Henkel's publications).

Discussion: P. 12, see also above. More data is needed and calculations to support precipitation of vivanite at the last glacial.

Discussion P. 13: Again, also abiotic reduction can result in 2 ‰ fractionation.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2019-426>, 2019.

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