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Biogeosciences Discussions

Interactive comment on "A geochemical modelling study of the evolution of the chemical composition of seawater linked to a global glaciation: implications for life sustainability" *by* G. Le Hir et al.

Anonymous Referee #1

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Comments on "A geochemical modelling study of the evolution of the chemical composition of seawater linked to a global glaciation: implications for life sustainability" by Hir et al.

This manuscript provides an interesting perspective on ocean chemistry during and after "snowball" earth events not found in other modeling efforts. In particular, the conclusions with respect to pH changes were very interesting, and the inclusion of hydrothermal reactions with oceanic crust are novel. I have the benefit of writing this review after having read the manuscript as well as two other comments submitted in response to the manuscript, so my comments here were influenced by the two available reviews in addition to the manuscript itself.

Modeling is useful because it allows us to create simplified model systems of the world, tweak the parameters, and see what happens. From a non-modeler's perspective, models are particularly useful when we can learn something unexpected or interesting about a system that helps us interpret the known data. However, models are in and of themselves not data, per se. They can help us interpret data, but they are not data (would you argue otherwise? As an interactive review, I would appreciate your perspective on this). As a general comment, I felt like the paper was written as if the models were facts. I would recommend shifting the focus so that the models could then be used to help interpret the known record better than presented here.

p. 1841: Line 1: "Oceans covered more than 75

Line 11: Hoffman and Schrag: while I agree this is a good overview paper, perhaps the reference should read "Hoffman and Schrag (2002) and references therein" in deference to the vast literature on the subject of Neoproterozoic glaciation. Or better yet, cite some of the original literature in addition to Hoffman and Schrag.

Line 12: "Indeed the shift towards negative values recorded in those carbonates (Hoffman et al., 1998) is often considered as a biological, suggesting that the Precambrian life was deeply affected by a large environmental perturbation. "

Or, as hypothesized by others, it could be related to a release of methane and not related to productivity at all.

Line 19: I am flattered by the citation (Corsetti et al., 2006)–this was for the most part a review paper, so credit should be given where it is due (same comment as above–add, "and references therein"). In particular, the acritarch work of Grey et al. (Geology, 2003) should be cited with respect to the Marinoan biotic record. The point of this paper was to challenge the idea that a biotic crisis should be expected with a snowball earth, so in my mind no "paradox" exists between the geologic and the biologic evidence. Note also that red algae are known from long before the glaciation (1.2 Ga, papers by Butterfield) and certainly exist today, so the concept that everything that survived was "simple" is not tenable.

p. 1842: The initial assumptions appear reasonable. Note that Olcott et al. (Science, 2005) used biomarkers to reveal thin or no ice at a section in Brazil during Neoproterozoic glaciation, which would fit with the ocean-atm exchange hypothesized in the first set of models and could be perhaps used as a constraint.

I echo one of the other reviewer's comments that there is evidence for a hydrologic cycle during the glaciation. Also, see the paper on chemical weathering during glaciation by Rieu et al (Geology, 2007). In light of these, and other, papers, the concept of a hydrologic shutdown with no continental weathering input is probably not tenable. The model runs should be modified to fit the new constraints–I do not know how difficult this would be, but given the fact that the model has a continental input term, it would be reasonable to add input during the glaciation and see what happens. The fact that all the models assume shutdown of the hydrologic cycle could be viewed as a serious issue for this manuscript and needs to be considered.

p. 1846: "The inception of the global glaciation is simulated through an instantaneously shut down of all continental weathering fluxes as a consequence of the rapid growth of continental ice sheets." Does your model actually grow continental ice sheets, or do you merely shut off continental weathering? If the latter, here is an example where interpretation is creeping into the "observations" section. Also, Marinoan in line 19 of the same page is spelled incorrectly.

p. 1847: What exactly is a "quasi absence of a vertical gradient"? Also, the model has an interesting decrease in atm O2 by 30

p. 1848: What is "quasi-anoxic"? Is it, or is it not, anoxic?

