

Interactive
Comment

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

Anonymous Referee #1

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General Comments

In this manuscript Jones et al describe the biogeochemistry of manganese in a stratified ferruginous lake. Because this lake has low sulfate levels, the processes occurring in the lake may mimic those which occurred in the early oceans. The work presented here is fairly straight-forward, and I particularly like the use of high energy X-ray spectroscopy to examine the Mn phases. In many ways I find this to be the most novel aspects of this work, although I don't see any major flaws in the rest of the work. There are a few small to medium size points I would like the authors to address before the work is published. My comments are keyed to lines numbers and pages as (line number, page).

Specific Comments

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1. First I think it is overly simplistic to use the results presented here to suggest Mn oxidation is relatively insensitive to environmental parameters. Table 8 shows a 20–70 fold range in rate constants, which suggests some big differences to me (not the “marked similarity” referred to on line 7, p. 4079). If, for example, you could show that some of these differences in Table 8 are related to the temperatures of the incubations then that might be a different story. Also, it seems a little strange to discuss why your rate constants may be problematic (20, 4078), followed by a discussion of their similarity to other rate constants in the literature (5, 4079).

2. I realize that Fe reduction coupled to methane oxidation is the newest “reaction du jour” in geobiology, but the self promotion here (23–25, 4065) seems unnecessary.

3. What is an “authigenic form of Mn” (4, 4074) and is it similar to/the same as the “authigenic oxides” discussed on p. 4080 (lines 11–12)? This terminology needs to be cleaned up.

4. In discussing the model fits in Fig. 9 (starting on 27,4077) it seems to me that fits 4 and 5 do a pretty good job as compared to the other 3 fits in reproducing the particulate Mn profiles. Also, if lateral input is the primary mode of Mn input to the lake, doesn’t that invalidate the 1-D modeling you subsequently discuss?

5. I would say “Mn is oxidized” rather than “Mn oxidizes” (25, 4081).

6. I would say “must have originally been deposited”, i.e., add the word “been” (26, 4081).

7. More details are needed on how the fluxes such as in Eq. 1 were calculated (e.g., what depth intervals were used in the calculations, and how exactly was dC/dz calculated). The same problem exists for the fluxes listed in Table 7.

8. What exactly are you referring to on (5,4082) when you talk about “overlying waters”? Is this the same as the “bottom waters” you refer to 3 lines down?

9. If the bottom waters are anoxic then where are these large quantities of Mn oxides

(13, 4082) being deposited?

8. In your alternate explanation (22, 4082) where does all this Mn(II) come from? Also, I'm sure I'm missing something but how is this scenario different than the one outlined above?

9. Fig. 8 – What depth interval is being plotted in the right panel.

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