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Climate of the Past Discussions

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

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We would like to thank Referee 1 for the constructive comments. Each enquiry is addressed below in the order they appeared in the review.

Referee 1: what are the absolute ages of these units anyway?

Our response: The age of the cap dolostones is ~635 Ma (Jiang et a. 2011, Hoffman et al. 2007)

Referee 1: a number of figures which could be effectively combined together

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Our response: As suggested, figures could be combined together in a revised manuscript.

Referee 1: 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

Our response: We mistakenly referred to the experimental article on Li isotope fractionation in dolomite as submitted to Earth Surface Dynamics. In fact, it was submitted to Climate of the Past (manuscript cp-2018-113) and since it has been accepted for publication (on 11/03/2019).

Referee 1: the authors discuss diagenetic processes to have affected Doushantuo Fm. samples (L260-263). Diagenesis in carbonates is known to shift d7Li 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.

Our response: As indicated by Referee 1, diagenesis could shift $\delta^7 Li$ values. In the original manuscript, we indicated that because diagenesis may have affected the Doushantuo Fm (Formation), the Li isotope compositions for this formation are not included in the model discussion. A revised manuscript could emphasize these points and include the references kindly provided by the referee.

Referee 1: 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.

Our response: The discussion of freshwater and seawater mixing (the *plumewater* model of Shields, 2005) is not intended to be a test of the validity of this model (this was done in other publications); but only to test its incidence on our Li data since it has been proposed as a possible model for cap carbonate formation.

Referee 1: 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.

Our response: There seems to be some confusion about whether diagenesis had some effect or not. As indicated in the original manuscript, we propose it may have had an effect on the Doushantuo Fm but there is no evidence that it is the case for the Nuccaleena Fm.

Referee 1: experimental investigation of Li isotope fractionation between a range of natural carbonate chemistry (pure CaCO3, CaMgCO3, MgCO3 etc.) and solutions which is yet missing but would be greatly welcome, considering a possible impact to paleoclimate studies.

Our response: Experimental investigation of Li isotope fractionation between a range of natural carbonate chemistry (pure $CaCO_3$, $CaMgCO_3$, $MgCO_3$ etc.) has been done for calcium carbonate (Marriott et al. 2004a, b), and results for Ca-Mg and Mg carbonates are presented in Taylor et al. (2019) (cp-2018-113).

Referee 1: phase or chemistry of those carbonates is missing. These were likely not dolostones. Please clarify.

Our response: The rocks studies are indeed dolostones, please refer to Section 2 and references therein in the original manuscript.

Referee 1: calculated fractionation factors (L320-321) are burdened with huge uncertainties

Our response: The concern about the uncertainty on the isotopic fractionation factor is a fair point. It would be more appropriate to report the difference of isotope ratios between mineral and solution ($\Delta^7 Li_{min-sol}$), rather that the isotopic fractionation factor ($\alpha_{min-sol}$). In this case, we can see that seawater temperatures of 10 and 40 ℃ give ($\Delta^7 Li_{min-sol}$) values of -26 $\pm$ 9 ‰and -20 $\pm$ 7 ‰, respectively (thus seawater being between 26 $\pm$ 9 ‰and 20 $\pm$ 7 ‰higher than dolomite). These values are also within error of each other, and prevents the issue of an isotopic fractionation factor becoming greater than 1 (and thus an opposite isotopic fractionation).

Referee 1: On L325-328, the authors discuss rapid evolution towards modern d7Li 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.

Our response: The lines of evidence for a rapid evolution towards modern $\delta^7 Li$ values are the data themselves and the previously proposed duration for cap carbonates deposition (Condon et al., 2005). Considering the latter, the increase in $\delta^7 Li$ values at the base of the formation towards modern values would suggest this change occurs in as little as 0.1 Myr.

Referee 1: 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?

Our response: It is of course difficult to verify whether the Li isotope fractionation between sediments and seawater was constant during the Neoproterozoic. The

same applies to studies of Phanerozoic carbonates, where such constant isotopic fractionation could not be verified but yet was assumed (Lechler et al. 2015, Pogge von Strandmann et al. 2013, 2017).

Referee 1: was weathering similar before or during onset of Marinoan glaciation similar to modern era?

Our response: we are indeed unable to test whether weathering was similar before or during onset of Marinoan glaciation similar to modern era. What the data and model show is that shortly after the onset of cap carbonate deposition, the seawater $\delta^7 Li$ returns to values similar to that pre-Marinoan glaciation. The narrative of the manuscript could be changed to reflect this cautious analysis, without impacting the importance of the findings.

Referee 1: 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?

Our response: As indicated in the original manuscript, Kasemann et al. (2005) have also studied the Nuccaleena Fm and showed that Ca supply to the oceans could have been 14 to 140 times greater than the modern flux. Our estimate falls within this range. Note that, as pointed out in the original manuscript, Li isotopes are a more unequivocal proxy for chemical weathering than Ca isotopes (which are also be sensitive to the environment of carbonate formation; e.g. Fantle and Higgins, 2014).

Referee 1: I am not sure why new rivers would preferentially drain landscapes similar to modern high latitudes

Our response: Data show that to reproduce the Li isotope composition of oceans at the start of deglaciation, rivers would need to have the same Li isotope composition as

modern high latitude rivers. This does not imply that only high latitudes were drained, but that the average world river would have had a Li isotope composition similar to some of the rivers draining modern high latitude landscapes.

Referee 1: It is the lithology that drives d7Li – Kisakurek et al. Chem Geol 2004, for example.

Our response: We respectfully disagree. Lithology imparts a small range of $\delta^7 Li$ values compared to the effect of clay formation (Burton and Vigier, 2011). This is why Li isotopes are such a useful tool to study chemical weathering (the nature of the parent material doesn't matter so much).

Referee 1: The oceanic water cycle is assumed to be kept largely similar even during glaciation (L381). I am not sure this is a reasonable expectation.

Our response: Our discussion does not intend to evaluate whether this is a reasonable hypothesis. It is just an end-member model considered to account for a *Slushball* model (i.e. partial glaciation) that others have proposed as an alternative to Snowball Earth (Fairchild and Kennedy, 2007). We just show that even in the extreme scenario where the Slushball was very slushy (i.e. hydrological cycle similar to present), we obtain conclusions similar to when considering a completely glaciated Earth.

Referee 1: 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

Our response: We agree such high-resolution data are not available. However, considering that the *plumewater* model is one of the plausible models for cap carbonate formation, we thought it was worth testing.

Typos and minor issues kindly identified by Referee 1 could be easily dealt with in a revised manuscript.

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