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Biogeosciences Discussions

Interactive comment on "Biogeochemistry of manganese in Lake Matano, Indonesia" *by* C. Jones et al.

Anonymous Referee #2

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GENERAL COMMENTS

Jones et al. present a study of the biogeochemistry of manganese in Lake Matano. What makes this study exceptionally important is the ferruginous nature of the lake; it is a unique and valuable analog of Precambrian oceans. Hence the findings regarding Mn cycling in the water column presented here have important implications for understanding the origins of sedimentary Mn deposits. The work is well done and although the main conclusions are robust, some points concerning the kinetics of Mn oxidation and the enzymes involved are unconvincing or overly speculative given the data. Detailed suggestions and questions are described below in "specific comments". With minor revisions this should be a solid Biogeosciences paper.

SPECIFIC COMMENTS

1. The applicability of Lake Matano for understanding Precambrian biogeochemistry of the oceans is not emphasized early enough or strongly enough. I suggest highlighting this point in abstract (perhaps in the the first few sentences of the abstract).

2. The next to last sentence of the abstract, "...Mn is likely sequestered in these sediments as pseudo kutnahorite" seems bold given that no particulate Mn was detected in deep anoxic waters. If this statement is to remain in the abstract, possible reasons for this observation should be discussed.

Regarding the determination of Mn oxidation rates:

3. The addition of MnCl2 to 40 umol represents a several-fold increase (at least) in concentration of Mn(II) over the natural concentration. This should be acknowledged in the manuscript along with any potential artifacts (e.g. changes in the microbial community, surface chemistry of minerals, and saturation state of Mn(II)-oxidizing enzymes).

4. As acknowledged by the authors, the length of incubation times was extraordinarily long. The first two time points may represent a more accurate measure of the rate, so this may be worth calculating. In addition to possible artifacts of long incubations noted by the authors, another explanation for the slower rates determined by incubations could be saturation of cell surfaces and/or enzymes by Mn oxides, especially with the artificially high Mn(II) concentration.

5. Adsorption of Mn(II) onto Mn oxides needs to be further considered and discussed as a mechanism of Mn(II) removal and in the discussion of Mn phases. Several studies have found that this can account for a significant fraction of dMn removal (e.g. GCA 57:3907-3923). If the authors believe the XANES and/or extraction data suggests otherwise, it should be discussed explicitly.

6. Page 23 L18-20 Characterization of rate constant variation from diverse settings as "marked similarity" was surprising– this seems like a significant range to me. Inclusion of data from deep sea hydrothermal plumes (e.g. Deep Sea Research Part A 37: 1619-

1637) and the Black Sea (Black Sea Oceanography (eds. E. Izdar and J.W. Murray, pp. 173-185) would also be useful.

Other specific comments:

7. I found the rationale and methods of Mn flux calculations to be confusing (page 24 and figure 10). With a broad audience in mind, spelling this out would be useful (e.g. why does Mn reduction equal the sum of upward and downward fluxes?).

8. The significance of the mineralogy results could be expanded through comparison to those from marine systems and bacterial cultures, e.g. GCA 73:6517-6530 and references therein.

9. Pg 23-24: tying Cu concentration to likely enzymatic mechanism is tenuous because of the lack of knowledge concerning Cu demand for multicopper oxidases, Cu acquisition capabilities of Mn(II) oxidizing bacteria, and the physiological function of these enzymes.

10. The authors discussion of Mn(III) in the introduction seems out of place because it is not revisited in results/discussion.

TECHNICAL CORRECTIONS

1. Table 6 appears to reference itself – should be Table 8?

2. Fig 3: recommend showing a detail of the chemocline so that dynamics between 115 to 125 m are more visible.

3. Fig 2 is confusing at first glance – the detail should be linked to the density plot, not the temperature plot.

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