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Comment

Interactive comment on “Redox regime shifts in microbially-mediated biogeochemical cycles” by T. Bush et al.

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This paper presents a general mathematical model for microbial catalyzed biogeochemical cycles. The model is used to investigate changes in the redox state of the system, which the authors refer to as a “regime shift”. This work is significant because, as the authors point out, there is evidence that such shifts in redox state have occurred in the past, and understanding such shifts is important for predicting how biogeochemical cycles will respond to environmental change (e.g. climate change). The authors present several models of increasing complexity and in all cases show that redox regime shifts have the potential to occur and that these shifts are an inherent feature in the model topology. They then set out conditions that must be fulfilled for such a regime shift to occur and follow this by a discussion as to where and when in

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nature these conditions are likely to be fulfilled.

Overall I thought this was a very good paper. The work was rigorous and presented clearly. I particularly liked how the authors first demonstrate their main conclusions with a very simple model and then relax their assumptions to present more realistic versions of the model. This strategy presents the information in a way that should be easily understandable for readers who may not be as comfortable with the underlying mathematical details. In general I would recommend this paper for publication with only a few comments that the authors may wish to consider.

In the Discussion section, the authors mention cases in Earth's history when such regime shifts may have occurred, such as the rise of an oxygen atmosphere, iron cycling in the Archean-Proterozoic ocean and the rise of sulfide levels contributing to mass extinction events. I would consider putting more emphasis on this aspect earlier in the paper. I think more discussion of these events in the Introduction would give the readers a greater appreciation for the importance of the work. Right now this discussion feels sort of "tacked on", when in reality it is quite significant to the model results. Also I think it would be interesting to see a model simulation parameterized to investigate one of these events, but perhaps this would be too much for the current manuscript.

My second point is with regard to Section 7.2. In this section the authors investigate whether the conditions for regime shifts are met in the natural environment. In particular they investigate the condition that the oxidizer/reducer growth rate must be saturated with respect to s_r or s_o (i.e. $s_{tot} \gg K_{or}, K_{ro}$). They state that this can be true for sulfur and nitrogen cycling microbes. I agree that this is true for sulfur cyclers, but I am not convinced that this is true for nitrogen cyclers, as nitrogen is often a key limiting nutrient in many biological systems. For example in the marine environment NO_3^- varies from near zero in the surface ocean to $30 \mu\text{M}$ in the deep ocean, this is of the same order of magnitude as their half saturation constants in Table 2. I think some more discussion is warranted with regards to this point.

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