

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

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This manuscript asks the simple question : can we understand and explain rapid transitions, i.e., regime shifts in biogeochemical systems. We know from the geochemical record that such rapid transitions most likely have taken place in the past, e.g. rapid (on a geological time scale at least) switches in the chemistry of the ocean, moving from a ferruginous ocean to a sulphidic ocean. Accordingly, the question of such rapid regime shifts and their causes is very pertinent to the field of biogeochemistry.

The authors propose here a mathematical model mechanism for biogeochemical regime shifts, which is intriguingly simple. Note that “simple” should be understood with a positive meaning , so not as simplistic, but rather as elegant and purified, i.e., capturing the essence of what is going on. The authors do a good job of stripping com-

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plex biogeochemical reality, so one is only left with a few simple differential equations (Eqns 1-3 in the paper) that represent the essence of a redox cycle. At the same time, the authors do a considerable effort to link and translate their simple model in terms of actual biogeochemical processes.

Although this theoretical paper is a bit outside the “normal” territory of Biogeosciences, this is not a problem, and I fully endorse its publication as it enriches the scope of Biogeosciences. The paper is very carefully prepared and well written. It avoids mathematical jargon (which can be a pitfall in these papers) and the lengthy derivations are included in the supplemental material, which keeps the text very readable. The main messages of the paper are well articulated upfront, and the model concepts are always connected to reality. This is done by examining a number of extensions and variants of the simple model, which simulate more realistic biogeochemical situations. Whether the mechanism at hand really explains the rapid geochemical transformations in the ocean as observed in the paleorecord remains to be seen, but at least this paper proposes a thought provoking idea.

I have only a few suggestions:

The crucial parameters that are varied in the model analysis are the intrinsic growth parameters v_{or} and v_{ro} (Figure 1). Yet implicitly, v_{or} is assumed to scale with the electron donor concentration (acetate) and v_{ro} scales with the electron acceptor concentration (O_2). It’s much easier to understand what happens in the model, if this dependency is made explicit. In a model world where there a lot of acetate (v_{or} large) one expect the system to go to an reduced state ($s_o \ll s_r$). In contrast, in a model world where there is a lot of oxygen available (v_{ro} large) one expect the system to go to an oxidized state ($s_o \ll s_r$). Accordingly, one may well $[O_2]$ or $[acetate]$ on the x-axis in figure 1.

Furthermore, my feeling is also that the behaviour of the model in Figure 1 is governed by the ratio of $[O_2]$ over $[acetate]$ (or equally by the v_{or}/v_{ro} ratio). So the model

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behavior is governed by one parameter rather than two independent parameters (i.e. Fig 1a and Fig 1c essentially provide the same info).

Another geochemically relevant case would be the a model which has a fixed input of the electron acceptor (F_{O_2}), and a fixed input of the electron donor (F_{Ac}). So the differential equations for oxygen and acetate would look like $do(t)/dt = F_{O_2} - R_{ro}$ and $da(t)/dt = F_{Ac} - R_{or}$ or My gut feeling is that this model would predict a reduced system state if $F_{Ac} > F_{O_2}$, and an oxidized system state if $F_{Ac} < F_{O_2}$. Clearly the model is of an intermediate complexity, in between the simple model in Section 2 and the more complex model in Section 5.

Minor comments (1) Integrate the footnotes into the main text (2) The term “topology” is used throughout the text, without clarifying it (for the non-mathematicians, which is a sizable part of the audience). (3) P3 L65. acetate (4) Table 2: $SO_4 \rightarrow H_2S$. Hans Roy and colleagues have recently published an article on the Monod constants of sulfate reduction. Concurrent low- and high-affinity sulfate reduction kinetics in marine sediment. / Tarpgaard, Irene Harder; Røy, Hans; Jørgensen, Bo Barker. *Geochimica et Cosmochimica Acta*, Vol. 75, 2011, p. 2997-3010.

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