?Indeed the precipitation of iron formation (BIF) requires three conditions to be verified: (1) deep ocean anoxia, (2) low sulphur availability, and (3) surface-water oxygenated.? The latter need not be concurrent?many papers assume the first two occur, followed by the last. What is your implication by listing all three in the same sentence?

p. 1850: I concur with a previous reviewer that many Neoproterozoic diamictites contain abundant carbonate debris, including in some cases a fine grained carbonate matrix, which one would assume is even more susceptible to acidification than large blocks. Thus, it is clear that carbonate dissolution did not, in fact, go to completion. Therefore, I wonder if the model results with respect to carbonate dissolution are reasonable?

Can we really interpret the appearance of a geological contact to infer temporal meaning? Just because a contact appears "knife sharp" does not really mean much in terms of timing, without some independent knowledge of the time involved across the contact. In my opinion, all the knife sharp contact means is that the system changed from a glacially dominated depositional system to a carbonate dominated one–the time in between this shift is unclear.

p. 1851: "Carbonates immediately capping the glacial deposits are supposed to be formed by the rapid continental carbonate weathering in the direct aftermath of the snowball"– This statement is based on the classic snowball earth hypothesis of Hoffman et al (1998). There are other hypotheses out there to explain cap carbonates. Also, the reference should be Shields, 2005.

P.1854: The model run assumed a glaciation of 30 million years. Post-sturtian, but pre-Marinoan units from Australia were recently dated by Kendall et al (Geology 2006) at 643 Ma, which is only 8 million years before the termination of the Marinoan event, suggesting that the actual duration of the glaciation was even less. Therefore, the authors should consider model runs with a much shorter glacial duration (5 million years or less, for example). This is particularly pertinent for figure 10 on page 1875–

the productivity decline is drastically different, and less severe, if the glacial duration is only 5 million years. Since the biotic response is in the title of the paper, this is an important issue to consider. Jacobsen and Kaufman (1999) also used Sr isotopes to argue for a shorter glacial duration.

p. 1858: the discussion on dolomite versus calcite is interesting, but given the fact that we still do not understand dolomite ppt in the modern, I would consider cutting it.

General comments: I want to strongly echo the comment of reviewer Ridgwell: is this manuscript really about life or rather about modeling syn- and post-glacial geochemical conditions? I suspect the latter. Other than pH, the "life" aspect is not well developed, and given the available data, really cannot be developed further within the scope of this manuscript. The life aspect seems like an add-on, in some respects, rather than the focus. With respect to pH, Fraiser and Bottjer (2006) and Payne et al (2007, GSA Bul) recently suggested an acidic ocean crisis for the Permp-Triassic extinction, where the oceans fell below carbonate saturation–yes, there was a major extinction, but many complex eukaryotes survived, so even the pH angle may not have been as detrimental as many may postulate.

The reference list could use some attention. The authors seem to miss some recent papers that are important for their modeling (Kendall et al., for example, on snowball duration, and references on the hydrologic and weathering cycle during Neoproterozoic glaciation), and rely on review papers (Hoffman and Schrag, for example) rather than primary citation of the literature.

Comments on Comments from the other reviewers:

Anonymous Reviewer noted that the work of Yin et al. (2007) indicate "evidence for progressive (eukaryotic) biological evolution in the aftermath of the Marinoan glaciation, particularly from the Doushantuo Formation in China (Yin et al., 2007 and references therein)", and suggested that the paper should be recast in light of this. Unfortunately, there is no evidence for a connection between the progressive evolution and snowball

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earth. Evolution progresses–but it is nearly impossible to demonstrate cause and effect, especially when paleoenvironmental issues are superimposed on the record (for example, the cherts where Yin et al. found the fossils simply do not occur in the glacial diamicites). Furthermore, Bailey et al. (Nature, 2007) interpret some of the fossils in the Doushantuo differently, which the reviewer failed to mention.

Interactive comment on Biogeosciences Discuss., 4, 1839, 2007.

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Interactive Comment