

Interactive comment on “Rapid recovery of Ediacaran oceans in the aftermath of the Marinoan glaciation” by Anthony Dosseto et al.

Anonymous Referee #1

Received and published: 8 January 2019

The new Li elemental and isotope data is presented for two suites of important past dolostone archives of the Marinoan glaciation/deglaciation (what are the absolute ages of these units anyway?). The manuscript is organized in a logical order although there is a number of figures which could be effectively combined together (Figs 3a,b and Fig 4 could be one two-panel figure, Fig 5 could be reduced to a single panel, and perhaps some of the later figures with models). Then, the difference could be immediately apparent to the reader. I see 12 figures simply as an over-shoot but other than that, the data and interpretation are on a largely sound and reasonable end. What is more serious, and I am sure some of the authors know this fairly well by themselves, the manuscript heavily relies on the results of a study from the same group (Taylor et al. 2018), seemingly submitted to the Earth Surface Dynamics journal in 2018 and stated

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as ‘pre-print’ in the reference list). At least, this is the impression from the manuscript but until today (January 8, 2019), I have not seen this work in that journal nor I obtained any information about the progress of that work. Taking as granted it had happened in 2018, this is a truly sub-optimal behavior, which would normally lead to instantaneous rejection of the manuscript for formal reasons. In this respect, I urge the authors to finish the other experimental manuscript and let it through reviews before this manuscript is accepted. I leave this decision to the Editor-in-Chief. There are some other major issues. For example, the authors discuss diagenetic processes to have affected Doushantuo Fm. samples (L260-263). Diagenesis in carbonates is known to shift $\delta^7\text{Li}$ values towards heavier end (see Scholz et al. GCA 2010, You et al. Geophys Res Lett 2003, Ullmann et al. GCA 2013). This could result in modification of authors’ observations. Alternatively, they introduce mixing between freshwater and seawater (L263-264). In the latter case, this should be evidenced by some of the key seawater elemental and/or isotope ratios for seawater addition. Considering a key role of these samples suites, I am sure there is a multitude of other elemental data for the samples which could be used. Combined with this, there is some contrast between two places here (L260-263 versus L271). So, either diagenesis had some effect, even if minor, as stated earlier, or it did not have any effect, as suggested later. This should be clarified. To justify dolostones as direct monitors of seawater composition (L289-290) it would require experimental investigation of Li isotope fractionation between a range of natural carbonate chemistry (pure CaCO_3 , CaMgCO_3 , MgCO_3 etc.) and solutions which is yet missing but would be greatly welcome, considering a possible impact to paleoclimate studies. Therefore, the statement here should be sized down. This is linked to L298 where carbonates are discussed but phase or chemistry of those carbonates is missing. These were likely not dolostones. Please clarify. The serious issue of missing reference and work of Taylor et al. has been discussed above and, therefore, L303 and further, may receive little check in the absence of that paper. Whether the relationship presented in Eq. 1 is true and correct, cannot be evaluated at present. Also, the calculated fractionation factors (L320-321) are burdened with huge uncertainties, so I am not

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sure if this has any meaning and if those uncertainties were considered in further calculations. For example, 0.974 ± 0.241 can easily turn into alpha greater unity and the conclusions would then require a complete re-write. On L325-328, the authors discuss rapid evolution towards modern $\delta^7\text{Li}$ values. What are the lines of evidence for this statement? I do not dispute this but some solid piece of evidence should be provided. Residence time of Li is quite long (variably estimated at 1.2-3 Myr), so 'rapid development' may be quite a prolonged period indeed. This is broadly linked with further discussion (L345-346) where constant Li isotope fractionation between sediments and seawater is stated. This is dependent on lithology, in first case. Can we assume that this was similar in Neoproterozoic? Also, was weathering similar before or during onset of Marinoan glaciation similar to modern era? (L348-349) If not, then seawater from that era could be completely different in terms of Li systematics, considering contemporaneous lithological diversity on continents, weathering processes and continental runoff to ocean. Are then the modeled required riverine fluxes real at all? (L371, L374) Is there any other chemical or isotope support, even if from different part of the world? The comparison with modern fluxes (Huh et al. GCA 1998 etc.) may be less straightforward due to landscape and lithology development over time (see also L427-428 where this is discussed; in fact, I am not sure why new rivers would preferentially drain landscapes similar to modern high latitudes. It is the lithology that drives $\delta^7\text{Li}$ – Kisakurek et al. Chem Geol 2004, for example). Moreover, majority of modern Icelandic rivers has $\delta^7\text{Li}$ at ca. $\sim 20\%$ of course with a large range of values. See a summary figure in Tomascak et al. 2016 volume. The oceanic water cycle is assumed to be kept largely similar even during glaciation (L381). I am not sure this is a reasonable expectation. It broadly relates to ocean stratification for one of the models (L406-408) but mixing time of Li is rather fast, on order of ~ 1 kyr (Misra and Froelich, Science 2012) and such high-resolution data are not available for cap dolostone formation.

Below, some typos and minor issues are listed:

L103: greenhouse stage, not state L134 and 393: several hundred thousand years,

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not 100's of kyr L139: ...this DURATION is significantly shorter than THAT proposed by Condon... L163: ...were used FOR 18O16O... L165: is $\delta^{18}\text{O}$ normally expressed relative to NBS19 calcite? Can you rather report it recalculated to V-SMOW? L165: What are the internal errors? 1SD? 2SD? 1SE? 2SE? L166: The same for external REPRODUCIBILITY (not errors) L176: why the bulk and/or silicate fractions were not analyzed? L189: which element(s) was (were) used as internal monitor(s)? L185-196: Flesch et al. have introduced L-SVEC, not IRMM-016. L200: Carignan et al. have not introduced normalization to L-SVEC using MC-ICPMS. The first such measurements were published by Tomascak et al. (Chem Geol 1999). L221-226: this belongs into Discussion, not into Results section. L229: delete (expressed as $\delta^7\text{Li}$). If you want to define this, do it in Section 3.3. L260: avoid using r^2 values, anyway this low value of 0.47 is useless and does tell little about any correlation L284-285: is this viable considering a gentle dissolution technique for selective carbonate fraction? It would be hard to believe incomplete isolation of carbonate fraction as suggested on L332-333. Also, Li would unlikely fractionate even if part of carbonate remained intact. L285: more ABUNDANT, not abundance L292: ...comparing MEAN VALUES for each formation AND excluding... L296: ...probably INDICATING THAT the Li... L316: delete OF in the parenthesis L329: replace depending on with DESPITE since temperature is likely not to play any measurable effect on $\delta^7\text{Li}$, as evidenced in several experimental studies L348: put AND between the two references L364: the MODEL results, not model's L365: replace as with BECAUSE L380: SHUTS L398: delete WOULD L424: replace an with Li

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2018-119>, 2018.

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